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VOLUME 48 SPRING, 1974 NUMBER 1

THE ANTIGEN-ANTIBODY REACTION AND THE PHYSICS AND CHEMISTRY OF LIFE

PAULINE M. H. MAZUMDAR *

An antigen is any substance to which an animal can make antibodies. The idea of an animal responding to the invasion of its body by foreign substances, usually bacteria, by producing a substance which reacted with them and inactivated them in some way first appeared in the 1880s; the current concept is that animals can produce antibodies which react with almost anything—the substances produced by bacteria, the bacteria themselves, cells from other animals, serum proteins, plant products, carbohydrates, even substances not found in nature.

So when I read in a review written by E. P. Pick of Vienna in 1912, that only proteins could be antigens, I was surprised. His review, of about 200 pages,[1] with 23 pages of references would seem to be a *summa* of all that was known about antigens at that date. He says:

> There is one definite characteristic possessed by all antigens, and that is, that they are all colloids . . . the second, which is intimately connected with the first, is that all

---

* This paper is dedicated to Dr. Michael Heidelberger who helped me to understand the problems discussed in it.

[1] E. P. Pick, "Biochemie der Antigene, mit besonderer Berücksichtigung der chemischen Grundlagen der Antigenspezifizität," in *Handbuch der pathogenen Micro-organismen,* 2. Aufl., eds. W. Kolle and A. von Wassermann (Jena, 1912), pp. 685-868.

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> true antigens are proteins. All attempts to raise antibodies with non-proteins have so far failed, so that one is justified in making the statement, "Kein Antigen ohne Eiweiss" —no antigen without albumen.

Pick then goes on to review a number of papers whose authors have investigated this point, and he says that a non-protein may become attached to a protein and take on antigenic properties, and he uses this explanation to account for the findings in several of these papers, that a non-protein did in fact behave as an antigen. In this way he explains the results of Abel and Ford,[2] of Johns Hopkins, who seem in their paper to show beyond any (modern) doubt that the toxin they prepared from toadstools gathered in the woods around Baltimore was clearly identifiable as a carbohydrate, in fact even more exactly as a glycoside, and not a protein; and with which they were able to produce an antibody in rabbits with a titre of 1:5000. Ford says,[3]

> The demonstration by Dr. Abel and myself that the *Amanita* haemolysin for which we have repeatedly made an antihaemolytic serum, is a glycoside, is not without theoretical interest, both in regard to current opinion on the subject, and to the possibility of making antibodies to other poisons of a glycosidal nature.

This was written in 1908, four years before Pick's review. Pick's restriction of antigenic properties to proteins alone, is not, then, due to the rather uninteresting reason of lack of any other evidence. It is an attempt to interpret a body of evidence which could have been interpreted differently, if seen from a different theoretical point of view.

In fact at this time there were two possible theoretical approaches to the experimental material that was being produced, and they both started off from the assumption that everything of importance that happened in the living body depended upon the properties of proteins: this essay is an exegesis of the text, "Kein Antigen ohne Eiweiss." The two main strands of thought in physiology around 1900 which contributed to the interpretation of the reaction of antigen with antibody centered in the one case on the principles of organic chemistry, and in the other on the physical properties of living matter which were being investigated by the new methods of colloid chemistry. Of the two, the chemical school came to its fullest strength a little the earlier, and was later supplanted by the physical one; the two groups agreed on the central importance of the proteins in their schemes of thought.

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[2] J. J. Abel and W. W. Ford, "On the poisons of *Amanita phalloides*," *J. Biol. Chem.*, 1907, *2*: 273-288.

[3] W. W. Ford, "Pathology of *Amanita phalloides* intoxication," *J. Infect. Dis.*, 1908, *5*: 116-132.

From the point of view of 1900, the "chemical" line of thought could be traced back to Edward Pflüger's idea that the cell substance consisted of one enormous molecule of albumen on which chemical compounds formed and broke down as long as the cell substance lived. The physical school followed Thomas Graham who in 1861 distinguished between crystalloids, typical of the inorganic world, and colloids, whose peculiar physical properties and chemical inertia made them the elements of life; the most important colloids were the proteins, but they shared physical properties with the inorganic colloids, and the inorganic could be taken as an analogy of organic life.

The facts about the antigen-antibody reaction were rediscovered several times, each time with a different antigen and antibody. The first was that of Paul Ehrlich in 1897[4] on the neutralization of diphtheria toxin by antitoxin; the paradox he detected was called "das Ehrlich'sche Phänomen"; after him in 1900 Jules Bordet[5] at the Institute Pasteur showed the same effect with an antibody which haemolysed red cells; in 1902 J. Danysz[6] also at the Institute Pasteur found that the same peculiar relationship existed between a plant toxin, ricin, and its antitoxin, and this was called "Le phenomène de Danysz." Also in 1902 Philipp Eisenberg and Richard Volk[7] in Vienna showed the same effect using typhoid bacilli and a horse antityphoid serum.

The effect that they all discovered concerned the quantitative relationship between antigen and antibody in the reaction.

I

Ehrlich's work was a byproduct of the need for standardization of a diphtheria antiserum for clinical use. The clinical results of the use of the antiserum had been very variable and the therapeutic dose unpredictable from batch to batch, and in 1896 an Institute was started under Ehrlich to develop methods of testing and standardization. In 1897 Ehrlich published the paper in which he drew attention to the paradox

---

[4] P. Ehrlich, "Die Wertbemessung des Diphtherie-heilserums, und deren theoretische Grundlagen," *Klin. Jahrb.*, 1897, *6*: 299-326, and in *Collected Papers of Paul Ehrlich,* eds. F. Himmelweit, M. Marquardt and Sir Henry Dale, 3 vols. (London-New York: Pergamon, 1956-60); vol. 2, pp. 86-106.

[5] J. Bordet, "Les sérums hémolytiques, leurs anticorps et les théories des sérums cytolytiques," *Ann. de l'Inst. Pasteur,* 1900, *14*: 257-296.

[6] J. Danysz, "Mélanges des toxines avec des antitoxines," *Ann. de l'Inst. Pasteur,* 1902, *16*: 331-345.

[7] P. Eisenberg and R. Volk, "Untersuchungen über die Agglutination," *Ztschr. f. Hyg.*, 1902, *40*: 155-195.

Table 1.

Ehrlich's Biological Standardization of Diphtheria Toxin and Antitoxin

TOXIN: 1 lethal dose = minimum dose necessary to kill all 250 G guinea pigs within 4 days

ANTITOXIN: 1 unit = minimum dose necessary to neutralize 100 L.D. of original toxin

L_0 dose of new toxin = number of L.D. neutralized by 1 unit of original antitoxin

L_+ dose of new toxin = number of L.D. just *not* neutralized by 1 unit of original antitoxin

$L_+ - L_0$ = 1 L.D. (theoretically, but in practice was always more)

of the antigen-antibody reaction[8] (Table 1). In this, the first biological standardization, he begins by defining units which could be measured *in vivo* using an original standard batch of toxin and antitoxin, and then quantitates the new toxins and antitoxins relative to the old. He defines the L_0 dose of the new toxin as the number of lethal doses neutralized by one unit of original antiserum; and the L_+ dose, as the number of lethal doses just not neutralized. There should have been a difference of one lethal dose between L_0 and L_+, but in practice, there was always more. He quotes eleven different assays, carried out on toxins from various different laboratories, including one from Dr. Bulloch in London; the differences varied between 17 and 22 lethal doses, and the L_0 doses, instead of being around 100 L.D., varied between 27 and 109.

Ehrlich explained these difficulties in the following way.[9] The determination of the L_0 dose of a toxin was quite simple and followed the laws of definite proportions. If one-tenth of a unit of antiserum neutralized a certain quantity of toxin, a whole unit neutralized ten times as much. But if different toxins were compared with each other, a lethal dose of one did not react with the same amount of antiserum as did a lethal dose of another; he suggested that combining power and toxicity were independent properties, and that as a toxin solution aged, it lost its toxicity, but kept its binding power unchanged. The L_0 dose, a measure of binding, remained unchanged, but the L_+ dose, a measure of toxicity, varied, becoming larger and larger as toxicity declined. He wrote:[10]

---

[8] Ehrlich (n. 4 above).

[9] P. Ehrlich, "Observations on the constitution of diphtheria toxin," *Trans. Jenner Inst. for Prevent. Med.* (1898) and in *Coll. Papers,* 2, 134-142.

[10] P. Ehrlich, "Ueber die Giftcomponenten des Diphtherie-toxins," *Berl. klin. Wschr.,* 1903, *40*: 793-797, 825-829, 848-851, and *Coll. Papers,* 2, 347-367.

> This phenomenon is most easily explained by assuming that the toxin molecule has two functioning groups. The one, the haptophore group, binds the antitoxin, and functions in the body to anchor the toxin in the tissues; it is quite stable; the other, the toxophore group, is relatively easy to break down, and mediates the toxicity.

This idea of a molecule with two or more separate functional groups occurs again and again in Ehrlich's explanations of things. His biographer, Miss Marquardt, calls it a red thread,[11] running through his work. It appears in his "Habilitationsschrift" of 1885 in a quotation from Hoppe-Seyler about dyes:[12]

> . . . light emission and absorption are not a function of the whole molecule . . . the light absorption of indigo is unchanged if it is converted into indigo-blue-sulphonic acid; we can therefore conclude that the atom group which absorbs the yellow light so strongly and produces the blue colour of indigo, is not the same as unites with the sulphonic acid, but another. . . .

In his thesis, Ehrlich suggests that the uptake of oxygen by the cell is probably by means of groups of this kind, independent of those which are also part of the same giant molecule, but serve other functions of the cell. These independent groups or side chains underwent reactions and formed compounds of the kind known in organic chemistry. He says:[13]

> Anybody who considers the diverse activities which are carried out by each individual cell, cannot but agree with Pflüger's view that living protoplasm must represent a giant molecule. Everything—specific vital activity, reproduction, assimilation, growth, multiplication, perception, thought, will—is the work of the cell substance . . . the expression of a certain chemical organization. . . .

In a paper which appeared in 1901[14] he compared an antibody to diazobenzaldehyde, a substance with two reactive groups attached to a central benzene ring (Fig. 1). Each of these groups can form an independent series of compounds; to make the analogy still closer, he says, certain constituents of the living cells may unite with the diazo-group, which then represents the haptophore, leaving the aldehyde group free to combine with other, perhaps toxic substances, which thus introduced damage the living cell. Here he is using the analogy to account for the damage done by an antibody to the cell, for example a bacterium to which it attaches.

---

[11] M. Marquardt, *Paul Ehrlich* (New York: Schuman, 1951), p. 18.

[12] P. Ehrlich, *Das Sauerstoffbedürfniss des Organismus.* Habilititationsschrift (Berlin, 1885); and in *Coll. Papers,* 1, 364-432.

[13] *Ibid.,* p. 436.

[14] P. Ehrlich and J. Morgenroth, "Ueber Haemolysine: fünfte Mitteilung," *Berl. klin. Wschr.,* 1901, *38*: 251-257.

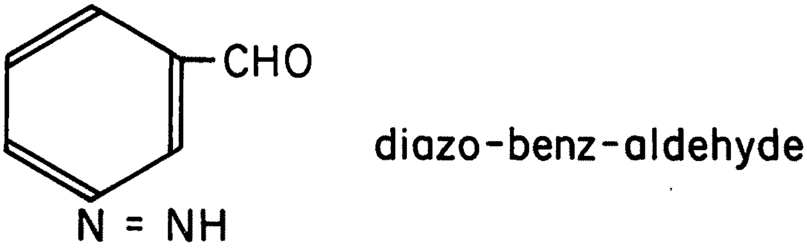

Fig. 1.

Ehrlich had a strong visual imagination; his pictures of antibodies attaching themselves to antigens are very memorable; but in this case it rather ran away with him, and he dived into what he called elsewhere "his tangential chemistry."

He "gained the impression," he said[15] that in a pure toxin, 1 unit of antiserum would neutralize 200 lethal doses, and that all lesser figures showed the effects of degeneration. The "discovery" of this number suggested a "spectrum" for diphtheria toxin divided into 200 segments (Fig. 2).

Each toxic broth, the liquid medium in which the bacteria had been grown, contained a number of different toxins, and their degeneration products, called toxoids, of varying affinity, or avidity, for the antitoxin. In addition, there was also a toxone which was responsible not for the acute symptoms of poisoning but for the skin swelling at the site of injection.

The amount of toxone could be assessed on a fresh, undegenerated broth by the difference between L_0 and L_+: if L_+ was twice L_0 the toxic mixture was half toxin and half toxone; as time passed, the difference increased as more toxin broke down; some of these non-toxic breakdown products had more affinity (or avidity) for the antitoxin and so absorbed it first, causing an initial absorption of antitoxin with no reduction in the toxicity of the mixture.

If one draws a set of curves using one of Ehrlich's own quantitative examples,[16] it is easier for us to see what he meant; he himself never drew curves; he did not like what he called a "mathematical treatment," and felt that this was not biology. Others among his contemporaries did it though, so it is not unhistoric (Fig. 3).

The upper curve starts with a shoulder where successive quantities of

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[15] Ehrlich (n. 10 above).

[16] Ehrlich (n. 9 above).

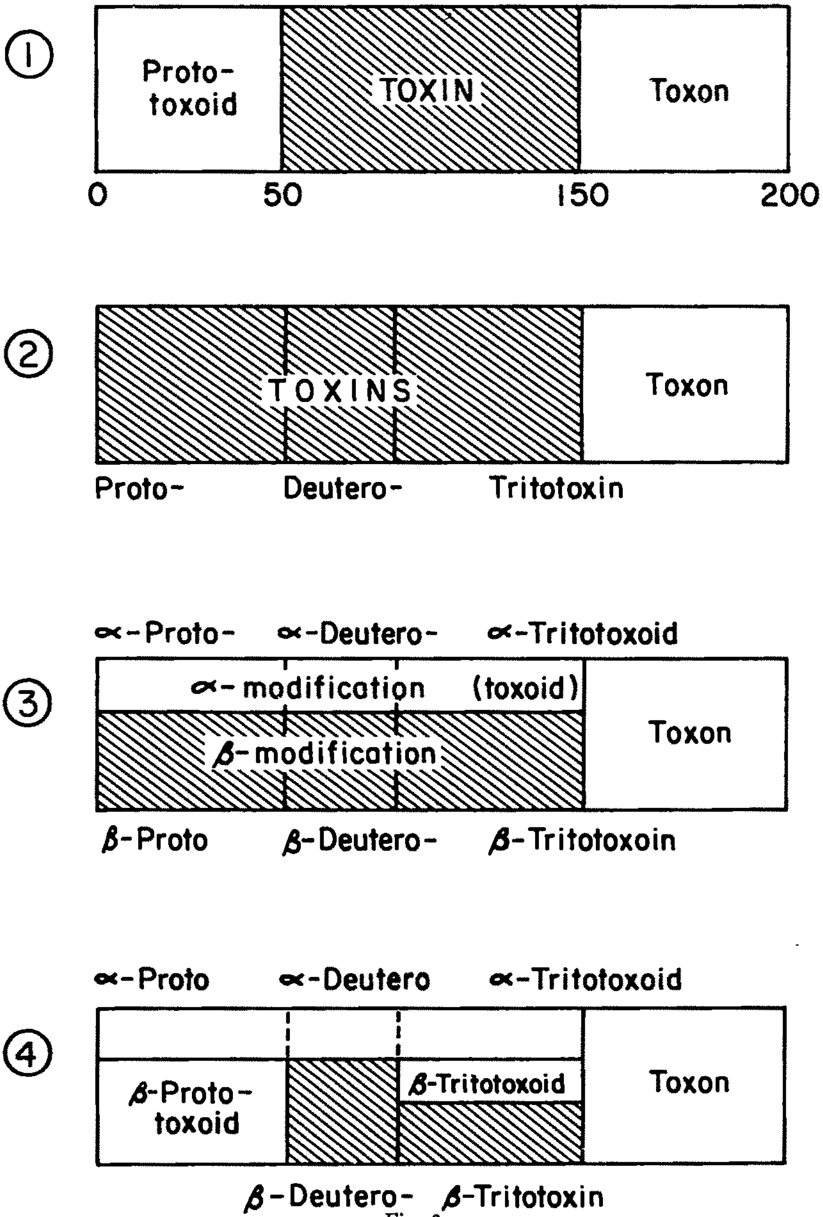

Fig. 2.

An Ehrlich "Giftspectrumschema," showing progressive modification and loss of toxicity on passage of time. Different modifications have different affinity for antibody. Toxic area shaded, non-toxic plain.
(From L. Aschoff, *"Ehrlich's Seitenkettentheorie und ihre Anwendung auf die künstlichen Immunisierungsprozesse,"* Jena, 1902, p. 78.)

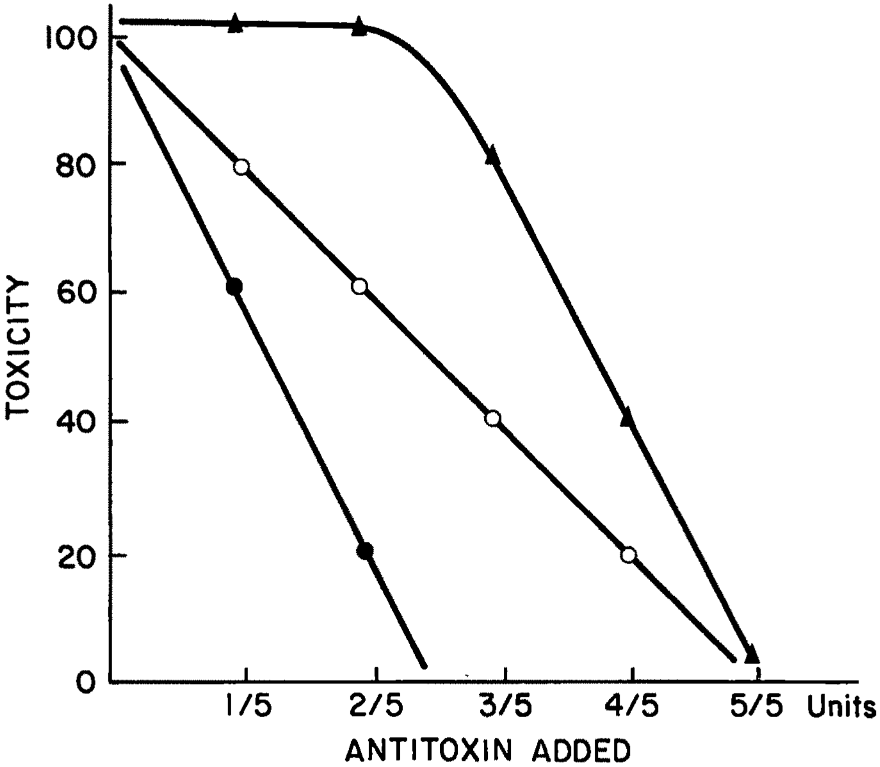

Fig. 3.

"Mathematical" Treatment of Ehrlich's Neutralization of Toxin by Antitoxin. Curves for three different toxic broths with the same L_0 dose of 100 L.D. Plot of toxicity remaining in the mixture (ordinates) against units of antiserum added (abscissae). (Plotted from examples in P. Ehrlich, "Observations on the constitution of diphtheria toxin," *Trans. Jenner Inst. for Prevent. Med.*, 1899.)

antiserum do not affect the toxicity much; he ascribes this to consumption of the antiserum by non-toxic components of high avidity, probably a prototoxoid. After this the slope changes and the toxicity drops proportionally to antiserum added. Here he says that mixtures of toxin and toxoid with the same avidity for antiserum are being neutralized. In the other two curves, the mixtures contain different proportions of toxic and non-toxic components, the steepness of the lowest one suggesting that it contains very little non-toxic material.

Ehrlich's assumption that antigen and antibody formed a firmly linked complex like the compounds of organic chemistry was criticised by Svante Arrhenius from the standpoint of his own work on electrolytic dissociation. He made the same assumption as Ehrlich, that the law of

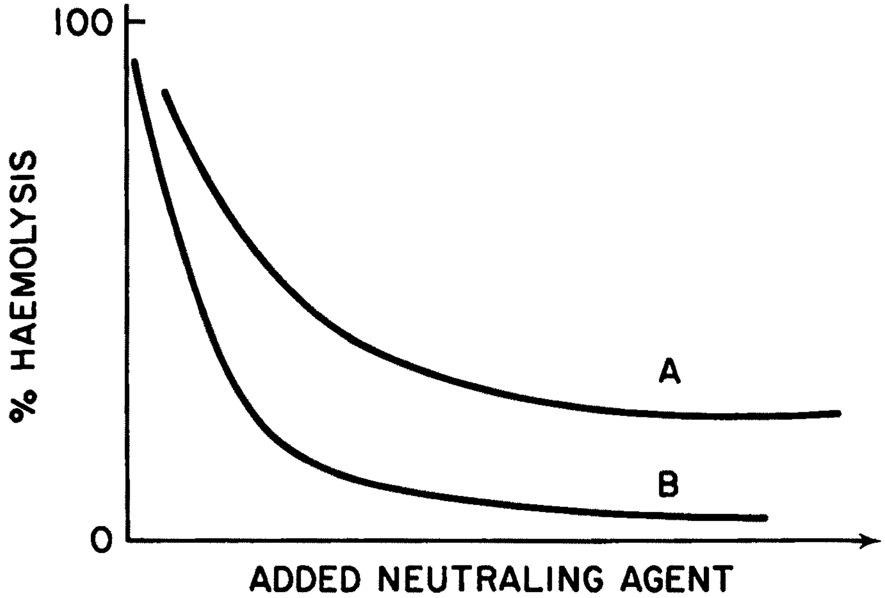

Fig. 4.

Arrhenius' Model.

Curves for the neutralization of ammonia by boracic acid, and of tetanus toxin by antitoxin, using haemolysis of red cells as indicator in both bases.

A. $\mathrm{AmOH} + \mathrm{HBor} \rightleftharpoons \mathrm{Am}^{+} + \mathrm{Bor}^{-} + \mathrm{H_2O}$
$(\mathrm{AmOH})(\mathrm{HBor}) = \mathrm{K}\,(\mathrm{Am}^{+})(\mathrm{Bor}^{-})$

B. $\text{Toxin} + \text{Antitoxin} \rightleftharpoons \text{Bound toxin}$
$(\text{Toxin})(\text{Antitoxin}) = \mathrm{K}'\,(\text{Bound toxin}^{2})$

(Adapted from Sv. Arrhenius, *Immunochemistry, the Application of the Principles of Physical Chemistry to the Study of the Biological Antibodies* [New York, 1907], pp. 174-180.)

definite proportions applied to the reaction, but he suggested that the reaction did not go to completion, forming a real chemical compound, but instead formed a reversible equilibrium between the reactants, as in the reaction of a weak base with a weak acid (Fig. 4).

In a lecture at the University of California in 1904[17] Arrhenius said,

> they were unable to explain all the phenomena indicating the existence of chemical equilibria. . . . To explain some of these . . . Ehrlich and his school invented the artificial hypothesis that these poisons consist really of a great number of different poisons and innocuous substances which combine with antitoxin. . . . Nearly every new phenomenon led him and his school to invoke the presence of a new substance;

---

[17] Sv. Arrhenius, *Immunochemistry: the Application of the Principles of Physical Chemistry to the Study of Biological Antibodies* (New York, 1907), p. 30.

> owing to this circumstance the theory of Ehrlich has to a great extent lost its credibility.

Arrhenius' criticism came too late to have much influence on Ehrlich, who had by 1904 almost ceased to be interested in immunology and had moved on to apply his side-chain theory to chemotherapy instead. His colleague, or rather helper, Madsen, had gone to work with Arrhenius, after making some hundreds of "Gift-spectrums" in the Ehrlich manner, and was now making equilibrium curves for Arrhenius. Ehrlich says rather plaintively somewhere that he is glad Madsen has gone to work with such a famous man.[18]

Speculative organic chemistry of Ehrlich's kind was still alive in other fields of physiology. Pflüger by this time was no longer active: he died in 1910 at the age of 80; but his thinking had been taken up by Max Verworn, who repeated the "living albumen" idea in his *Allgemeine Physiologie*[19] (subtitled "Ein Grundriss der Lehre vom Leben"—"a basic textbook on life") which went through six editions between 1894 and 1916, and whose conception of the organic compounds formed by the labile "living albumen" molecule are close to those of both Pflüger on the one hand, and Ehrlich on the other.

Ehrlich in fact found them annoyingly close. In an article in Nothnagel's *Pathologie* in 1901,[20] Ehrlich says,

> I feel justified in believing that the study of receptors has opened up a new and important field of biological investigation. . . . I should like to quote the following paragraph from Verworn. . . .

and he quotes one of Verworn's descriptions of the vital functioning of protoplasm as it depends upon the constant formation and decomposition of organic compounds of very labile "living albumen" molecule, which he calls "Biogen" for short, and which changes with the death of the protoplasm into a stable modification, a "dead albumen." Ehrlich quotes this with approval; in fact, he claims priority for it:

> It seems to have entirely escaped Verworn that I had expressed entirely analogous views in much greater detail twelve years previously. . . .

He means, in his thesis, in the part already quoted.

---

[18] Ehrlich (n. 10 above).

[19] M. Verworn, *Allgemeine Physiologie: ein Grundriss der Lehre vom Leben,* 1. Aufl. (Jena, 1894).

[20] P. Ehrlich "Ueber den Receptorenapparat der rothen Blutkörperchen," in *Nothnagel's specielle Pathologie und Therapie,* vol. 8 (Vienna, 1901), and in *Coll. Papers,* 2, 316-323.

II.

The second point of view from which the antigen-antibody reaction could be considered, was that of colloid chemistry. The colloid model of the reaction underlies almost everything published on it between 1900 and about 1920.

The peculiarities of colloids, and their relation to living matter, were described in the immensely influential words of Thomas Graham, which appeared in 1861.[21] After dividing substances into two classes, colloids and crystalloids, on the basis of their diffusibility and ability to crystallize, he says,

> Low diffusibility is not the only property which the bodies [sc., the colloids] . . . possess in common. They are distinguished by the gelatinous character of their hydrates. . . . They appear singularly inert in the capacity of acids and bases, and in all the ordinary chemical reactions. But, on the other hand, their peculiar physical aggregation with the chemical indifference referred to, appears to be required in substances that can intervene in the organic processes of life. The plastic elements of the animal body are found in this class; . . . another and eminently characteristic quality of colloids is their mutability. Their existence is a continued metastasis . . . The colloid is in fact a dynamical state of matter, the crystalloidal being the statical condition. The colloid possesses ENERGIA: it may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.

This mixture of mechanistic and near-Aristotelian ideas is very characteristic of the work of the colloid chemists. The important constituents of living matter were colloids, and they were proteins, and so would not undergo ordinary chemical reactions, conforming with the law of definite proportions as in Ehrlich's model, or with the law of mass action, as in Arrhenius. Instead, they formed complexes by adsorption, a process in which one substance forms a film over the surface of another, in indefinite proportions. Adsorption was thought of as a purely physical phenomenon, dependent on the surface area of the adsorbent and the surface tension of the solution. Colloids were said not to form true solutions but to consist of large particles, sometimes large enough to see, forming a semisolid structure with a large surface area. The large molecules of proteins formed this kind of chemically inert but surface-active structure; the giant molecule idea fits in very well with this conception, but in this case it is thought to form adsorption complexes rather than organic compounds as Ehrlich and Pflüger had suggested.

Colloids also have the property of precipitating out of solution when the physical properties of the solution, such as the ionic content, are changed; a negatively charged colloid would be precipitated by addition of positively charged ions. The resemblance of this to the agglutination of bacteria by an antibody was first noticed by Bordet in 1896.[22] He wrote,

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[21] T. Graham, "Liquid diffusion applied to analysis," *Phil. Transl*, 1861, *151*: 183-199.

> These clumping phenomena seem due . . . to some phenomenon of molecular physics . . . It is probable that the serum acts on the bacteria by changing the relations of molecular attraction between the bacteria and the surrounding fluid.

In 1900 Bordet described[23] the experiment with a haemolytic antiserum in which he discovered the variable proportions in which antibody unites with antigen, as Ehrich had already done for diphtheria toxin and antitoxin in 1897. Bordet found when he added a suspension of red cells to his antiserum, that if he added the blood all at once, twice as much could be lysed by the antibody as if he added it in divided doses. He concluded that the first cells added in the divided doses absorbed more antibody than the minimum necessary to lyse them, leaving none over for the later fractions added. He compared these variable proportions in which the cells and antibody were able to unite to the uptake of dye by paper, and demonstrated it by what I think was a "thought experiment." He soaked a piece of filter paper in a weak dye solution, taking up all the dye, and colouring the paper; he then cut a similar piece of filter paper in several pieces and dipped them one by one in a similar dye solution; all the colour was taken up by the first two or three, leaving the rest colourless.

When Philipp Eisenberg and Richard Volk had published their discovery of the same variable proportions in the union of antityphoid antibody with typhoid bacteria, Bordet commented,[24]

> It would seem legitimate then to assume that the law of fixed proportions is not applicable to the absorption by cells or bacteria of the active principles of specific sera. . . . The conditions are very unlike those met with in ordinary chemistry which depend on equations and equivalents.

He says that it is no more possible to say that antigen and antibody combine in definite proportions, than that paint combines in definite proportions with a wall.

Following the lead of Bordet, Karl Landsteiner and his coworker

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[22] J. Bordet, "Sur le mode d'action des sérums préventifs," *Ann. de l'Inst. Pasteur,* 1896, *10*: 193-219.

[23] Bordet (n. 5 above).

[24] J. Bordet, "Sur le mode d'action des antitoxines sur les toxines," *Ann. de l'Inst. Pasteur,* 1903, *17*: 161-186.

N. Jagič[25] also commented on Eisenberg and Volk's published results; the most interesting part of their paper is the speculative section with which it ends. The dyeing analogy, Landsteiner says, comes up in every discussion of this problem, but it does not answer the question of whether the process is chemical or physical, as in dyeing it is not known with certainty which it is; there is evidence for both views, and neither excludes the other. He then quotes a series of papers by the colloid chemist van Bemmelen in which the phenomenon called "adhesion" or "absorption" is discussed, and the uptake of a salt by a colloid shown to be a function of concentration and temperature. The reactions of antibodies, Landsteiner says, seem to be chemical in their individuality but physical in their quantitative aspects:

> It appears that in the living organism these peculiar reactions play a particularly important role, and that the combinations of colloids, which form the larger part of the living substance often follow no fixed proportions . . . on these grounds then, the protoplasmic substance cannot be seen as a definite compound of which the constitution can be shown by the valency theory. The other viewpoint seems more likely, in which this substance is seen as a system of which the parts have variable, not fixed relationships to each other. . . .

The chemist Wilhelm Biltz in discussing this article in 1904[26] wrote:

> Landsteiner . . . [was] the first to recognize the far-reaching analogy between immunity and related phenomena and the adsorption process in inorganic colloids on the other . . . Lively interest in this relationship was later shown by [other authors]; among these Landsteiner expressed himself most precisely, and with his opinions created a new line of research.

Biltz went on to point out the analogy between the precipitation of a colloid by another colloid of opposite charge, and the precipitation of antigen by antibody; he illustrates Landsteiner's comparison by Eisenberg and Volk's results with those of van Bemmelen by plotting the two sets of curves side by side. The curves he says, conform with "mehr oder weniger Glück" to the formula:

$$K = \frac{\text{Concentration of absorbed material}^{n}}{\text{Concentration of free material}}$$

which could represent either an adsorption, as in colloid chemistry, or a differential solubility as suggested by Arrhenius; but Biltz himself doubted

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[25] K. Landsteiner and J. Jagič, "Ueber die Verbindungen und der Enstehung vom Immunkörpern," *Münch. med. Wschr.*, 1903, *50*: 746-768.

[26] W. Biltz, "Ein Versuch zur Deutung der Agglutinationsvorgänge," *Ztschr, f. phys. Chemie*, 1904, *48*: 615-623.

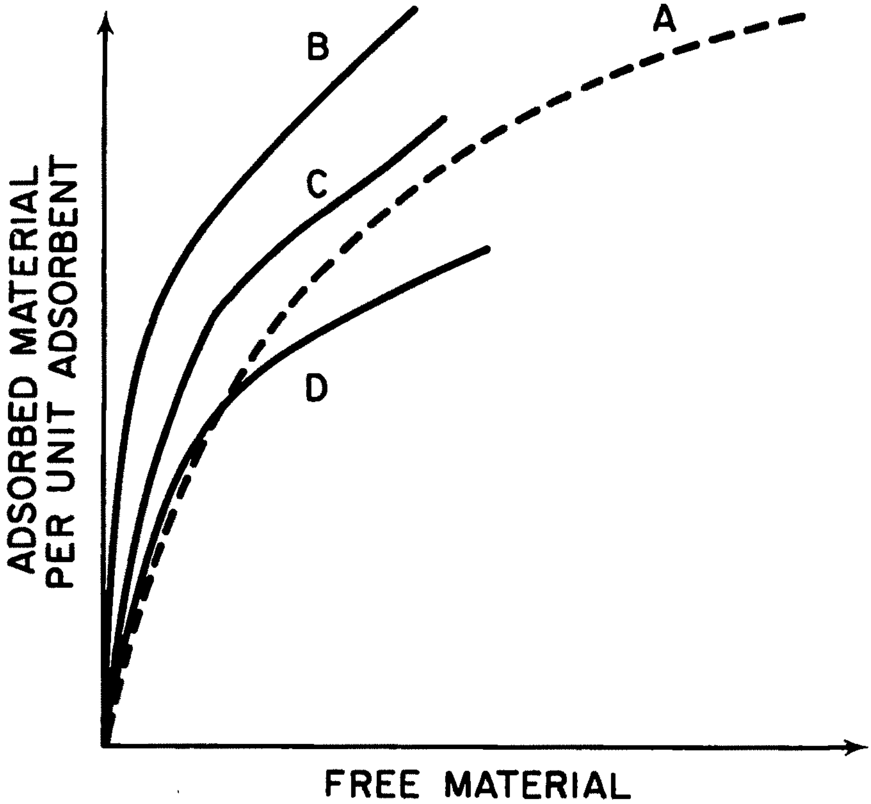

Fig. 5.

Eisenberg & Volk's Results.

Plotted as a Freundlich "Adsorption isotherm" in which

$$\frac{\text{Adsorbed}}{(\text{Free})^{1/n}} = k$$

Curve A. Eisenberg & Volk: Antityphoid antibody adsorbed by typhoid bacilli (ordinates) vs. antibody free in solution (abscissae).

Curves B, C, & D:
Freundlich: Adsorption of succinic acid, propionic acid & acetic acid respectively by charcoal.

(Adapted from P. Eisenberg & R. Volk, "Untersuchungen über die Agglutination," *Ztschr. f. Hyg.*, 1902, *40*: 155-195, and H. Freundlich, *Kapillärchemie* 2. Aufl., 1922, p. 195.)

whether the gas laws, and therefore the solubility interpretation, could apply to colloids.

In Fig. 5 I have also replotted Eisenberg and Volk's results, and compared their curve with those typical of colloid adsorption, produced by another colloid chemist, Freundlich.[27] Eisenberg and Volk's tables were recalculated and plotted in various ways by many people at this time, so it is not unhistorical to do it.

The antigen-antibody reaction was not the only biological process to be related to colloid chemistry. The Viennese chemist Wolfgang Pauli,[28] for example, suggested that the honeycomb structure of colloid protoplasm accounted for the ability of the cell to have antagonistic chemical reactions, such as oxidation and reduction, going on together. This had been explained by organic chemists like Ehrlich as occurring on different side chains of a molecule.

Landsteiner refers to this paper of Pauli's in his article on the colloid interpretation of the antigen-antibody reaction. The paper was reprinted several times, and appeared in translation in a little collection of Pauli's articles in 1907; [29] in the "author's preface" Pauli says,

> The development of the guiding thought common to all of them [sc., his articles] stands out quite clearly. Its foundation is the extensive parallelism between the laws which govern changes in the colloidal state *in vitro,* and in the living organism.

The importance which Pauli gave this parallelism was pointed out in another of his papers, an address [30] given in 1899 before the k.k. Gesellschaft der Aerzte in Wien of which Landsteiner was also a member. He said,

> Unquestionably they [sc., the methods of colloid chemistry] enlarge that territory which the organic and the inorganic world have in common. The last barriers between the two cannot as yet be broken down . . . there always remains an unsolved portion, the kernel, as it were, of vital phenomena.
>
> The cause of the final failure of the new instruments can rest only in their origin. They have all been evolved from the study of lifeless matter. For a complete understanding of the living, the words of a great physiologist will probably hold: "Life can perhaps only be understood through life itself."

I thought at first that this "great physiologist" must be Verworn; Verworn says somewhere,[31]

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[27] H. Freundlich, *Colloid and Capillary Chemistry.* Trans. from the 3rd German ed. by H. S. Hatfield (New York, 1926), p. 195.

[28] Wolfgang Pauli, "Der kolloidaler Zustand und die Vorgänge der lebendiger Substanz," *Naturwiss. Rundschau,* 1902, *17:* 312.

[29] Wolfgang Pauli, *Physical Chemistry in the Service of Medicine: Seven Addresses.* Trans. M. H. Fischer (London and New York, 1907), p. iii.

[30] Wolfgang Pauli, *Ueber physikalisch-chemische Methoden und Probleme in der Medizin* (Vienna, 1900).

[31] M. Verworn, *Allgemeine Physiologies,* 5. Aufl. (Jena, 1909), p. 577.

> in the living cell substance, by the side of the known proteins, which are also found in dead cell-substance, are certain other proteins or compounds of proteins, which are only present in the living, and with their break down bring life to an end. . . .

But in fact it was not Verworn, it was from an essay of Ewald Hering of Leipzig, whose main work was on the nervous system, that the quotation comes.[32] The context of the quotation is,

> To whatever point the physical or chemical investigation of the animal organism has penetrated, it has sooner or later came upon the mysterious action of the living substance . . . can it surprise us that today the old fallacious doctrine of vital force which we imagined had been definitely vanquished should again rear its head under new names? Let us cease considering physiology merely as a sort of applied physics and chemistry . . . [it is] an idle task to seek an exhaustive explanation of the living from the dead. Life can be fully understood only through life. . . .

The view then which Pauli, the colloid chemist, is quoting with approval is a kind of a vitalism; although Hering calls vitalism "the old fallacious doctrine," he certainly seems to be postulating a force apart from what is known of applied physics and chemistry. Perhaps he may be included under "mechanistic vitalism," one of the three kinds of vitalism defined by Max Verworn:[33]

> Mechanistic vitalism is the view that . . . the life processes rest basically on physicochemical factors but the conditions in the living organism are so complex that they have up to now not been elucidated. These complex conditions which are peculiar to living organisms in contrast to inorganic nature one, may for the present call the life force.

Verworn's own suggestion for the "complex conditions peculiar to living organisms" is that[34] "The life-process rests upon the metabolism of albumen." Pauli's suggestion is the physical chemistry of the colloidal proteins of the protoplasm. Its unwillingness to submit to the law of definite proportions, or the law of mass action, or the gas laws, suggested that the life-processes might belong to a different universe, whose laws might be those of colloidal chemistry. In spite of this, Verworn, like Hering, speaks of "the vitalistic error" and says that,[35] "in always new dress, the old spectre pops up unexpectedly here and there."

An interesting insight into how these ideas were received by the scientific public can be obtained from book reviews in the contemporary journals. In May 1905, for example, Richard Ewald reviewed Verworn's *Die Biogen Hypothese* of 1903 for the *Berliner klinische Wochenschrift*. He has nothing critical to say of it at all; he simply explains the "living albumen" hypothesis, and says that the book is full of good ideas, and should stimulate many more.[36]

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[32] K. E. K. Hering, "Zur Theorie der Nerventätigkeit." Academic discourse delivered before the Univ. of Leipzig (1898). Trans. in *Lectures on the Specific Energies of the Nervous System* (Chicago, 1913).

[33] Verworn (n. 26 above), p. 50.

[34] *Ibid.*, p. 141.

[35] M. Verworn, *Die Bewegung der lebendigen Substanz: ein vergleichend-physiologische Untersuchung der Contractions-erscheinungen* (Jena, 1892).

Pauli, in reviewing a book for the *Wiener klinische Wochenschrift* for 1899 says[37]

> Verworn, with his identification of cell physiology with general physiology has aroused powerful and legitimate opposition.

which was exemplified by the author Pauli was reviewing, (who gave more importance to the cell nucleus); he does not suggest that the "living albumen" hypothesis was rousing any opposition.

On the other hand, a review in the *Berliner klinische Wochenschrift* in 1905 of a book on vitalism[38] says,

> The reviewer does not think it useful for medicine to lose itself in this kind of philosophical speculation. It is to be feared that this may lead us back to the age of the old and exceedingly dangerous *Naturphilosophie*.

This would seem to be a shaft directed at what Verworn calls "Vitalismus 'der alten Schrot und Korn' " ("22 ct. chip off the old block") by which he meant the Aristotelian "entelechy" of Driesch.

Although Landsteiner had undergone a kind of colloid revelation in 1903, he did not stay very long in the classical colloid camp. By 1904 he and Jagič wrote,[39]

> It seems that (immune) agglutination and precipitation must depend on salt-linkages between the reacting substances, and this is another analogy with the dyeing process, as it is known that only those coloured susbtances which are either basic or acidic can be used as dyes.

They refer to the amphoteric properties of proteins, by which under different circumstances they can form salts with either acids or bases. This idea, that of the ionization of proteins themselves, was not accepted in classical colloid chemistry; changes in charge of the protein were put down to absorbtion of ions from the medium, and not to changes in ionization of the protein itself, until the ionization interpretation was pressed further by Jacques Loeb in 1922.

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[36] J. R. Ewald, review of Max Verworn, "Die Biogen Hypothese," *Berl. klin. Wschr.*, 1905, *42*: 577-578.

[37] Wolfgang Pauli, review of F. Schenk, "Physiologische Charakteristic der Zelle," *Wiener klin. Wschr*, 1899, *12*: 1072-1073.

[38] Von Hansemann, review of Karl Camillo Schneider, "Vitalismus: elementäre Lebensfunctionen," *Berl. klin. Wschr.*, 1905, *42*: 403.

[39] K. Landsteiner and N. Jagič, "Ueber Reactionen anorganischer Kolloide und Immunkörperreactionen," *Münch. med. Wschr.*, 1904, *51*: 1185-1180.

In 1905, Landsteiner and Reich wrote,[40]

> We think, that the reactions of antibodies are inseparable from chemical processes, probably processes like the formation of salts . . . particularly since the proteins, because of their amphoteric nature, can react chemically, . . . and that the electrical charge of inorganic colloids is a result of their chemical, that is, acidic or basic, nature. We say more, that because of the lack of sharp dividing line between so-called physical and chemical processes, the discussion as to whether these reactions are one or the other is meaningless . . . the absorption of agglutinins has nothing in particular to do with the colloid character of the substances.

After this, Landsteiner entered upon a course in which he systematically investigated the influence on antigenic specificity of first the acidic and basic groups of the protein molecule, and later other groups, less obviously related to an electrical charge hypothesis of the antigen-antibody reaction. This would seem to be veering round to a chemical "side-chain" hypothesis again, except that Landsteiner postulated a much looser kind of linkage for the complexes formed, at first an ionizable, rather than the covalent organic chemical linkage of Ehrlich, and later an even less well defined type of linkage.

At the time though, nobody thought to refer this to Ehrlich; he was firmly out of fashion, and the colloid hypothesis, in spite of Landsteiner's defection, was firmly in. Even those workers who were investigating the chemical structure of antigens tended to refer to Bordet's dyeing analogy, rather than the side-chain theory.

The parallel fading of the influence of Verworn can also be documented by the attention paid to his "living albumen" hypothesis in successive editions of Rudolf Höber's *Physikalische Chemie der Zelle und der Gewebe*.[41] Höber was antagonistic to Verworn from the start, but in his second edition of 1906 he gives a long perfectly straight account of the "Biogen Hypothesis," then he says that we have no grounds for belief in any such complicated labile molecule as the seat of life. The reasons for the lability of albumen are quite different. In his fourth edition of 1914, he gives the problem much less attention; he says,

> There no more exists a dead, stable albumen and a living labile one than there exist dead and living sugar and dead and living fat.

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[40] K. Landsteiner and M. Reich, "Ueber die Verbindungen der Immunkörper," *Centralbl. für Bakt.* (Orig.), 1905, *39*: 83-93.

In the fifth edition of 1922, and thereafter, it is not mentioned any more.

Although the giant molecule hypothesis had lost support, the colloid hypothesis of the antigen-antibody reaction was still very well supported; in fact it was completely taken for granted by almost every writer on the subject. Not only E. P. Pick, the author of the review of 1912, with which I started this paper, accepted it totally, but also Bordet, in his *Traité de l'immunité* of 1920, assumed that any substance capable of functioning as an antigen would be both a colloid and an albuminoid, and that the reactions of antibodies with antigens depended on forces determining the reactions of colloids.[42] By this time it was a dogma of immunology to be stated in the first line or two of any discussion of the subject, and then assumed to underlie all the rest.

The scientific public, too, knew which side it was on; a similar total acceptance of the colloid theory is demonstrated in a correspondance published by the *British Medical Journal* in 1917. This manages to combine Ehrlich and Verworn and all other *sales Boches* in one general condemnation; the writer says:[43]

> I wish to ask whether it is not time to simplify the theory of immunity. Much modern work seems intended to support "orthodox Ehrlichism" though by multiplication of subhypotheses its confusion grows worse confounded. Moreover, the "giant molecule" side-chain theory is wholly discredited . . . the "biogen" can no longer be considered a chemical entity. . . . Nevertheless, the structure built on the foundation of the giant molecule still grows. . . .

The following week he was answered by another correspondent, who wrote,[44]

> Progress in our knowledge of the biochemistry of immunity will be delayed so long as the modes of thought connoted by the series of *ad-hoc* terms first introduced by Ehrlich and since reduplicated by others are allowed to dominate the clear view that the laws of immunity are the laws of reaction by adsorption in colloidal solution. There is nothing new whatever nor ever was, in the side-chain theory except the fantastic terms. . . . The pioneer work on immunity which has carried us so far forward . . . was that of Bordet, Gengou and others of the French school; it was burdened from Germany with the intolerable weight of a useless philosophy of jangling terms for a type of reaction well known in colloidal chemistry. . . .

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[41] R. Höber, *Physikalische Chemie der Zelle und der Gewebe* (Leipzig: 2. Aufl. 1906; 4. Aufl., 1914; 5. Aufl., 1922; and 6. Aufl., 1926).

[42] J. Bordet, *Traité de l'immunité* (Paris, 1920), p. 485.

[43] M. Roberts, "The theory of immunity," a letter to the editor. *Brit. Med. J.*, 9 Dec. 1917, 777-778.

[44] B. Moore, "The theory of immunity," a letter to the editor. *Brit. Med. J.*, 22 Dec., 1917, 842-843.

Two incidents began the end of colloid chemistry. One of these was the publication by Jacques Loeb of the Rockefeller Institute of his book *Proteins and the Theory of Colloidal Behaviour* in 1922.[45] In this he demonstrated that proteins do in fact undergo chemical reactions, and that adsorption and precipitation depend not upon a different kind of colloid chemical laws, but on ionization and the mass-action equation; this is almost exactly what Landsteiner, and later Pauli, had suggested twenty years before. However, in this case, Loeb managed to destroy with it not only the application of colloidal analogies to the antigen-antibody reaction, but the entire assumption that proteins needed a special set of laws to account for their reactions. Curiously, Loeb had produced in 1912 a very passionate declaration in favour of a mechanistic conception of life[46] with examples drawn from plant tropisms and from parthenogenesis in eggs, induced by inorganic means; but none of his examples, then, were as good an argument against vitalism as this one turned out to be. Loeb's demonstration had shown that proteins underwent chemical reactions and needed no special laws to describe them. A year later, in 1923, Michael Heidelberger and O. T. Avery,[47] also of the Rockefeller Institute, showed that the antigenic substance of the *Pneumococci* was a carbohydrate, not a protein, and that each type of *Pneumococcus* produced a different carbohydrate; they differed from each other in the sugars involved in the carbohydrate chain, and in the way they were linked. Heidelberger and Avery themselves could hardly believe it. They wrote,

> Whether the specific substance is precipitated by immune serum, by uranyl nitrate, by basic lead acetate, or by safranine, precipitants of three totally unrelated types; whether the method of purification is based on simple fractional precipitatoin or on adsorption, the product recovered is always essentially the same, and always a carbohydrate.

The effect of these two incidents was to change the whole outlook of immunology; at first, one finds Landsteiner, for example, referring to Heidelberger's carbohydrate as a "colloidal" carbohydrate: it was actually a brown sticky substance, which could be called colloid, or at least gluey. It was also assumed that the carbohydrates were not antigenic on their own, but should be attached to the bacteria for full effectiveness;

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[45] J. Loeb, *Proteins and the Theory of Colloidal Behaviour* (New York: McGraw-Hill, 1922).

[46] J. Loeb, "A mechanistic conception of life," *Popular Science Monthly*, 1912, *80*: 5-10.

[47] M. Heidelberger and O. T. Avery, "The soluble specific substance of Pneumococcus," 2nd paper. *J..Exper. Med.* 1923, *40*: 301-306.

that was true for some animals, but it was later found that in mice and human beings they would act alone; it was felt that proteins were at least *better* antigens than carbohydrates, but later even this distinction faded.

Finally the whole system linking colloids proteins and life simply disappeared completely from the biological sciences. Pick's confident assertion that only proteins could be antigens appears now to be not just incomplete, but incomprehensible.

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THE BACKGROUND TO ARTHUR HARDEN'S DISCOVERY OF COZYMASE

ROBERT E. KOHLER, JR.

Introduction

The discovery of cell-free fermentation in yeast juice by Eduard Buchner (1865-1917) in 1897 opened up to chemists and biologists a new world of biochemistry.[1] Buchner's new method of extraction, by grinding cells with sand and pressing out the cell juices through kieselguhr, provided for the first time a general method of obtaining the intracellular agents of vital chemistry. And Buchner's demonstration that the agent of fermentation was a true soluble enzyme, "zymase," provided biochemists with a paradigm of a complex physiological reaction that could be studied by the methods of physical chemistry *in vitro*. It was soon widely believed that most or all of cell chemistry was likewise due to enzymes, and the combination of this new faith in the essential simplicity of cell functions and the new *in vitro* system to explore led to the rapid development of the new field of biochemistry.[2]

The most influential biochemical event after zymase was unquestionably the discovery by Arthur Harden (1864-1942) in 1905 that zymase required both an organic cofactor, "cozymase," and also inorganic phosphate.[3] A few years later Harden and William Young (1878-1942) showed further that a glucose phosphate ester accumulated during fermentation *in vitro*.[4] These were both extraordinary and unprecedented results. It had been generally assumed that zymase was a simple enzyme, and the fact that at least three essential components were involved was a great surprise. Buchner himself was sceptical at first,[5] but the superb experiments of Harden and Young were unassailable, and Buchner soon accepted Harden's view, with some chagrin at having failed to perceive the clues himself. (He had been entangled in a rather premature and fruitless dispute over the chemical intermediates of fermentation.)

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[1] E. Buchner, "Gährung ohne Hefezellen," *Ber. deut. chem. Ges.* 1897, *30*: 117-124.

[2] R. E. Kohler, "The reception of Eduard Buchner's discovery of cell-free fermentation," *J. Hist. Biol.* 1972, *5*: 327-353.

[3] A. Harden and W. J. Young, "The alcoholic fermentation of yeast-juice," *J. Physiol.* 1905, *32*: i-iii.

[4] W. J. Young, "The organic phosphorus compound formed by yeast-juice from soluble phosphates," *Proc. Chem. Soc.* 1907, *23*: 65-66.

[5] E. Buchner and W. Antoni, "Existiert ein Coenzym für die Zymase?" *Zeits. f. physiol. Chem.* 1905, *46*: 136-154.

Harden's novel discoveries excited tremendous interest in the nature of cozymase and the role of phosphorylation in fermentation.[6] In the early 1920s Otto Meyerhof (1884-1951) recognized that the cozymase of yeast was identical with the cofactor required for muscle glycolysis, and Hans Euler (1873-1964) began to unravel the chemical structure of coenzyme. The role of phosphorylation became clear in the early 1930s when Gustav Embden (1874-1933) suddenly unraveled the steps of glucose breakdown and showed that hexose phosphates were indeed direct intermediates, as Meyerhof and others had proposed. Almost simultaneously Otto Warburg (1883-1970) revealed that coenzyme was a dihydropyridine nucleotide, and demonstrated its role as a reversible hydrogen acceptor-donor in the respiratory chain.

Harden continued to be active in the study of yeast glycolysis throughout the 1920s, but his later work was increasingly out of step with that of other biochemists. He never accepted hexose phosphates as true intermediates in glycolysis, preferring a different theory he had proposed in 1908. Thus the most interesting and productive period of Harden's career was 1900-1908, from his first experiments on yeast to his two major discoveries; and it is this period which will be discussed here.

There is also a more general theme that gives the origins of Harden's work a particular interest. Harden began his work just at the time when biochemistry was becoming established as a professional specialty, on the borderline between chemistry and biology.[7] Thus it was not until the postwar years that biochemists were trained as biochemists, and the question arises, how those who first began to practice biochemistry became interested in a field which at the time had few established positions and no regular mode of entry. It is a question of prime importance for all the various interdisciplinary specialties that form so important a part of modern science. Many early biochemists were recruited from physiological chemistry; but many more—and many of the most important, including Buchner and Harden were not. Harden, like Buchner, was an organic chemist, and became interested in biological problems by personal association with a biologist. Buchner's mentor was his brother,

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[6] A. Harden, *Alcoholic Fermentation* (London, 1911, and later editions.) Fritz Lippman has testified to the inspiration he received from Harden's book. Fritz Lipmann, *Wanderings of a Biochemist* (New York: Interscience, 1971).

[7] R. E. Kohler, "The enzyme theory and the origin of biochemistry," *Isis* 1973, *64*: 181-196.

Hans Buchner (1860-1903), the immunologist.[8] Harden was similarly influenced by his colleague, the microbiologist Allen Macfadyen (1860-1907), and the circumstances of their association have as great an historical interest as that of the two Buchners. As we shall see, the remarkable parallels between these two cases suggest a good deal about the special context of early biochemistry.

Arthur Harden's Early Career

Harden was the only son of a Manchester businessman of strict and austere noncomformist principles: "the family was brought up . . . abjuring the theatre and regarding Christmas as a pagan festival."[9] Harden's character reflected his dour Lancashire background:

> His personal relations with his fellow men were controlled by an uncommon reserve. His attitude toward most was that of a benevolent neutral. Few reached intimacy with him, but those who did penetrate his protective covering found in him a lovable and staunch friend. He was generally cheerful, but never exuberant, and suspicious of exuberance in others. . . . He . . . said he had never known great elation or great depression.[10]

His scientific style was likewise determinedly dry and restrained:

> Harden's outstanding qualities as an investigator were clarity of mind, precision of observation, and a capacity to analyze dispassionately the results of an experiment and define their significance. He mistrusted the use of his imagination beyond a few paces in advance of the facts. Had he exercised less restraint, he might have gone further; as it was he had little to withdraw.[11]

Since the days of John Dalton, whom Harden resembled in style and temperament, Manchester had been a center of chemical education. At Owens College (later Manchester University), which Harden attended from 1882 to 1886, he was deeply influenced by the great teacher of chemistry, Henry Roscoe (1833-1915). After graduating with honors in chemistry, Harden studied with Otto Fischer (1852-1932) in Erlangen and received his Ph.D. in organic chemistry in 1888. He then returned to Manchester as assistant to Roscoe's successor, H. B. Dixon (1852-1930). For the next nine years Harden devoted himself to teaching and to writing textbooks. With Roscoe he wrote a popular textbook of inorganic chemistry (1895) and an historical study of Dalton's work (1896). He also collaborated in a laboratory text and a revised edition of Roscoe and Schorlemmer's monumental treatise on inorganic chemistry. Harden did almost no original research; his two published papers concerned an improved stereometer and the analysis of some bronze age artefacts found near Manchester. As Hopkins observed: "it seems . . . that up to the time he left Manchester, at the age of thirty two, Harden had not manifested an overpowering desire for scientific research.[12]

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[8] R. E. Kohler, "The background to Eduard Buchner's discovery of cell-free fermentation," *J. Hist. Biol.* 1971, *4*: 35-61.

[9] F. G. Hopkins and C. J. Martin, "Arthur Harden," *Obit. Not. F.R.S.* 1942, *4*: 3-14. See p. 3.

[10] *Ibid.*, p. 14.

[11] *Ibid.*

Whether Harden's disinclination for research hindered his career at Owens, or whether he just grew restless, in 1896 he applied for the position of Principal of Wandsworth Technical Institute and of Inspector in the Department of Arts and Sciences at South Kensington. Neither post could have been a step up, but fortunately he was unsuccessful in both applications. In 1897 he did obtain a post in London, as chemist at the Jenner (formerly the British) Institute of Preventative Medicine. This appointment was undoubtedly due to his patron, Roscoe, by then an influential statesman of science, who had been closely involved with the formation of the British Institute in 1891 and had been chairman of its governing board since 1893. The Jenner Institute had been organized to provide postgraduate instruction in chemistry and bacteriology to aspiring public health officers. Harden was to be in charge of chemical instruction and analysis of water and foodstuffs. It was on the strength of his practical knowledge of general chemistry and his teaching record that he was hired. Harden obviously did not leave Manchester in search of more opportunity for research.

But ironically, fortune threw in Harden's lap the gift he had never sought. Only a few years after he came to the Jenner Institute, the London medical schools instituted their own programs in public health. Harden's course was discontinued, and Harden found himself free to devote all his time to laboratory research.[13] In the dozen years since his degree Harden had done no work in organic, much less medical chemistry, and he was thus open to suggestion. The research he took up was an investigation of bacterial metabolism, suggested to him by his colleague in the bacteriological section, Allen Macfadyen.

Allen Macfadyen's Early Career

Macfadyen, only five years Harden's senior, was born in Glasgow and educated in Edinburgh, where he obtained an M.D. in 1886 and a B.S.

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[12] *Ibid.,* p. 4.
[13] *Ibid.*

in 1888.[14] He then studied in Munich with Max Pettenkofer (1838-1901), the by then rather conservative founder of the science of hygiene, and in Bern with the physiological chemist Marcel Nencki (1847-1901).[15] With Nencki he investigated the physiology of digestion and published a paper showing that bacteria are not necessary for digestion, but only the enzymes secreted by the small intestine.[16] This work apparently aroused his interest in the less well studied enzymes of bacteria, and on his return to England in 1889 as a Grocers' Company Research Scholar at the College of State Medicine, Macfadyen studied the proteolytic enzymes secreted by bacteria.[17] The College of State Medicine was a small private institution, initiated in 1886 in two borrowed rooms to provide training in public health.[18] As it was at the time the only source of training in public health in England, it had begun to thrive, but by the time Macfadyen joined the small staff in 1891, various medical schools had begun to teach hygiene and Macfadyen lectured on bacteriology to a diminishing number of paying students. It was clearly a precarious position.

In 1892 Macfadyen published some very interesting work on the intracellular enzymes and toxins of bacteria.[19] The idea of intracellular toxins was a timely one. The discovery of the protein nerve toxins secreted by diphtheria and tetanus bacilli in 1888, and of the corresponding protein antitoxins in 1891 led to the hope that all infectious diseases would likewise be due to chemical toxins, and that all diseases would be as readily cured by chemical substances in immune sera. Toxins were eagerly sought in the culture fluids of other pathogens, and when none were found, it was assumed that the presumed toxins of cholera, typhus, etc. remained inside the bacterial cell. Here Macfadyen's experience with the secreted enzymes of bacteria came in. Macfadyen recognized, as others did at the time, the striking similarities between enzymes and bacterial toxins: both were heat sensitive, specific, functional proteins.

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[14] R. T. H., "Allen Macfadyen," *J. Hygiene* 1907, *7*: 319-322. Anon., "Allen Macfadyen," *Lancet, 1907, I,* 696-697.

[15] M. Hahn, "Marcel Nencki," *Ber. deut. chem. Ges.* 1902, *35*: 4503-4521.

[16] A. Macfadyen, M. Nencki, and N. Sieber, "Untersuchungen über die chemischen Vorgänge im menschlichen Dünndarm," *Archiv. f. expt. Pathol.* 1891, *28:* 311-350.

[17] T. L. Brunton and A. Macfadyen, "The ferment-action of bacteria," *Proc. Roy. Soc* 1889, *46B*: 542-553.

[18] H. Chick, M. Hume, and M. Macfarlane, *War on Disease, A History of the Lister Institute* (London: Deutsch, 1971), pp. 32-34.

[19] A. Macfadyen, "A research into the nature and action of the enzymes produced by bacteria," *J. Anat. Physiol.* 1892, *26*: 409-453.

Enzymes were generally not believed to act inside the cell protoplasm, having been defined as strictly exo-cellular by W. Kühne in 1877. However, Macfadyen had picked up the by then rather conservative notion that active enzymes and toxins were both in a special "unstable state," the same chemically active state to which Eduard Pflüger had ascribed the peculiar activities of living protoplasm.[20] Macfadyen thus had reason to hope that both enzymes and toxins would indeed be found inside the bacterial protoplasm, and he began the search in his area of special competence, enzymes. By extracting cholera and pneumonia bacilli with concentrated glycerol (the standard technique) he obtained extracts with protoeolytic activity. Macfadyen was encouraged: "it seemed probable that the study of the formation of these enzymes would also throw further light on the mode of production of the toxalbumins."[21] Macfadyen found that cholera extracts were indeed toxic to animals, and concluded that "the soluble cell proteids which possess these toxic properties are closely related in constitution to the ferments, and may be identical with them, or a modification of them."[22]

There is a remarkable parallel here with Hans Buchner's experiments on the intracellular immunoproteins, which were going on at just this time in Munich. Hans Buchner was led by the similar hope of finding specific antitoxins in bacterial protoplasm. And just as Buchner's experiments led later to the discovery of zymase, so Macfadyen's led indirectly to the discovery of cozymase.

In 1893 the ailing College of State Medicine was amalgamated with the new British Institute of Preventative Medicine, which had been incorporated in 1891, but which for lack of endowment had remained dormant.[23] In 1893 several substantial donations permitted it to begin operation in the modest quarters of the old College. The staff consisted of Armand Ruffer (1859-1917), London educated French physician (director), Joseph Lunt, in charge of water analyses and chemical lectures, and Macfadyen, who was in charge of bacteriological analyses and instruction. Macfadyen turned his attention from enzymes and toxins to more practical problems, and published several minor papers on airborne dust, ringworm, pasteurization, and other public health topics.

In 1895 Ruffer contracted diphtheria from his work on the manufacture of diphtheria antiserum, and after recovering sufficiently to move, resigned in 1896 and departed. Macfadyen was named honorary secretary and in 1897, director of the Institute. In the same year Lunt left for greener pastures, and was replaced by Arthur Harden.[24] There were by that time eighty students and nine research workers at the British Institute, and thanks to a large endowment by Rupert Cecil Guinness, first Earl of Iveagh, the construction of a lavish new building in Chelsea was completed and opened under the new (and short-lived) name of the Jenner Institute in 1898.[25] Harden was in charge of the chemical department, and Macfadyen was both director and chief of the bacteriological section.

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[20] *Ibid.*, 409-410.

[21] *Ibid.*, p. 410.

[22] *Ibid.*, pp. 428-429.

[23] H. Chick *et al.* (n. 18 above), pp. 31-34

Macfadyen's Work on Zymase

Although the Jenner Institute was primarily concerned with application of microbial knowledge to medicine and public health, Macfadyen's interests extended to industrial microbiology too, including alcoholic fermentation. Chick *et al* state that "as early as 1896, Macfadyen had mooted the idea of a laboratory for industrial bacteriology."[26] Lister approved Macfadyen's proposal, and it was decided that such work fell within the scope of the new constitution agreed upon with Lord Iveagh:

> The lawyers solemnly decreed that "inasmuch as the purity and good quality of beer and other articles of consumption have an important bearing on our health, zymotechnical instruction and investigation fall within the province of the Institute's work." Lord Iveagh, being a Guinness, was unlikely to object to the project.[27]

In 1899 a fermentation research laboratory was established, named for Emil Hansen, the chief mycologist of the Carlsberg Brewery. A young mycologist, G. Harris Morris, was installed, and instruction in the theory and industrial practice of fermentation was offered.

Public interest in the theory of fermentation was then at its height, following Buchner's dramatic discovery of cell-free fermentation two years earlier. Although cell-free fermentation had been accepted, there was still some controversy over the nature of zymase, and Macfadyen himself set out to reinvestigate fermentation in extracts of top yeasts of the sort used in English breweries.[28]

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[24] *Ibid.*, p. 41.

[25] *Ibid.*, pp. 43-53. The name was changed to the Lister Institute in 1903.

[26] *Ibid.*, p. 56.

[27] *Ibid.*

[28] A. Macfadyen, G. H. Morris, and S. Rowland, "On expressed yeast-cell plasma (Buchner's "Zymase")," *Proc. Roy. Soc.* 1900, *67B*: 250-266. English brewers used yeast strains that rose to the top of the fermenting wort; the "bottom yeasts" of German brewers sank to the bottom.

Macfadyen succeeded in obtaining active extracts, but several results did not agree with Buchner's reports. Most impressive to Macfadyen was the high level of autofermentation, *i.e.*, fermentation of extract without added glucose, which he found was comparable to that with glucose.[29] Buchner had made no corrections for this autofermentation. Macfadyen also found that the activity of extracts was sharply diminished by dilution, a result not characteristic of a purely enzymatic reaction.[30] Finally, he was surprised to find that more glucose was used up than could be accounted for by the alcohol and carbon dioxide formed. From these facts Macfadyen concluded that zymase was not an enzyme, but living protoplasm.[31] According to Macfadyen's theory, sugar molecules were first incorporated into living protoplasm, which then broke down into alcohol and carbon dioxide. Only sugar that entered this metabolic cycle of anabolism and catabolism could be fermented. The apparent disappearance of glucose was explained as a net balance of protoplasm synthesis over degradation in vitro.[32]

Macfadyen's theory is readily identifiable as a variant of a general theory of protoplasm that was very popular in late ninteenth-century physiology. The most influential version of this theory was Eduard Pflügers' (1829-1910) theory of tissue respiration (1875), according to which oxygen and nutrient molecules had to be incorporated into side-chains of the giant protoplasm molecule for oxidation to occur.[33] Similar versions of the protoplasm theory turn up in the controversy over zymase; but by 1900 the chemistry of Pflüger's *Riesenmolekel* was seriously outdated, and Macfadyen's was the last such theory to be seriously proposed for zymase. It probably seemed somewhat reactionary even at the time.[34] It is puzzling to find Macfadyen supporting the old protoplasm theory, in view of his advanced interest in intracellular enzymes and toxins. But we must also recall that in 1892 Macfadyen associated both enzymes and toxins with Pflüger's "living protein" or protoplasm. Indeed, it was this conservative theory that led to Macfadyen's novel interest in intracellular proteins of microbes. But the theory was aging, and only seven years later it was a shaky foundation

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[29] *Ibid.*, p. 255.

[30] *Ibid.*, pp. 260, 262.

[31] *Ibid.*, p. 265.

[32] *Ibid.*, pp. 264-265.

[33] E. Pflüger, "Ueber die physiologische Verbrennung in den lebenden Organismen," *Archiv ges. Physiol.* 1875, *10*: 251-367.

[34] R. E. Kohler (n. 2 above).

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for Macfadyen's theory of zymase. The times had changed rapidly, and Macfadyen had failed to keep pace.

Macfadyen's Later Work, 1900-1908

Macfadyen promised further reports on fermentation, but for several reasons these did not appear. For one thing, the Hansen Fermentation Laboratory was not a success; it stimulated little interest and attracted almost no students, and in 1901 it was closed down.[35] The contrariness of Macfadyen's work on zymase may also have influenced this decision. Moreover, in 1900 Macfadyen had been relieved of the directorship of the Institute. It is doubtful that his character was well-suited for administrative work:

> Dr. Macfadyen was of a naturally reserved and shy disposition and few except his intimate friends were aware of the fine character and gentle nature which he possessed. He was ever ready to think of others before himself, and it was his extreme modesty which prevented him from coming more to the front.[36]

The larger scale of the new Jenner Institute apparently strained Macfadyen's capacities too far:

> It must have become increasingly apparent that he was not the right man for the post [director], for in 1900 the Governing Body announced that "in the conditions of the altered circumstances of the Institute the present engagement of the Director be determined [*sic.*, terminated] and that for the present no Director be appointed." Macfadyen was given charge of the bacteriology department and also received a salary as secretary to the Institute.[37]

This was the first of several blows to Macfadyen's career.

As if in recompense, however, Macfadyen was appointed Fullerian Professor of physiology at the Royal Institution in 1901, a post he held until 1904. This appointment probably was due to a contact with Sir James Dewar (1842-1923), professor of chemistry at the Royal Institution. In 1899 Dewar and Sir James Crichton Browne had suggested to Macfadyen that they collaborate in studying the effects of very low temperatures on bacteria. Dewar, already famous for his work on liquified gases, provided liquid air, and Macfadyen provided pathogenic bacteria, which it turned out were not harmed in the least even by long immersion.[38] Even the sensitive living protoplasm of luminescent bacteria survived. This work with Dewar also revived Macfadyen's old interest in bacterial endo-toxins. In 1900 tetanus and diphtheria toxins were still the only ones known, and though it was still believed that the symptoms of cholera, plague, etc. were caused by similar toxins inside the cell, these had still not been isolated. Macfadyen's early hope that he had discovered the cholera endo-toxin was apparently unfounded; he never pressed his claim, and a promised paper on anthrax endo-toxin never appeared. Macfadyen's situation was thus similar to Hans Buchner's in 1893: he suspected the existence of intracellular toxins, but could not get them out of the cell.

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[35] H. Chick *et al.* (n. 18 above), p. 57.

[36] Anon. (n. 14 above), p. 696.

[37] H. Chick *et al.* (n. 18 above), p. 61.

[38] A. Macfadyen, "On the influence of the temperature of liquid air on bacteria," *Proc. Roy. Soc.* 1900, *67B*: 180-182. "Further note on the influence of liquid air on bacteria," *ibid.*, pp. 339-340.

But in 1902 Macfadyen was expert in Eduard Buchner's new method of extraction, and his work with Dewar had shown that bacterial protoplasm could be kept alive by deep freezing. Since Macfadyen believed that endo-toxins were part of the living protoplasm, it is not hard to understand his renewed hope of extracting active endo-toxins. In 1902 he announced that he had obtained an intracellular toxin from typhoid bacilli by freezing them in liquid air and grinding them in a chilled mechanical mill—the one previously used for making yeast extract.[39] The opalescent juice he obtained caused only mild fever in monkeys; but it did stimulate the production of anti-typhoid serum, which alleviated the symptoms of typhoid fever in infected guinea pigs.[40] At the time, this result must have looked like a major breakthrough in the cure of infectious diseases, and Macfadyen gave all his attention to isolating endotoxins from other pathogens. This work involved the extraction of large amounts of pathogenic cholera and typhus bacilli, and the use of large mills and presses. Contamination was inevitable, and as a result of his work, Macfadyen and two of his assistants were infected with typhoid fever, and his two assistants died.[41]

In 1903 a new director, Charles Martin (1866-1955), had been appointed to the Lister Institute, and at Lord Iveagh's request he carried out a thorough assessment of the research accomplishments at the Institute since the Iveagh bequest of 1897. Iveagh had heard disturbing rumors, and an investigating committee found that performance of most departments had been disappointing, notably pathological chemistry and bacteriology, "where, they said, the work was not conducted in such a way as to attract investigators to the laboratories."[42] The reference was certainly to the fatal accidents attending Macfadyen's work. His work on endotoxins probably seemed of dubious practical value, and his views on zymase were at that very time being proved wrong by his colleague, Harden. (Chick *et al* report that the investigating committee was pleased with the "recent signs of increasing activity in the chemical laboratory.")[43]

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[39] A. Macfadyen and S. Rowland, "An intracellular toxin of the typhoid bacillus," *ibid.*, 1902, *71B*: 77-78.

[40] A. Macfadyen, "Upon the immunising effects of the intracellular contents of the typhoid bacillus," *ibid.*, pp. 351-352.

[41] H. Chick *et al* (n. 18 above), p. 73.

As a result of this investigation, in 1904 Macfadyen was given three years salary and dismissed; it is reported he left "with some reluctance."[44] He continued his labors at Kings College and the Wellcome Laboratory,[45] and reported the isolation of endotoxins from the cholera vibrio,[46] *Pneumococcus,*[47] and hog cholera,[48] and the preparation of specific antisera. He was at work on plague and Malta fever (brucellosis) when he died in 1907 of an infection of the latter contracted in his work. He had been weakened, so his obituary says, by anxiety and disappointment, presumably over the wreck of his career.[49] In retrospect, Macfadyen was on the right track: endotoxins do exist, but they are not strictly analogous to the toxins then known, and the important antigens of cholera, etc., proved to be part not of the cell protoplasm, but the cell surface. By a cruel trick of fate, Macfadyen's work was doomed to failure. Chick *et al* write:

> He died before his time, and perhaps he had lived before it too; for he had good ideas, which have since been developed by others, but with the knowledge and techniques available to him they never quite reached fruition.[50]

Arthur Harden's Work on Fermentation

Harden's new career in research began in 1898, when Macfadyen suggested to him that he investigate the products of bacterial metabolism as a possible means of chemically diagnosing different varieties of *B. coli.* In his thorough-going way, Harden went beyond Macfadyen's limited aim; by a painstaking analysis he tried to account quantitatively for all the metabolized carbon.[51] He identified several new products, and proposed a detailed mechanism for the metabolism of glucose, which involved the simultaneous cleavage of two molecules of sugar. This was a prematurely ambitious scheme; as Hopkins pointed out, it anticipated bacterial physiology by some thirty years.

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[42] *Ibid.,* pp. 72-73.

[43] *Ibid.*

[44] *Ibid.*

[45] Anon (n. 14 above).

[46] A. Macfadyen, "Upon an anti-cholera serum," *Lancet* 1906, *III,* 494-496.

[47] A Macfadyen, "Remarks upon the pneumotoxin," *Brit. Med. J.* 1906, *III,* 776-777.

[48] A. Macfadyen, "Ueber ein Toxin des *Bacillus suisepticus,*" *Centralb. Bakt.* 1901, *43*: 143-145.

[49] Anon (n. 14 above), p. 696.

[50] H. Chick *et al* (n. 18 above), p. 73.

When Macfadyen turned his whole attention to the search for endotoxins, Harden continued the work on zymase. His next few papers show him pursuing Macfadyen's unfinished business. In 1901 he and Sydney Rowland, Macfadyen's assistant, published a study of the auto-liquifaction of pressed yeast.[52] As they had expected, this spontaneous digestion was a true alcoholic fermentation, analogous to Macfadyen's autofermentation of yeast juice. But they also showed that it was glycogen that was fermented, not the yeast protoplasm.[53] Macfadyen's protoplasmic glucose is conspicuously absent in Harden's report. This work was followed by a thorough chemical study of yeast glycogen[54] done in collaboration with William Young, who had just come to the Jenner Institute.[55] Like Harden, Young was a native of Manchester and a graduate (M.S.) of Owens College; he too was a fine experimental chemist, and in his years with Harden from 1902 to 1914 he played an important part in all of Harden's work, notably in the isolation of hexose diphosphate.

Another curious but historically important piece of work inherited from Macfadyen appeared early in 1903; it concerned the effect of blood serum on zymase.[56] In his brief historical account of the discovery of cozymase Harden referred to these experiments:

> In the course of some preliminary experiments (commenced by the late Allen Macfadyen but subsequently abandoned) on the production of antiferments by the injection of yeast juice into animals, the serum of the treated animals was tested for the presence of such antibodies both for the alcoholic and proteoclastic enzymes of yeast juice, and it was then observed that the serum of normal and treated animals alike greatly diminished the autolysis of yeast juice.[57]

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[51] A. Harden, "The chemical action of *B. coli communis* and similar organisms on carbohydrates," *J. Chem. Soc.* 1901, *79*. 610-628.

[52] A. Harden and S. Rowland, "Autofermentation and liquifaction of pressed yeast, *J. Chem. Soc.* 1901, *79*: 1227-1235.

[53] *Ibid.*, pp. 1234-1235.

[54] A. Harden and W. J. Young, "Glycogen from yeast," *J. Chem. Soc.* 1902, *81*: 1224-1233.

[55] D. Maxwell, "William J. Young," *J. Chem. Soc.* 1943, *146*: 44-45.

[56] A. Harden, "Ueber alkoholische Gährung mit Hefepresssaft (Buchner's Zymase) bei gegenwart von Blutserum," *Ber. deut chem. Ges.* 1903, *36*: 715-716.

[57] A. Harden (n. 6 above), p 38.

Unfortunately, Harden did not say exactly what Macfadyen's original interest in anti-zymase had been. Anti-enzymes had been known for some years, and around 1900 there was considerable speculation that anti-enzymes might carry out an anti-hydrolytic reaction—*i.e.*, synthesis. Although Macfadyen did not believe zymase was a true enzyme, anti-zymase would still have been of interest to him.

Harden's interest was apparently aroused by the observation that although yeast juice was known to contain a proteolytic enzyme, it did not digest serum proteins. (This fact was probably a chance observation of Macfadyen's work.) Investigating further, Harden found that "the blood serum of guinea pig, hog, and horse strongly suppress the autolysis of yeast juice." [58] Horse serum reduced the digestion of yeast juice protein by 90 per cent. This striking effect was not due to specific anti-protease, since normal serum from animals not immunized against yeast juice had the same effect.[59] The inhibition of the yeast protease was a non-specific chemical effect of the serum itself. This fact was of particular interest, since it was widely believed that the rapid disappearance of zymase activity in yeast extracts was due to the digestion of zymase by protease.[60] Harden thus saw that serum might be used to increase the fermentation of glucose by yeast extracts: "it was thus of the greatest interest to investigate the effect of the addition of serum to a mixture of press-juice and sugar." [61] Serum did increase this fermentation, by 60-80 percent, and Harden concluded that this effect was indeed due to preservation of the "fermentation-causing agent" from digestion.

In 1903 Harden and Young published a thorough reinvestigation of Macfadyen's experiments on yeast juice.[62] Their primary aim, they stated, had been to explain the "disappearing glucose," *i.e.*, the key evidence for Macfadyen's theory of protoplasm. Harden showed that the difference between extracts of English and German yeasts that Macfadyen had observed was more apparent than real. English yeast juice responded much less well to the addition of glucose, and thus its autofermentation appeared to be much greater than that of German yeast juice. Harden and Young confirmed the large loss of free glucose, but showed that it could be quantitatively accounted for as a nonreducing derivative, from which glucose could be liberated by boiling with acid,[63] a treatment Harden knew hydrolyzed glycogen. But Harden cautiously concluded that whatever this derivative was, it was produced by an enzyme different from zymase.[64] (In fact, it turned out to be glucose phosphate.)

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[58] A. Harden (n. 56 above), p. 715.

[59] Harden himself gave no protocols for experiments with immune sera; these may well have all been done by Macfadyen.

[60] See E. Buchner, H. Buchner, and M. Hahn, *Die Zymase-Gährung* (Munich, 1903), pp. 287 ff.

[61] A. Harden (n. 56 above), p. 716. All previous experiments had been carried out on yeast juice without added glucose.

[62] A. Harden and W. J. Young, "Gährversuch mit Presssaft aus obergähriger Hefe," *Ber. deut. chem. Ges.* 1904, *37*: 1052-1070.

Harden also returned to the question of how blood serum inhibited the action of yeast protease, and it was in the course of clearing up this minor detail that he stumbled upon cozymase. Not knowing what chemical component of serum inhibited the protease, Harden tried to reproduce the same effect by other means.[65] It had been known since 1898 that enzyme catalyzed reactions were reversible, and by 1904 it had been shown that a concentrated solution of amino-acids inhibited (or even reversed) the action of proteases.[66] Harden therefore tested boiled, autolysed yeast juice, in which the protein had been degraded to amino acids. This solution caused a marked increase in the fermentation of yeast juice, but to Harden's surprise, the control solution of boiled fresh juice produced the same effect. It was not amino-acid inhibition of protease that was stimulating the action of zymase, but some specific heat stable factor in yeast juice.[67]

The results of further experiments on this new factor were reported to the Physiological Society on 12 November 1904, eight months later.[68] The cofactor was dialyzable and precipitable by alcohol, and more important, it was absolutely required for the action of zymase. This astonishing fact Harden demonstrated by filtering yeast juice under pressure through a Martin filter, a porcelain filter saturated with gelatine. The gelatinous protein material retained by the filter was totally inactive, but when recombined with the filtrate it could ferment glucose at the rate of the original extract. It apparently took some time for Harden to hit upon this method of separating zymase and cozymase. Hopkins and Martin later noted that Harden first tried simple dialysis, but that proved too slow: zymase was destroyed at a faster rate.[69] Another biographer suggests that C. J. Martin himself, who had invented the gelatine filter some years earlier to separate the enzyme and toxin of snake venom, suggested its use to Harden.[70] If so, Harden was doubly fortunate in his associates at the Lister Institute.

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[63] *Ibid.*, pp. 1064-1067.

[64] *Ibid.*, pp. 1067-1068.

[65] A. Harden (n. 6 above), p. 38.

[66] See H. M. Vernon, *Intracellular Enzymes* (London, 1908), pp. 185 ff.

[67] A. Harden (n. 6 above), p. 38.

[68] A. Harden and W. J. Young (n. 3 above), pp. ii-iii.

[69] Hopkins and Martin (n. 9 above), p. 7.

Harden's Work on Cozymase and Phosphate, 1905-1908

Coenzymes were not unprecedented in 1904. In 1897 Gabriel Bertrand (1867-1962) had discovered that laccase (an oxidase) required manganese, and proposed the name "coferment" for such cofactors.[71] He also noted that calcium appeared to be necessary for the action of pectase, and in 1904 R. Magnus found that liver lipase required an organic cofactor.[72] But zymase requires two cofactors, cozymase and inorganic phosphate; this extra complication was quite novel and unexpected, and took Harden some time to unravel.

The first detailed paper on cozymase appeared early in 1906, more than a year after the preliminary announcement.[73] Here Harden and Young presented evidence that part of the effect of boiled yeast juice was due to phosphate, but left open the question whether a second cofactor was also required. Harden later left the impression that the discovery of the phosphate effect was the result of an exhaustive empirical analysis:

> As a result of a large number of attempts to isolate the constituent of boiled yeast juice which brings about the increase in fermentation, it was found that whenever an increase was produced phosphoric acid was present. The effect of the addition of phosphates to yeast juice was, therefore, examined and it was found that a well-marked initial rapid evolution of carbon dioxide was thus produced.[74]

But it is interesting that the stimulation of zymase by phosphate had been thoroughly investigated by a Polish physiological chemist, Augustin Wroblewski, in 1901.[75] Harden did not even mention Wroblewski until much later, although Wroblewski's long paper on zymase was the most thorough chemical analysis of yeast juice yet done, and was published in a well-known journal. He described 67 experiments on the stimulation of zymase by phosphate, and discussed at length his theory that the action of phosphate was to protect the sensitive pieces of protoplasm against acids or bases (*i.e.*, by what is now known as buffer action).[76] He proposed in fact that the protective action of phosphate was a general feature of cell life, and cited the ubiquitous presence of phosphate in the cell juices of all organisms. It is difficult to believe that Harden could not have known of Wroblewski's paper in 1905; but it is equally hard to believe that so straightforward a man would have failed to give credit if credit were really due. I suspect that Harden and Young had begun to suspect phosphate, searched the zymase literature for precedents, and found more than they had hoped for.

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[70] I. Smedley-Maclean, "Arthur Harden," *Biochem. J.* 1941, *45*: 1071-1081. Chick *et al* repeat this detail, but give no source.

[71] G. Bertrand, "Sur l'intervention du manganese dans les oxydations provoquées par la laccase," *Comptes Rendus* 1897, *124*: 1032-1035.

[72] R. Magnus, "Zur Wirkungsweise des Esterspaltenden Fermentes (Lipase) der Leber," *Z. physiol. Chem.* 1904, *42*: 149-154.

[73] A. Harden and W. J. Young, "The alcoholic ferment of yeast-juice," *Proc. Roy. Soc.* 1905, *77B*: 405-420.

[74] *Ibid.*, p. 414.

[75] A. Wroblewski, "Ueber den Buchner'schen Hefepressaft," *J. prakt. Chem.* 1901, *64*: 1-70. See pp. 10-16.

In any case, it soon became clear that phosphate was not the only active cofactor in yeast juice. The crucial test was in principle a simple one: if the addition of phosphate alone restored the activity of the residue of zymase left behind on a Martin filter, then phosphate was the sole cofactor in boiled yeast juice. Harden had done this experiment by 1906, but reported only that "no decisive result has as yet been obtained." [77] It is extremely difficult, in fact, to get the filtered residue perfectly inactive and free from dissolved cofactors, without its being permanently inactivated. Harden was right to be wary of a negative result, and did not officially conclude that a second cofactor was required until his second paper six months later.[78] It is evident from his protocols how hard he tried to get a positive result. But the addition of phosphate alone to the filtered residue never restored fermentation without the addition of filtrate or boiled yeast juice too.

Harden had other reasons to suspect the existence of a second cofactor, as a result of a very important technical innovation he had made early in his studies of zymase. Instead of measuring released carbon dioxide gravimetrically in sealed evacuated flasks, as Buchner had done, Harden devised an apparatus for measuring the released gas volumetrically. Thus whereas Buchner was limited to measurements at two or three intervals in the course of an experiment (because measurement interrupted the experiment), Harden could measure the rate of fermentation continuously. For the first time the detailed kinetics of zymase action and the effect of reagents on zymase could be investigated. As Chick *et al* observed, Harden's new technique is a symbol of the emergence of the new dynamic biochemistry, with its concern not just with chemical statics, but with the dynamics of chemical change.[79] It also led to an important practical result. Harden found that the addition of phosphate or boiled yeast juice had two quite distinct effects on the kinetics of zymase: a sharp but temporary initial rise in rate of fermentation; and a prolongation of the normal period of fermentation. Harden noticed that boiled juice generally gave a more marked prolongation than phosphate did, and this difference apparently convinced him that yeast juice contained a cofactor in addition to phosphate.[80]

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[76] *Ibid.*, pp. 15-16.

[77] A. Harden and W. J. Young (n. 73 above), p. 419.

[78] A. Harden and W. J. Young, "The coferment of yeast-juice," *Proc. Roy. Soc.* 1906, *78B*: 369-375.

Even more impressive to Harden was the observation that the initial burst of carbon dioxide caused by phosphate was strictly proportional to the amount of reagent added.[81] Such a result would cause the pulse of any chemist to quicken, perhaps even Harden's, for it was the first suggestion of a stoichiometric intermediate step in alcoholic fermentation. Following up this lead, Harden and Young found that the added phosphate was rapidly and completely converted to a form no longer precipitable by magnesium: *i.e.*, it became attached to an organic molecule,[82] from which it was released by boiling with acid. This fact, combined with Macfadyen's "disappearing glucose" left little doubt as to what this molecule was. Harden predicted that the organic phosphate formed was a glucose phosphate ester,[83] and this prediction was borne out by Young's isolation of a hexose phosphate from yeast juice in 1907.

Harden was characteristically cautious about proposing a mechanism to account for these remarkable new facts:

> The fact that the extra carbon dioxide . . . is equivalent to the phosphate present suggests the superposition of two actions. Whether this is to be explained by the presence of two distinct enzymes or simply by the increased activity of a single enzyme remains to be decided.[84]

In their third paper in 1908 Harden and Young wrote an overall equation $2\ C_6H_{12}O_6 + 2\ Na_2HPO_4 = 2\ CO_2 + 2\ C_2H_5OH + C_6H_{10}O_4(PO_4Na_2)_2$ expressing the molar relation of fermentation and phosphorylation.[85] The function of the coenzyme, Harden tentatively suggested, might be to mediate the transfer of phosphate to glucose. In his fifth paper later in 1908, Harden cautiously ventured a speculation on the relation of glycolysis and phosphorylation.[86] He proposed that two molecules of glucose decomposed into four small fragments, two of which became alcohol and carbon dioxide, while the other two were recombined to form the phosphorylated hexose chain. At what point phosphorylation occurred he was uncertain:

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[79] H. Chick *et al* (n. 18 above), p. 73.

[80] A. Harden and W. J. Young (n. 73 above), p. 419.

[81] *Ibid.*, pp. 411-413.

[82] *Ibid.*, p. 417.

[83] *Ibid.*, p. 418.

[84] *Ibid.*, p. 416.

[85] A. Harden and W. J. Young, "The function of phosphates in the fermentation of glucose by yeast-juice," *Proc. Roy. Soc.* 1908, *80B;* 299-311. See pp. 302-303.

> The introduction of the phosphoric acid groups may possibly occur before the rupture of the original molecules, and may even be the determining factor of this rupture, or again this introduction may take place during or after the formation of the carbon chain.[87]

Gradually Harden settled on the idea that phosphorylation and fermentation were stoichiometric, coupled reactions, the former somehow providing the driving force for the latter. He had already shown that hexose phosphate was prevented from accumulating in yeast by a special enzyme, phosphatase; [88] phosphate was thus recycled and served a catalyic function. This was the view Harden upheld in his Nobel Address in 1929:

> It appears to me that the fundamental idea expressed in the original equation of Harden and Young is nearer to the truth than any alternative that has yet been suggested. A coupled reaction of some kind occurs, as the result of which the introduction of two phosphate groups into certain sugar molecules . . . induces the decomposition of another molecule.[89]

This was only four years before Embden's discovery of phosphoglyceric acid suddenly revealed the mechanism of the breakdown of glucose phosphate. In Harden's conservatism we see, perhaps, another aspect of his "undue suspicion of adventures into the realm of imagination": having ventured, he was equally unwilling to retreat.

Conclusion

It is interesting to ponder briefly on the striking similarities in the way Eduard Buchner and Arthur Harden began their work in biochemistry. Both were organic chemists, neither very successful. Both, one suspects, were ready to seize the opportunity for change offered by the chance of birth or an unsettled career. For both this opportunity was the result of contact with a microbiologist, who was already working on what would in time be labled as a "biochemical" problem. Thus Buchner's discovery was an unexpected offshoot of Hans Buchner's experiments on immunoproteins; Harden's, of Macfadyen's experiments on anti-zymase. The parallel is significant. As I have pointed out before, the new immunology of toxins and antibodies had already become a biochemical science in the early 1890s, and was an important precedent for the emerging biochemistry of enzymes. It is no accident that the two most important early biochemical discoveries were the indirect result of work by microbiologists working in immunology.

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[86] A. Harden and W. J. Young, "The function of phosphates in alcoholic fermentation," *ibid.* 1908, *82B*:321-330.

[87] *Ibid.*, p. 329.

[88] A. Harden and W. J. Young (n. 85 above), p. 303.

[89] A. Harden, "The function of phosphate in alcoholic fermentation," *Nobel Lectures Chemistry 1922-1941* (Amsterdam: Elsevier, 1966), pp. 131-143.

The parallel can be drawn closer: both Macfadyen and Hans Buchner had a very special interest in the *intracellular* active proteins of microorganisms, at a time in the early 1890s when few immunologists or enzymologists did. Ironically, both Buchner and Macfadyen acquired their novel interest from ideas that were already old-fashioned at the time: in Buchner's case it was Nägeli's conception of disease as active warfare (for Buchner, chemical warfare); in Macfadyen's, it was Pflüger's idea of the living protoplasm molecule. For Buchner and Macfadyen themselves, these theories were a dead-end. Buchner never found bacterial endo-proteins with real therapeutic value; and Macfadyen never isolated the endo-toxins he hoped he had found. The less sublime but more practical and profitable offshoots of their work they left to their protégés to pursue. Thus both suffered the ironic fate of many pioneers: despite excellent instincts and timing, their vision was limited by their particular experience and background, and they were neglected and surpassed in their own time. Despite his eminent position, Hans Buchner was remembered as Eduard's brother; Macfadyen's advanced interests earned him disdain and an early death. In retrospect, however, their visionary concern with intracellular proteins staked out the province that was quickly and successfully colonized by practical biochemists, such as Eduard Buchner and Arthur Harden. In some ways Eduard Buchner and Harden are less interesting and sympathetic than their mentors, and it is not surprising, perhaps, that their best biochemical work was their first. It may be that this accidental conjunction of theoretical vision and a bent for practical experiment is necessary for the emergence of a new hybrid science from two separate disciplines, each with its own opportunities and limitations. Given the inertial tendency of men and traditions, it is perhaps not surprising that the crucial combination of vision and practical capacity occurred in the persons of two men, both products of the old order but one the first of the new.

HOSPITAL PATIENTS IN FLORENCE: SAN PAOLO, 1567-68

BERNICE J. TREXLER

Two hospital admission books from the male ward of San Paolo in Florence from 1554 through 1568 seem to be the oldest survivors of this genre in the Florentine Archives. The latter book is particularly noteworthy: for fifteen months it records the "diagnosis" of the majority of patients as part of a formula which includes also the patient's name, dates of entry and discharge, bed number, and often the patient's occupation and place of origin.

This study is based on twelve of the fifteen months during which the "diagnosis" is noted: from October 1, 1567, through September 30, 1568. Background information on San Paolo will furnish a historical context. The duties of the *infermiere* who kept these records will be touched on, the diagnoses and their frequency will be examined, as well as information on the patients and their disposition. Finally, the use made of the available beds, and the occupations of the patients will be analyzed.

The largest hospital in the city caring for the sick poor was Santa Maria Nuova, with 230 beds in 1454. San Paolo was modest; its twenty-bed capacity put it on a par with several other hospitals. It was founded around 1284 as a hospice for the poor and pilgrims by a small group of Florentine citizens, who cared for their clients during leisure time. By the fourteenth century the sick poor were cared for by men who lived in the hospital. In order to care for sick females, lay women joined the hospital during this period. These men and women formed third orders (lay brothers and sisters) of Saint Francis and were known as "pinzocheri." These *pinzocheri*—twelve in 1398—set up rules for governing the hospital, and were supervised by a Franciscan "visitor" who was to oversee the functioning of the hospital and the deportment of the lay brothers and sisters.[1]

In 1404 there were thirty-five beds for the sick poor,[2] and it would appear that by this time nursing care was foremost, with the healthy poor and pilgrims going to other hospices in the city. The hospital did not grow in size, for in 1567-8 twenty beds were enumerated in the male ward. Since in other city hospitals and hospices there were regularly more beds allotted for males than females, total hospital beds may still have been about thirty-five.[3]

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[1] L. Passerini, *Storia degli Stabilimenti di Beneficienze di Firenze;* (Florence, 1853), pp. 165, 167, 170.

[2] *A*(rchivio) di *S*(tato), *F*(irenze), *Provvisioni* 93, ff 196rv (9 Feb. 1404 stil. flor.).

During the fifteenth century, the hospital was wracked by internal dissension, only the sketchy outlines of which have surfaced. In 1425 Pope Martin V demanded that the *pinzocheri* reform, laying emphasis upon limiting the women's freedom of movement. Continued supervision of the hospital by the Franciscan supervisor was stressed. Perhaps Pope Nicholas V's 1451 commission to the archbishop, Franciscan supervisor, and Proconsul of the guild of Judges and Notaries to step in bore some fruit, for the next known reform was in 1516, when the lay sisters asked Pope Leo X to remove them from the supervision of the Order of Saint Francis at Santa Croce. The Pope complied by putting them directly under the jurisdiction of the archbishop of Florence, and gave them permission to become nuns. Still, a Franciscan from Santa Croce continued to supervise the lay brothers and the hospital through 1571.[4] In that year the papal nuntio signed new constitutions for the hospital.[5] In 1588 San Paolo ceased to be a hospital by order of Grand Duke Ferdinand I, and was designated a convalescent house. The building was repaired and equipped with 120 beds for men, women, and children. Grand Duke Peter Leopold suppressed the hospital altogether in 1780.[6]

In view of the hospital's turbulent history one is not only surprised that these two admission books have survived, but that they show no administrative deterioration. In fact, the second admission book, "Maschi 1562-1568,"[7] is more sophisticated and detailed than the earlier "Liber infirmorum maschi 1554-1561."[8] The 1554-61 book always notes the bed number, but contains no admission numbers, and includes the discharge date only in the year 1554. Deaths were carefully indicated by a cross in the margin, but father's names and patient's occupations and origins are spotty. The formula used in this earlier volume is as follows:

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[3] This applies only to those hospitals and hospices with facilities for both men and women. For examples of this preponderancy of male beds, see Passerini, *Storia,* pp. 14-15.

[4] *Ibid.,* pp. 172-176.

[5] *ASF, Santa Maria Nuova, Fondo San Paolo, reg.* 912, *cap.* 47. I shall refer to this *Fondo* as *San Paolo.* Care of the sick poor seems to have continued uninterruptedly, at least on the male side of the hospital, although this does not preclude the possibility that females were being cared for. They would have had their own admission books, as in the death books of Santa Maria Nuova which were kept separately for females and males from 1470 to 1865. See *ASF, Santa Maria Nuova, regs.* 730-859.

[6] These were to be iron beds and to accommodate only one patient; Passerini, *Storia,* pp. 179-180, 183.

[7] *ASF, San Paolo, reg.* 889.

[8] *ASF, San Paolo, reg.* 640.

> Giovanni comes [here] sick, with his clothing, on the fifth day of April, 1554. N.2 [9]

Turning now to the later admission book, the frontispiece reads:

> This book is of the hospital of Saint Francis of the *Pinzocheri,* otherwise [called] San Paolo, in which will be taken note of all the sick. It commences on the second day of April, 1562.[10]

Bed numbers continue to be noted with regularity in the right hand margin; admission numbers appear from August, 1564 to October, 1567, and were placed in the left margin. The discharge date appears regularly only from 1567 through 1568. Deaths were still carefully marked, and father's names are more common. More origins and occupations were specified. By May of 1567 still more information is being included, as can be seen in this example:

> On May 6, 1567, comes here sick Beto di Rafaello, burler, with his clothing and without money. To bed 15.
> He left to [go to] the *Incurabili* on the 15th of the said month.[11]

From this point on there are scattered references to deaths from fever, somewhat detailed accident cases, and an occasional admission "diagnosis." Then in mid-September, 1567, the inscriber started noting the diagnoses in earnest, and continued this practice for the next fifteen months.[12] Only eighteen of the 364 patients during the twelve month period to be examined have no diagnosis.[13]

The admission book in question was kept by Benedetto di Lozo Marini, the hospital's *infermiere,* who assumed this post around November, 1566.[14] The diagnoses he recorded ceased with the termination of Benedetto's tenure in 1568. One wonders why Benedetto included the diagnoses in the record at all. Keeping a record of the admissions was certainly not his sole responsibility. The constitutions of San Paolo of 1425 had said that "uno infermiere" was to look after the well-being of the sick, to keep a record of the things he found, and report to the Franciscan supervisor and to the minister of the hospital every four months.[15] In 1571, just after Benedetto's tenure, the revised regulations barely touched on the duties of the *infermiere*. He was not to have administration of money, food, or goods, but only of work, and was to serve the hospital and the sick in the customary manner.[16] The duties we know Benedetto to have fulfilled are more in line with the regulations of 1374 from Santa Maria Nuova. There the *infermiere* was to write the patient's name, surname, and date of admission in the admission book, and record the date on which the patient died or left the hospital. Any money or goods deposited by the patient upon admission were to be returned to him. In case of death, these monies or goods became the hospital's property. The *infermiere*'s duties included general care of the sick, distribution of food, and administration of syrups, beverages, and individual medicines as ordered by the doctor.[17] Benedetto probably fulfilled these nursing duties as well as recording the admissions. He did collect, itemize, and return all money deposited by patients upon admission.[18] He also turned the money left after a patient's death over to the head of the hospital.[19]

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[9] "Giovanni vinne amalato con sua panni a di 5 di aprile 1554 N 2"; *ibid.,* f. 2

[10] "Questo libro è dello sped.^{e} di S° Fran° di Pinzochero altrimenti S° Pagolo, in sul quale si terra nota di tutti e malati che averranno cominciato a di 2 aprile 1562"; *ASF, San Paolo, reg.* 889.

[11] "E a di 6 di magio 1567 vene qui malatto beto di rafaelo purgatore e cho sua pani e sanza danari alletto 15 andosene agli incurabili a di 15 detto"; *ibid.,* f. 79v.

[12] *Ibid.,* ff. 87v-191r.

[13] This census is higher than the average of 307 patients per year in 1562-68. During 1554-61 the average was 343 patients per year.

[14] On an unpaginated folio in the back of the volume: ". . . comicatto questo di . . . dame benedetto di lozo marini infermiere di detto ispedale . . ."; *ASF, San Paolo, reg.* 889.

Benedetto certainly seems to have enjoyed his duties as inscriber. If he had been constrained to keep to a routine format, we would not have such choice items as these:

> On 24 October, 1567, he comes here sick of a constitutional illness, as can be seen in his face . . Giovanni di Lozo da Reo, miller. . . .[20]

Giovanni died after twelve days in the hospital. Of interest, too, is:

> On February 18, 1567, ill with fever Domenico di Rinaldo, spinner of carded wool in Via Romita . . . he came with the Misericordia. . . .[21]

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[15] Passerini, *Storia*, p. 173.

[16] *ASF, San Paolo, reg.* 912 ("Riforma per lo Spedale et monasterio di San Paolo . . . ," *cap*. 14.

[17] I. Del Lungo, *Il R. Arcispedale di Santa Maria Nuova* (Florence, 1888), p. 78.

[18] ". . . vene malatto di febre manetto . . . con sua pani e sanza danari. Ánzi con llire sei e s. 6 d. 8 riserbo ame fermiere £ 6 s. 6 d. 8. Andosene. . . . Riebe sopradetti danari."; *ASF, San Paolo, reg.* 889, f. 88v.

[19] ". . . rede lanima a dio . . . ene autti . . . lire quattro e sodi tre e per lui a messer govani ispedaligo de lo ispedale di S^{to} Pagolo"; *ibid.*, f. 91r.

[20] ". . . vene mallatto di fisicho chome si vede in la facia . . . govani di lozo de reo mugniaio . ."; *ibid.*, f. 91r.

[21] ". . . malatto di febre domenicho di rinaldo testitore di pani lani ini vie romita . . . e vene chola misirichordia . . ."; *ibid.*, f. 99v. Domenico seems to have been the only patient who was transported to the hospital by the *Misericordia,* a group of men composing the Fraternity of the *Misericordia,* who made it their charitable duty to transport people to the hospital; Passerini, *Storia*, p 469.

Perhaps the most touching note is the following:

> On May 4, 1568, came in one who was blind and wanted to remove the cataracts from his eyes, Stefano di Giovanni Terma from Genoa. . . . He left in the same condition in which he came. God be praised always.[22]

These miscellaneous examples of Benedetto's spontaneity make one wonder if the diagnosis was simply the complaint the patient reported upon admission, or if Benedetto himself listened to the various complaints, and "diagnosed" patients himself. Some of these patients may have been sent to the hospital by a doctor in the city, or San Paolo may have had doctors on duty as did San Matteo (a hospital of comparable size to San Paolo) and Santa Maria Nuova (a much larger hospital). In both San Matteo and Santa Maria Nuova salaried doctors lived in, and were to respond to any need for their services, as well as to make rounds twice a day.[23] The only doctor mentioned in the volume under study appears in a discharge note of Benedetto's: Meo, who had come in sick, left, "discharged by the doctor."[24] Since only three years later it was said that neither a doctor nor a surgeon had been used for some time, it may be that in 1567-68 the hospital depended on the charitable work of city practitioners who visited patients.[25]

Although the source of the "diagnoses" remains uncertain, their sophistication and variety are intriguing. For ease in handling, these diagnoses have been grouped into twelve categories, as shown in Chart A. I have included in *both* pertinent categories those patients who were admitted with more than one diagnosis, as well as those for whom additional pertinent information was given. For example, a patient having both fever and scabies is included in both Category I and Category II.

Category I is the largest grouping, containing 220 "fever" patients. This is sixty-four percent of the total number of patients with a diagnosis. Only six of the 220 have a specified type of fever: five had quartan and one tertian fever.[26] These six cases occurred between November and

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[22] ". . . vinne qui che deto era ciecho e vollie chavare le chate ratoli degli ochi stefano di govani terma da genova. . . . Andosene in medesimo termine che lui ci vene e dio ladato sia sempre"; *ASF, San Paolo, reg.* 889, f. 107r.

[23] *ASF, Santa Maria Nuova, reg.* 45, ff. 38, 173; reg 588, f. 133v; Passerini, *Storia*, pp. 156, 305.

[24] ". . . liscenziatto da dottore"; *ASF, San Paolo, reg.* 889, f. 90r.

[25] *ASF, San Paolo, reg.* 912, *cap.* XIV.

[26] *ASF, San Paolo, reg.* 889, e.g., f. 89v "febre"; e.g., f. 91v "la quarittana" (an intermittent fever which returned every third day); f. 93r "aveva laterzana" (an intermittent fever which returned every second day).

CHART A

Categories

Admiss. per month		I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII *
27	October 1567	12	2	3	1	2	0	1	0	2	2	1	Exhaustion
25	November	11	1	2	5	0	0	0	1	1	1	3	Hemorrhoids
27	December	13	3	1	3	2	0	2	0	0	0	0	Testicle Ailment Anio Ulise
19	January 1568	12	1	0	0	0	1	1	1	0	0	1	Cough
20	February	12	0	1	0	0	1	0	1	1	1	1	*Mal di pino*
31	March	15	6	0	2	1	0	0	3	0	1	1	*Mal di pino*
32	April	14	9	2	3	1	0	1	2	0	1	1	Tuberculosis Leg trouble
45	May	26	12	3	1	1	0	1	3	1	0	0	Cataracts Cold Foot ailment
30	June	15	5	1	0	0	1	1	4	0	1	0	*Abascia*
49	July	28	6	0	1	2	1	1	3	0	1	1	Old age Dropsy Inflamed throat
34	August	24	1	0	0	1	0	2	4	0	2	0	Dropsy
25	September	18	0	1	0	1	0	2	1	1	1	1	
364	Total	220	46	14	17	11	4	12	23	6	11	10	17

* One patient indicated by each diagnosis listed below.

April, whereas the heaviest influx of fever cases occurred in May, July, and August. Eighteen of the 220 listed an additional ailment. Ten suffered from scabies, while two complained of pain. One subsequently recovered from smallpox; one had *paroticha*; one had a boil; another had drainage; one was suffering from *fisigo*; and the eighteenth planned to go to the hospital of the *Incurabili* to take the wood treatment.[27]

Category II consists of the second largest grouping: forty-six patients (fourteen percent) suffering from skin diseases. Forty-two cases of scabies predominate, with four of these specifying large scabs and a fifth *rognia savatiecha*.[28] The category is completed by two cases of smallpox, one of herpes, and one case of erysipelas.[29] April and May were the peak months of admissions for scabies.

Category III includes fourteen patients who suffered from boils, ulcers, or drainage. There were six cases of boils (one of which was referred to before draining as an encrusted ulcer, and thereafter as a boil). Boils were specified as being on a rib, the chest, and on the head. One encrusted ulcer was on the neck.[30] The location of three of the four cases of drainage was specified: one from a leg, and two from the eyes. This category also includes three patients with a sore under an arm, the *paroticha*, and a fistula.[31]

Category IV embraces eight cases of *mal di fisigo* or constitutional illnesses, as well as five patients admitted with "many illnesses," one case of jaundice, two cases of obstruction,[32] and one case of dysentery.[33]

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[27] *Ibid.,* e.g., f. 104v "febre e rognia"; e.g., f. 108r "doglie e febre." F. 89r "febre . . . guaritto da vaiola"; f. 108r "febre . . . guarito di una paroticha che aveva in uno orechio" (probably an inflammation of a parotid gland); f. 94v ". . . febre . . . rende lanima . . . che aveva una postema in chapo . . ."; f. 112r "febre e scesa"; f. 103v "febre e di fisigo" (a constitutional malady); f. 113v "febre . . ando per la poliza per intrare agli ichurabili a pigliare laqua da legnio." This treatment is discussed in footnote 41.

[28] *Ibid.,* e.g., f. 109v "rognia"; f. 114v "rognia grosa"; f. 104v "rognia savatiecha" (field scabies is problematical).

[29] *Ibid.,* e.g., f. 89v "vaiolo"; f. 101r "ma di formicha"; f. 116v ". . . una resipolo che detto aveva neviso."

[30] *Ibid.,* e.g., f. 108v "una postema"; f. 92r "una groma di sopra allorichi . . . ropesi la postema." F. 90r "una groma ni golla."

[31] *Ibid.,* f. 89r "duna iscesa." F. 185r "aveva una piaga sotto il bracio"; f. 108r; f. 107r "una fistola."

[32] *Ibid.,* e.g., f. 90v "malatto di fisigo"; e.g., f. 91v "malatto di piu mali"; f. 101v "che sili era isparto il fele"; e.g., f. 102r "malatto di opillato." Perhaps these two cases of obstruction were cases of chronic constipation. In a manuscript containing medications, from Santa Maria Nuova (1515), there is a remedy titled "powder to purge obstructions": "Polvere a purghare oppilati," *B*(iblioteca) *N*(azionale) di *F*(irenze), *Manoscritti Magliabecchi* Cl. XV, cod. 92, f. 61r.

[33] *ASF, San Paolo, reg.* 889, f. 117v ". . . di malle di pondi . . .". A letter from Hieroni-

In Category V are eleven patients with *mal franzese* or syphilis. One of these patients was pronounced cured after spending seven days in San Paolo. Another was planning to go to the *Incurabili* to take the wood treatment, while a third planned to return to the *Incurabili*. Only one of the syphilis patients returned to San Paolo after having taken the wood treatment.[34]

Two patients, both with readmissions for *tichone* or typhus, comprise Category VI. The first case was hospitalized seven days, and readmitted thirteen days later for eight days. The second case was discharged after thirteen days in San Paolo, then returned the next day for six more days, only to leave unrecovered again.[35]

Category VII includes twelve patients complaining of pain. Four patients have unspecified pains. The remaining eight complained of fever and pain in the side, in a rib, in a testicle, in a leg, the pains of syphilis, and finally, one patient apparently had kidney stones.[36]

Category VIII contains twenty-three accident cases, seven percent of the admissions with diagnoses. Six of these patients complained of having received blows on the head. Five had suffered falls, two from the bridge of Santa Trinità in Florence.[37] Four other patients had been wounded: one on the arm, another on the knee and hand, a third had a piece of metal in his foot, while the fourth had received thirteen wounds.[38] Four patients had skull fractures, another a fractured face, still another had fractured his mouth and was readmitted when the wound broke open. One patient had been bitten on the leg by a dog.[39]

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mus Mercuriale to the Grand Duke of Tuscany, Ferdinand I, speaks of treating dysentery called "mal di pondi" in Florence; F. Puccinotti, *Storia della Medicina del Medio-evo,* II (Livorno, 1859), p. 798.

[34] *ASF, San Paolo, reg.* 889, f. 102r "malatto di mal franzese." F. 89v. F. 89r. F. 104v "di male franzese dise di reavere a ire a pigliare ili legni ali ichurabili." F. 186r.

[35] *Ibid.,* f. 97v "aveva uno tichone." F. 117r "e vise arimetare alletto . . . per che detto non era guarito di tichone . . . andosene . . . che no era achora guarito." Typhus raged through Europe in the 1500s, and the symptoms were carefully described by writers of the day. See B. Pullan, "The famine in Venice and the new poor law," *Bollettino di Storia della Società e dello Stato Veneziano V-VI* (Venice, 1963), p. 160.

[36] *ASF, San Paolo, reg.* 889, e.g., f. 95r "malatto di doglie." F. 102r "male di fiancho e febre"; f. 122v "una doglia in una choscia"; f. 124r "aveva male a uno testiculo"; f. 105v "male a una gamba"; f. 122v "doglie di male franzese"; f. 113r "malatto di renela."

[37] *Ibid.,* e.g., f. 113v "glera estatto dato di uno bastone i sue chapo"; f. 121v "era chaschatto a tera."

[38] *Ibid.,* e.g., f. 110r "ferito in uno bracio"; f. 122r; f. 92v; f. 110r.

[39] *Ibid.,* e.g., f. 120v "aveva rotto il chapo"; f. 119v; f. 122v "rotta la bocha"; f. 123v "torno qui che se era riguasto la bocha". F. 119r "ricevuto uno morso da uno chane chorso in una gamba."

Category IX includes those six patients who came in expressly for a laxative. Although only one of the four was specified as having syphilis, four of the six intended to follow the laxative with the wood treatment.[40]

In Category X are the eleven patients who planned to leave San Paolo to take the wood treatment, which was given only at the hospital of the *Incurabili.* Two of these had syphilis, one fever, and one scabies.[41]

Category XI contains ten patients who stated that they had taken the wood treatment at the *Incurabili.* Three of the ten had been previously admitted to San Paolo, although only one of the three had left with the stated intention of going to take the wood treatment. He returned to San Paolo one month after leaving. A second of the three returning was a syphilis patient, who returned to San Paolo after an absence of fifty days. Thus these patients fit the norm of thirty to fifty days hospitalization at the *Incurabili.* Only one of these ten patients was admitted to San Paolo specifically to convalesce.[42]

Category XII is a miscellaneous section for less common diagnoses. The individual diagnoses have been listed in Chart A in the month in which these patients entered the hospital. Under upper respiratory infections we find patients with a cough, *tisico* or tuberculosis, a cold, and one with an inflamed throat.[43] The complaints of exhaustion, old age, cataracts, and hemorrhoids are easily recognizable.[44] Complaints of a malady in a testicle, a foot ailment, or something wrong with a leg are too vague to be pinpointed.[45] Perhaps *anio ulise* is *anisuria*, a urinary infection. *Mal di pino* was a disease which attacked the extremities of the fingers, but otherwise remains a puzzle. *Abascia* should be gout, but *abascia sul petto* (on the chest) makes this seem doubtful. *Ritopico* is hydropicus or dropsy.[46]

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[40] *Ibid.,* e.g., f. 92r "malatto anzi sivolleba purgare"; e.g., f. 90r "per purgarsi per ire a pigliare ili legnio."

[41] *Ibid.,* e.g., f. 93r; f. 113v; f. 115v ". . . rognia grosa . . andosene . . . voleva andare per la poliza di potere andare ali ichurabili." Only one other patient left San Paolo in order to obtain such a permit. The others simply say they are going for the treatment. The treatment was thought efficacious for skin diseases as well as syphilis, and seems to have taken from thirty to fifty days to complete satisfactorily. Benedicenti writes that after a patient was purged in another hospital, he began a regime at the *Incurabili* of alternately sweating, eating a dry diet, and drinking a solution in which the bark of the cinchona tree had been soaked. The strict regimen was considered debilitating to many; if necessary, they were to return to a hospital in the city to convalesce; A. Benedicenti, *Malati—Medici e Farmacisti I* (Milan, 1924), pp. 724-25. An eighteenth-century doctor at the *Incurabili,* where syphilis was still being treated in a similar manner, stated that the dessicating treatment used in past times was no longer necessary since sarsparilla was now mixed with the "holy wood." Patients were still being purged prior to their admission to the hospital, and after completion of the treatment they were sent to San Paolo to convalesce; *BNF, Ms. Palatino,* 843, ff. 8v-11r, by Dr. G. T. Tozzetti (1751). For the founding of the *Incurabili* hospitals, see B. Pullan, *Rich and Poor in Renaissance Venice* (Cambridge, Mass.; Harvard Univ. Press, 1971), p. 233.

[42] *ASF, San Paolo, reg.* 889, e.g., f. 91r ". . . e venutto qui che aveva preso ilegnio ali churabili." F. 102v ". . . che tornava da gli churabili Aveva preso ilegnio e per ristoro. . . ."

[43] *Ibid.,* ff. 98r, 105v, 108v, 118v "ispramazia."

In the early sixteenth century ideas about contagious diseases were being formulated. In 1546, the Veronese physician Girolamo Fracastoro published his concept of contagion as an infection passing from one individual to another. He stated three fundamental methods of infection: those by contact alone, those where contact left contagious fomites, such as scabies, etc., and those which transferred infection at a distance, such as ophthalmias and other exanthemata, which were called smallpox and the like.[47]

Fracastoro's ideas were certainly known in Florence at the time of our study. Had his theories been accepted and utilized in San Paolo at this time? Were known or suspected contagious diseases isolated, or at least an effort made to keep patients having possibly contagious diseases in separate beds? From the records of the institution of San Paolo as a convalescent hospital in 1588 we know that large wooden beds had been in use during the period under study. These beds often held more than one patient. The results of a bed study indicate that beds were often shared, and there is no indication of an attempt to isolate contagious diseases.

The male ward of the hospital presumably contained twenty beds, since the assigned beds were numbered one through twenty. Yet during this year 1567-68 five beds (numbers 3, 4, 9, 18, and 20) were not assigned to patients. Perhaps these beds were no longer serviceable or had been removed. There was also a "fractured heads room" with two beds in it which was upstairs from the ward.[48]

Generally only one patient was in a bed, although there were at least forty-one instances of doubling two patients in one bed for periods lasting from one to eighteen days. Chart B shows the total number of patient days each bed was in use per month and the known cases of doubling (see digits in parentheses in the chart). This chart also includes the number of admissions and the mean length of hospital stay per month. A glance at Chart B does not indicate a correlation between patient load, length of stay, and availability of bed space. Not shown in this chart is the fact that less than fifteen percent of the known instances of doubling seem to have been dictated by necessity. That is, sharing can be explained only occasionally by the fact that all beds were occupied.

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[44] *Ibid.*, f. 89v "stacheza"; f. 118r "vechiaia"; f. 107r; f. 91v "morice."

[45] *Ibid.*, f. 94r "male di granela"; f. 111r "malatto in uno pie"; f. 105v "perchosa in . . . gamba."

[46] *Ibid.*, f. 94v; f. 100r; f. 114v; f. 116r.

[47] R. H. Major, *Classic Descriptions of Diseases* (Springfield, Ill.: Charles C Thomas, 1939), p. 7.

[48] *ASF, San Paolo, reg.* 889, f. 113v ". . . ando ini chapi rotti . . . alletto di rotti 1. Vene qui a giu . . . e andosene. . . ."

Actually, the disposition of the patients appears to have been random, even when doubling them up. Relating diagnoses to beds or to doubling in the ward has failed to turn up any clear principles of distribution. Some beds were obviously favored more than others, but the reason for such favoritism is unclear.

In spite of the apparent arbitrariness of assigning patients to beds, we do find several instances where the patient's welfare seems to have dictated transferring. The most striking instance of this is the transfer from bed #14 to bed #10 of a fever patient, who was transferred "for his convenience" (or comfort).[49] He left #14 and moved into #10 with another fever patient. A wounded patient was moved from bed #11 to bed #8 (neither bed otherwise occupied) "because he is not well." [50] Another patient was transferred from one unshared bed to an empty bed after "his boil had drained." [51]

Two patients with fractured skulls were transferred to the "fractured heads room" from ward beds. One was transferred "for more security." [52] He had been admitted to the ward even though he had a fractured skull. Generally, though, fractures went directly to the "fractured head room," which represents the only perceptible relationship between diagnosis and bed assignment. Of the twenty-two accident cases admitted to the hospital, eight were sent directly to the "fracture room." Three of these patients had suffered skull fractures; three had been dealt blows on the head; one had been wounded; and one had fractured his mouth.[53]

Benedetto's discharge notes have already provided us with the length

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[49] *Ibid.*, f. 186r ". . . rimesilo all letto 10 per pure sua chomodita. . . ."

[50] *Ibid.*, f. 122r ". . . e mesesi aletto 8 perche ne ista bene."

[51] *Ibid.*, f. 92r ". . . ropesi la postema adetto . . . e mesesi a letto dicanove. . . ."

[52] *Ibid*, f. 99r ". . . tramutamo ili sopradetto pagolo . . . e mesesi ini chamera di chapi roti e per piu sichurezza. . ." The hospital of Santa Maria Nuova also had a separate room called "capirotti" where the wounded went; Passerini, *Storia*, p. 305.

[53] Only one other unusual disposition of a patient was made during this twelve-month period: Carlo di Niccolò da Genova, a gentleman of Signore Aurelio, went upstairs to a room (presumably a private room) with a diagnosis of fever, f. 123r.

CHART B

Bed Utilization

			Number of patient days each bed in use per month														
Month	Month's Admiss.	Mean Stay	Bed #1	#2	#5	#6	#7	#8	#10	#11	#12	#13	#14	#15	#16	#17	#19
Oct.	27	11.1	0	17	21 *	8	17	16	12	49 * (3)	23	25	24	22	24	30 (2)	16
Nov.	25	12.5	0	11	0	49 (1)	23	16 *	0	16	15 *	26	16	18	17 *	22	11 *
Dec.	29	10.8	3	19	22	26 (1)	15	36 (1)	14	28	31	24 (1)	31	10	10 *	31	19
Jan.	19	9.4	0	0	12	15	27	23	0	3	23 (1)	21	29	17	15	27	12
Feb.	20	16.9	0	12 *	27	19	23	27	0	1	24	10	15	21	18	0	0
Mar.	30	10.6	3	18	13 *	23 (2)	27	10 *	3	15	22	31	10	21	13	12	17
Apr.	33	10.3	0	10	27	58 (4)	35 (2)	30	1	13 (1)	25	26	22	40 (2)	12 (1)	29	11
May	45	12.1	31 (1)	18	32	48 (2)	31	22	26	26	29	29	20	22	31	28	24
June	30	9.5	35 (2)	22	23	47 (1)	15	19 (1)	31	28	23	21	30	21	19 *	12	18

July	49	9.5	0	17	27	51 (3)	25	29 (2)	35 (1)	18	27	35 (1)	29	26	23	30	4 *
Aug.	34	9.8	0	0	25	31	28	30 * (2)	3	10 *	18	23	25	21 (1)	23	23	20 *
Sept.	25	11.0	9	21	0	14	25	12 *	7 * (1)	11	12	8	11 *	28	19	27	17
Total number of patients per year			7	18	24	25	33	28	21	26	28	34	29	29	23	23	19
Total number of cases of doubling per year			3	0	0	14	2	6	2	4	1	2	0	3	1	2	0
Deaths			0	1	1	0	0	1	1	2	0	4	1	1	0	1	0

Note: Asterisks indicate lack of discharge or transfer dates; digits in parentheses indicate number of cases of doubling per bed per month.

of stay, given in Chart B. They also give information about the state of patients' health upon discharge: for example, twenty patients were pronounced "cured," although one returned with the same complaint. Nine had been admitted with fever. One of these fever patients was discharged cured of smallpox, as was another patient who had no admitting diagnosis. Another fever patient recovered from the *paroticha*. Patients admitted with pain, exhaustion, erysipelas, syphilis, boils, and many illnesses were discharged cured. Two accident cases also recovered completely: one who had fallen, another who had been hit on the head.

We have more information on patients who were not fully recovered when discharged. The most interesting example is that of Giovanni di Pape, a worker at San Paolo. Admitted with a boil on a rib, he left the hospital the next day and was "carried to Santa Maria Nuova."[54] It is the only case of a patient being sent to another hospital, other than the *Incurabili*. One other item makes Giovanni unusual: he was the only patient who was admitted "without clothing." This would appear to indicate that he had been moved from his bed in the staff living quarters and placed in a ward bed. Another uncured patient left after four days in the hospital "to stay at the orchard to recover" from his boil.[55] A third patient who left San Paolo uncured was the blind patient who desired to have the cataracts removed from his eyes; a fourth was the patient with *tichone* who left the hospital twice unrecovered from that disease, in order to return to the shop where he worked.[56]

One of every eighteen patients—twenty in all—was readmitted during the year. Considering the lapse of time, and comparing the original and later diagnoses, one sees that six of these readmissions were unrelated to the earlier diagnosis. Six were clearly related to the earlier admissions, and five were probably related to their first admission. These eleven patients reentered within nine days of their discharge. Two patients were admitted a total of three times.

A very significant statistic to be gleaned from our book is the death rate. We might expect it to be high; in fact only three and a half percent of the patients died in the hospital. Such a low rate shows that San Paolo admitted a wide variety of sick people, the majority being other than terminally ill. Quite evidently, the hospital was not thought of as a last resort. Nothing shows this more clearly than the fact that only one of twenty-three accident cases died.

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[54] *ASF, San Paolo, reg.* 889, f. 115r ". . . Andosene anzi portolo a S^{ta} M^{a} No^{a}. . . ."

[55] *Ibid.*, f. 90r ". . . Andosene astare allortto . . . per guarire."

[56] *Ibid.*, f. 117r ". . . vose ire a bottega che no era achora guarito. . . ."

The thirteen deaths include five from *fisigo*, five from fever (including one who also had a boil), and one each from wounds, drainage, and "many illnesses." Those with constitutional illnesses (*fisigo*) were in the hospital from six to thirty days before their deaths; those with fever from four to nine days.

Having completed the medical analysis of the document, we turn now to a question of social import. What social groups had access to this medical care? San Paolo had been established to take in the poor and the pilgrims. Then it turned to caring for the *sick* poor. Who were then "the poor"? The regulations of the *Incurabili* (1574) state that those who were to be treated were the poor unable to afford medical care.[57] It is consequently not surprising that at San Paolo no patient admitted during the year under study had a clearly recognizable Florentine family name. The surprising aspect is rather to be found at the other end of the social spectrum: those admitted were not the indigents of Florentine society, but can be characterized as able-bodied manual laborers and small merchants. For instance, many of those admitted have an occupation listed. Among them were servants whose masters' names are given. Most of the Spaniards are identified as soldiers. And as we have seen, one patient left the hospital unrecovered because he wanted to return to the shop. These are strong indications that the majority of the patients were actively employed.

This judgment is based on an occupational analysis of those admitted. Fifty percent of the patients were thus identified, giving us a good sample upon which to base the premise that "sick poor" did not mean indigents. Chart C indicates the various professions, as well as their relationship to the diagnoses.[58]

The chart fails with one exception to turn up any clear relationship between disease and occupation. The cloth industry was Florence's chief industry, so it is not surprising to find that thirty-three percent of the total number of professions mentioned were involved therewith.

Two groups deserve special mention. It is interesting to note that eight priests were cared for at San Paolo rather than in the local hospital for

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[57] *ASF, Santa Maria Nuova, Fondo Incurabili, reg.* 1, *cap.* vi, "Non si possino . . . recivere se non . . . porteranno . . . fede del' lor parrocchiano che sono de maniera poveri, che non possono con il proprio medicarsi. . . ."

[58] Under cloth workers are included weavers of wool, brocade, and satin, dyers, spinners. washers, burlers, beaters, and croppers.

CHART C

Diagnoses by Occupation

	Fever	Skin Diseases	Boils and Drainage	Constitutional	Syphilis	Typhus	Pain	Accidents	Laxative	Wood Treatment	Post-*Incurabili*	Deaths
WORKERS: Cloth: 66	36	12	4	3	2	0	6	0	1	3	3	4
ASSORTED: 79 Employees or Apprentices	2	1	2	0	0	2	0	1	0	1	0	0
Servants	19	5	3	1	2	0	0	3	1	1	1	0
Porters	7	0	0	0	0	0	1	1	0	0	0	0
Masons	4	2	0	0	0	0	0	0	0	0	0	0
Manual laborers	8	1	0	0	0	0	0	2	0	0	0	0
Millers	2	1	0	1	0	0	0	0	0	0	0	1
ARTISANS OR MERCHANTS: 43												
Merchants	15	2	0	0	0	0	2	2	0	0	0	0
Tailors	1	2	0	1	0	0	1	0	0	1	0	0

Potters	2	3	0	0	0	0	0	0	0	0	0	0
Smiths	0	1	0	0	0	0	2	0	0	0	0	0
Bakers	6	1	0	0	0	0	0	0	0	0	0	0
Shoe Makers	3	1	0	2	0	0	0	0	0	0	0	1
PRIESTS: 8	2	4	0	1	2	0	0	0	0	0	1	1
FOREIGNERS: 16	10	2	1	0	1	0	0	0	1	1	1	0

priests.[59] Six of these priests were from the cathedral, Santa Maria del Fiore, the seventh from the Medici parish church of San Lorenzo, and the eighth came from outside the city. The priests represent only two percent of the total admissions, but twenty percent of the syphilis patients, and ten percent of the patients with skin diseases.

The last group is made up of sixteen foreigners. Two of them were Germans, apparently servants, while the remaining fourteen were Spaniards. Twelve of the Spaniards were soldiers manning the garrisons in the city. This group of foreigners accounts for five percent of both the fever and scabies patients, but only ten percent of the syphilis cases.

The absence of noticeably aged individuals is quite marked. Benedetto did not record the ages of his patients, yet only two are clearly elderly: the patient admitted for old age, and the blind man who wanted to have the cataracts removed from his eyes. This would again suggest the predominance in the hospital of able-bodied working men and merchants.[60]

Most of these men seem to have lived and worked in Florence, although the majority do not appear to have been Florentines by birth. In fact, eighty-seven percent of the patients whose origins are given were from outside Florence. Not only are dozens of villages and hamlets in Tuscany represented, but a number of cities as well, as far away as Genoa and as close as Prato. Nearly fifty percent of the servants had out-of-town origins, coming from such places as Empoli, Genoa, Legniaia, the Mugello, Pistoia, San Casciano, Verona, and Urbino. Eighteen percent of the cloth workers were from outside Florence, whereas seventy percent of the porters have outside origins noted, most of them from Lago Maggiore. This striking indication that Florence continued to draw immigrants for its labor force is only one of the significant finds which Benedetto's diligence has permitted us.

"The hospitals in Italy are provided with all the necessities. They are well constructed, one eats and drinks well in them, and there one is cared for with solicitude. The doctors are able, the beds and furniture are clean and well kept." [61]

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[59] Passerini, *Storia*, p. 521.

[60] One cannot assume that Benedetto's not noting a profession could indicate an aged individual or an indigent because some patients have no profession noted in their first admission whereas a profession may be listed in the second admission. In all, forty-three percent of the admissions include the patient's origin.

[61] Cited in Pio Paschini, *Tre ricerche sulla storia della chiesa nel cinquecento* (Rome, 1945), p. 11. For this reference I thank R. C. Trexler.

Martin Luther's positive evaluation of Italian hospitals can be partially verified by our findings. For one thing, the death rate was low, even though patients with different diseases did share beds, and no specific area was set aside for known communicable diseases. Too, the diagnoses do not indicate that patients were entering the hospital as a last resort. The detailed admissions notes suggest careful organization of patient care. Such sophistication in diagnosing and recording is another indication of a Florentine's passion for ordering and categorizing.

Even though the reputation of the hospitals may have attracted many patients, this alone does not explain their popularity. No doubt a stronger factor was their accessibility: they were free. When one was sick it was cheaper to be in the hospital where food, medicine, and physician's visits were provided than it was to stay at home. With patients remaining hospitalized from one to 150 days, no limit seems to have been set on the length of a patient's stay. It appears that most patients remained in the hospital until they were symptomless.

This paper answers only a few questions about one small Florentine hospital. One would like to know, for instance, what medication, treatments, and diet were ordered for each patient by the doctor. No such books have as yet cropped up in the Florentine archives, but as early as the fourteenth century instructions were given that such records be kept. Informative as well would be the ages of the patients. We do know that hospitals kept records of patients' ages at death, because a life expectancy table worked out at Santa Maria Nuova in 1526 has survived.[62] Knowing more about the physical plan of the hospital would tell us much about the organization of hospital care.[63]

Despite these limitations Benedetto's efforts have afforded us a valuable picture of the patients and diseases in a Florentine hospital of mid-sixteenth century.

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[62] R. C. Trexler, "Une table florentine d'espérance de vie"; *Annales E.S.C.* XXVI (1971), pp. 137-139.

[63] There is an eighteenth century floor plan of San Paolo in *ASF, Santa Maria Nuova, reg.* 24. This plan incorporates the remodeling done when San Paolo became a convalescent hospital.

THE BLIND MAN AND THE FRENCH REVOLUTION

DORA B. WEINER [*]

> "The world of the blind, filled with hands begging for alms, was transformed by Valentin Haüy into a world where those hands read and work."—Guéhenno

> "The Abbé de l'Epée created souls; I only invented eyeglasses."—Valentin Haüy

Blindness seems to have a frightening meaning for man, and therefore the blind have long been generously aided—and sometimes revered as prophets and poets who can "see" into the future. In medieval France, King Saint Louis endowed a hospital for "fifteen times twenty patients" in 1260 and the "Quinze-Vingts" [1] were soon imitated in Padua, Venice, and elsewhere.[2]

The eighteenth century took a lively interest in this question, and not for charitable reasons only: the problem of perception, the role of the sensory organs in conveying impressions of the outside world that the mind can transform into thought fascinated philosophers, especially after John Locke had formulated the problem for the Enlightenment. The French encyclopedists and *philosophes* found in sensualism a means for combating the dogma of innate ideas, and strengthening their belief that all values are relative, thus forging another weapon in their fight against the Catholic Church. Diderot's *Letter on the Blind for the Use of Those Who See* (1749)[3] earned him imprisonment at Vincennes—the point of blindness in the seeing having been too expertly made.

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[1] This hospital for the blind is still functioning, and the author here gratefully acknowledges the cordial reception and generous assistance offered by the present administrator, M. J. G. Galtier and the archivist, Mme Charles. An inventory of the archives was published in 1962: M. Robinet, *Inventaire général des archives des Quinze-Vingts* (Paris: Hôpital des Quinze-Vingts, 1962). See also M. Battelle, *Notice historique et statistique sur l'Hôpital royal des Quinze-Vingts* (Paris: Chapelet, s.d. [1856]); L. Le Grand, "Les Quinze-Vingts depuis leur fondation jusqu'à leur translation au Faubourg St. Antoine, 13ème au 18ème siècles," Société de l'historie de Paris et de l'Isle de France, *Mémoires,* 1886; *13:*107-260; 1887, *14:*1-208; and F.-G. Riffard-St. Martin, *Rapport sur l'Hôpital des Quinze-Vingts* (Paris: Imprimerie nationale, 1792).

[2] R. Sand, *The Advance to Social Medicine* (London: Staples, 1952), p. 407. The best bibliographic guide to literature on the blind during the era covered in this paper is C. Guyot and R. T. Guyot, *Liste littéraire philosophe ou Catalogue d'étude de ce qui a été publié jusqu'à nos jours sur les sourdsmuets, sur l'oreille, l'ouie, la voix, le langage, la mimique, les aveugles, etc.* (Groningue: Oomkens, 1842). The author wishes to thank Dr. John B. Blake, Director of the History of Medicine Division, National Library of Medicine, for bringing this book to her attention.

Active involvement of the *philosophes* was to be of great tactical advantage to the reformers. Even more important was the development, in the mid-eighteenth century, of an improved method of printing with raised characters.[4] This was perfected by Valentin Haüy (1745-1822), the great champion of the blind in France and the founder of a school to render blind children literate and self-supporting. Eventually his system would be replaced by a compact alphabet of raised dots invented by Louis Braille (1809-1852) in 1829.[5]

Valentin Haüy, the son of a poor weaver, had been educated by the Premonstrant Priests in his native Picardy and at the Collège de Navarre in Paris.[6] He had studied languages and calligraphy and was making a living as an expert on ancient and old-fashioned handwriting, and as a translator of foreign texts. In 1783, he was named Royal Interpreter, also serving the Admiralty and the Paris City Hall. He joined the Bureau académique d'écriture[7] which was presided over by Le Noir, the Paris Police Lieutenant and the Councillor of State Moreau. Haüy's involvement with ancient and foreign writing familiarized him with the problems inherent in conveying the meaning of words to persons who do not understand them. He tells the following story: one day, he watched some blind street musicians perform. Their outlandish costumes were designed to move the audience to mirth and pity. However, it was their use of touch and hearing that stimulated Haüy's imagination and gave rise to his method. In his *Essay on the Education of the Blind* (1786) he explains his program in an explicit subtitle:

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[3] D. Diderot, "Lettre sur les aveugles à l'usage de ceux qui voient," in *Oeuvres*, 21 vols. (Paris: Brière, 1821-23), I, pp. 279-364.

[4] Attempts at devising such a method had been made ever since the introduction of movable type in the 15th century. René Sand writes: "Gerolamo Cardano, quoting Erasmus on this subject, describes in Book VII of his *De subtilitate* a method of teaching the blind to read and write by the use of a quill pen with which they followed characters engraved in bronze plates. Nicholas Saunderson, professor of mathematics at Cambridge (1682-1739), who had been blind from the age of one, used a chequered board with nine holes pierced in each square; pins stuck in one or another of these holes represented different figures, and could also be used to construct geometrical diagrams." Sand, *Advance to Social Medicine*, p. 408, n. 1.

[5] See below, pp. 86-87; also n. 2.

[6] Born at St. Just-en-Chaussée in 1745, Valentin Haüy grew up together with his brother René-Just, two years his senior, the famous founder of crystallography. The priests at the local monastery recognized the two boys' outstanding intellectual gifts. Their education was paid for by Valentin's godfather, the Royal Councillor Valentin Joly, Controller of the Domains and Forests of the Generality of Amiens. Their mother accompanied the two boys to Paris where they attended college. (These details result from a perusal of the dossier "Valentin Haüy" at the Association Valentin Haüy pour le Bien des Aveugles, 5, rue Duroc, Paris. The author wishes to thank M. P. Schneider-Maunoury, the General Secretary, and the Librarian, M. Gautrot, for making these papers accessible to her.) This Association also privately published the best short biography of Haüy: P. Henri, *Valentin Haüy: Premier instituteur des aveugles* (Paris: Association Valentin Haüy, n.d.). See also P.-A. Dufau, *Notice sur Valentin Haüy, créateur des procédés spéciaux d'enseignement à l'usage des aveugles* (Paris: Fain, 1844).

> *Treatise on various ways, tested by experience, of enabling the blind to read with the help of touch, to print books in which they can study languages, history, geography, music, and so forth, and to perform varied tasks on simple machines, etc.*[8]

This title summarizes the curriculum of the school he would found: academic subjects would consist of history, geography, mathematics, and foreign languages including Greek, Latin, English and Italian. He was ambitious for his students, hoping that some of them might become teachers of seeing as well as blind pupils, maybe translators like himself, and, in any case, involved with Italian music. Indeed, singing and instrumental skills ranked high on his list, for practical as well as esthetic reasons: a choir and a small orchestra might earn money for the school. So would the products of the manual trades that each student must learn: weaving, rope- and mat-making, book binding, and especially printing; Haüy even projected the production of books in foreign languages, and of sheet-music.

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[7] Having been an "associate member" (agrégé) since 1781. Two of Haüy's addresses to that Bureau are preserved at the Bibliothèque nationale: "Essai sur la nécessité de se livrer à l'étude des anciennes écritures," in Mémoires et éloge lus dans la séance publique du Bureau académique d'ecriture, en présence de M. Lenoir, Conseiller d'état, Lieutenant général de police, et de M. Moreau, Conseiller du Roi en ses Conseils d'état et privé, son procureur au Châtelet, le 8 novembre 1781" (Paris: Houry, 1781), and "Mémoire sur l'éducation des aveugles," *ibid.*, 18 novembre 1784 (Paris: Houry, 1784). See also H. Maîstre, "Valentin Haüy et ses fonctions d'interprete," *Correspondance historique et archéologique*, 8ème année (1901), 242-47, 258-63.

[8] V. Haüy, *Essai sur l'éducation des aveugles ou Exposé de différents moyens vérifiés par l'expérience pour les mettre en état de lire à l'aide du tact, d'imprimer des livres dans lesquels ils puissent prendre des connaissances de langue, d'histoire, de géographie, de musique, etc., d'exécuter différents travaux relatifs aux métiers, etc.* Dédié au Roi (Paris: Imprimé par les enfants aveugles, sous la direction de M. Clousier, imprimeur du Roi, 1786). For correspondence regarding royal sponsorship of this book, see Archives nationales (henceforth referred to as AN) F 17 1145.

From the beginning Haüy adhered to certain principles regarding the education of the blind that would subsequently be questioned, discarded, and eventually re-instated. His underlying assumption was that blindness is a disability—no more. He treated blind children as children, not invalids. He mixed blind and sighted students from the start, using blind teachers to instruct sighted pupils and vice versa. He admitted boys and girls, housing them separately, of course, but letting them learn, work, play, and eat together. He spoke of them as "his children." Eventually fourteen of them married and nine of them had children of their own—he did not frown on a union of two blind persons, but was widely criticized for encouraging it. To Haüy the individual's feelings seemed more important than theory.

His purpose was primarily didactic rather than therapeutic, even though the details of his rules and method show an extraordinary sensitivity to the physical and psychological needs at all stages of blindness. One is impressed with his awareness of the blind children's eagerness and ability to learn, their need for companionship, for playmates and relaxation, for music and manual skills, for normal intercourse with sighted children and grownups.

Originality was not his outstanding contribution, for many individuals had already experimented with relief letters, maps, abacuses, chessboards, and even piano keys in order to replace sight by the sense of touch. Haüy had met Mademoiselle Paradis, the blind Viennese pianist who gave a much publicized concert in Paris in 1784. She showed him perforated and embroidered cards and maps, and also some embossed printing by the German Weissenburg with whom she was in touch, who had, in turn, learned from a Christian Niesen, also of Mannheim, in Germany.

Haüy's unique contribution is to have laid the firm, detailed, and lasting groundwork for schools to teach the blind. His intuitive understanding of the learning and adjustment problems faced by blind children is truly remarkable. So is his lifelong preoccupation with the mechanical and practical details of type, printing press, tools, machines. He was forever tinkering with the letters used for embossed printing, trying to make the product more legible; devising maps, musical notation. He was truly the contemporary not only of Rousseau's theories in the *Emile,* but of the plates for Diderot's *Encyclopédie* as well. His printing shop and workshops were established at the same time as his school. Typically, his response to an objection that books printed by his method were too voluminous (which they are) was to set to work experimenting with a "method of abbreviations," that is, "shorthand."[9] As it turned out, of course, communication with the blind progressed not by small improvements, but by methodologic quantum jumps—to Braille, then phonograph records, now tapes. In his textile shop, he introduced a multiple spinning wheel with spindles whose speed each blind worker controlled—thus eliminating the frustration that uniform speed might have imposed on blind children who might fumble or err.[10] At the turn of the century his imagination would be caught by the newly invented semaphore and the possibility of conveying messages by flashes of light, or by sound.[11] Had Louis Braille entered Haüy's school a little earlier than 1819, one might speculate that he would not have had to fight the established system for eleven years before he achieved recognition.

Haüy was promoting an attractive cause, namely a group of children, blind from birth, whom a good man of the people was trying to render literate and self-supporting by nurturing their stunted rational, manual, and artistic faculties.

He was a skillful salesman, ever eager to turn his audiences' compassion to the advantage of "his children." And the times were propitious, for the 1780s marked the high tide of humanitarianism, sentimentality and belief in the Rights of Man.

In an "Historical Sketch" appended to his book, Haüy tells how he introduced François Le Sueur, the most adept of his pupils, to an "academic assembly' (no doubt the *Bureau académique d'écriture*) presided over by Le Noir who

> . . . saw our initial attempts and welcomed them with an interest he soon communicated to ministers, protectors of the arts and of poverty. M. le Comte de Vergennes, M. le Baron de Breteuil, the Controller General, the Keeper of the Seals invited young Le Sueur to perform his exercises before them. . . .[12]

High society and the government thus became involved, and, more important, the newly founded Philanthropic Society[13] whose members were aristocratic or enlightened, humanitarian, and rich. Valentin Haüy joined that society in 1785 and, with its financial backing of 12 livres per month to support the education of 12 young men, opened a small school, 18, rue Notre-Dame des Victoires.[14]

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[9] Haüy, *Essai*, pp. 31-32.

[10] *Ibid.*, p. 117, n. 21.

[11] See below p. 80.

[12] V. Haüy, "Précis historique de la naissance, des progrès, et de l'état actuel de l'Institution des enfants aveugles," *Essai*, p. 121.

[13] Founded in 1780 by seven men, the Society had grown to 128 by 1784 and two years later to 436. The annual dues were high: 96 livres. One of its earliest projects was to support 12 blind children of workingmen, aged 2 to 12, with a monthly stipend of 6 livres (see an announcement in the *Journal de Paris*, December 20, 1783). It was this stipend, doubled, that gave Haüy's school its start. The number of children thus supported soon grew to 40.

Meantime the Marquis de Condorcet, Perpetual Secretary of the Academy of Sciences, invited Haüy to bring his students and his method before that prestigious assembly (to which René-Just Haüy had recently been elected). The four commissioners who reported on that meeting of February 16, 1785, were Desmarest, the geologist, Demours, the oculist, the celebrated anatomist and Secretary of the Royal Society of Medicine, Félix Vicq d'Azyr, and the Duc de la Rochefoucauld-Liancourt, the premier philanthropist of late eighteenth-century France. Their comprehensive and laudatory report[15] was soon followed by the ultimate official sanction, namely an audience, on December 26, 1786, with the king and his family who, the *Journal de Paris* reports "deigned grant marks of kindness" to Haüy and his students.[16] As one historian put it, "the enterprise was launched."[17]

The outbreak of the French Revolution seems, at first, to have brought few changes. The following episode may serve to illustrate the high regard in which Haüy was held by the citizens of Paris: on September 1, 1789, a M. Vacher de Charnois delivered a patriotic speech, after a mass in which Haüy's blind students sang.[18] Haüy asked to present the

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See J.-F. Payen, *Notice historique sur la Société philanthropique de Paris* (Battignoles: Hennuyer, 1846), and M. P. Péan de St. Gilles, *La maison philanthropique de Paris* (Paris: Lemerre, 1892), pp. 17-29, *passim.*

[14] Its first locale had been rue Coquillière. He was no doubt helped by his young wife, Louise Marguerite Allain, daughter of a merchant from Rambouillet. They had two daughters; the elder one died in 1797 at the age of 14. (Institution Valentin Haüy, dossier "Valentin Haüy.")

[15] This 13-page report, "Extrait des registres de l'Académie royale des sciences, Février 16, 1785," is reprinted in Haüy's book.

[16] Cf. *Journal de Paris,* January 1 and 8, 1787 See also L. P. de Bachaumont, *Mémoires secrets pour servir à l'histoire de la république des lettres en France depuis 1762 jusqu'à nos jours,* 36 vols. (London: John Adamson, 1783-89), vol. 33, pp. 257-58.

[17] L. Lallemand, *Histoire de la charité* (Paris: Picard, 1912), IV, Pt. II, Bk. I, p. 48. The whole world must have been ready for reform of this kind for similar schools were opened within the subsequent fifteen years at Liverpool (1791), St. Petersburg (1791), Edinburgh (1792), Bristol (1793), London (1799), Vienna (1804), Berlin (1806), Prague (1807), Amsterdam (1808), Zurich (1809), Copenhagen (1817), and the Perkins Institute for the Blind in Boston (1829). Sand, *Social Medicine,* p. 408, and A. Castiglioni. *A History of Medicine,* E. B. Krumbhaar, tr. (New York: Knopf, 1947), p. 734.

[18] Le Vacher de Charnois, *Discours sur le patriotisme prononcé le mardi, ler septembre 1789, à la suite d'une messe célébrée par les Enfants Aveugles, à l'occasion de l'Octave de St. Louis* (Paris: Clousier, 1789).

printed text of this speech to the City Council. On September 8, 1789, he

> arrived at the Hôtel de Ville, escorted by a batallion of National Guard. . . . The Assembly [of the Paris Commune] moved to the great hall to receive him. The two leaders of the Commune [Bailly, the mayor, and La Fayette, the commander of the National Guard] soon arrived. They entered to the sound of military music performed by Haüy's children.
>
> M. Haüy then asked for permission to have the blind children of both sexes perform a special piece of music relevant to the occasion. The singing of the young girls and the musical accompaniment greatly moved the audience with compassion toward these unfortunates and with veneration for the accomplishments and modesty of their teacher.[19]

In March 1790, Haüy again brought his students to the Hôtel de Ville for a public demonstration of their skills, and Sébastien Mercier, in his *Tableau de Paris,* commented that there is "nothing more touching than to watch Haüy in the midst of his students whose sight he seems to restore by perfecting their other senses." [20]

Haüy sought this kind of publicity because he felt a deep sense of worry about the future of his establishment. With the progress of the Revolution, the Maison philanthropique declined, contributions diminished, wealthy patrons began to emigrate. One of the participants reports:

> In order to help support themselves, the blind children sang masses in various churches [in 1790], and afterward a collection was made on behalf of the institution, since the Philanthropic Society was deteriorating daily and scarcely able to help.[21]

It soon became clear that government assistance would be needed if this school was to survive in an emergency situation created by the social and economic turmoil.

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[19] S. Lacroix, ed., *Actes de la Commune de Paris pendant la révolution,* 7 vols. (Paris: Cerf & Noblet, 1894-1909), v. 1, pp. 508-9.

[20] *Ibid.,* vol. 4, p. 477. The quotation is from L. S. Mercier, *Tableau de Paris,* 12 vols. (Amsterdam: n.p., 1783), vol. 12, p. 179.

[21] Galliod, *Notice historique sur l'établissement des jeunes aveugles* (Paris: Imprimé aux Quinze-Vingts, 1828), [10]. Since this is the only account of Haüy's institution during the revolutionary and Napoleonic era by a participant, this author was anxious to locate a copy of the text. But a three-week search in Paris in the summer of 1973 produced no results, either at the Bibliothèque nationale, or at the Archives nationales, the Quinze-Vingts archives, the Institution nationale des jeunes aveugles, or the Institution Valentin Haüy pour l'aide aux aveugles. Finally, on the afternoon before leaving, a renewed search in the library of the last-named institution revealed a card-board-bound volume by Galliod, dated 1828, said to be in Braille. A closer look revealed the long-sought text, printed on blank paper in relief letters—that is by Haüy's method, preceding Braille. The unusual method of production of this text accounts for its non-existence in the catalogues of printed books, manuscripts, or books in Braille. (Page references are to the long-hand transcript in my possession owed to the kind collaboration of M. Gautrot, librarian.)

The National Constituent Assembly first became aware of the blind when their plight was brought to the attention of its Poverty Committee. This Committee, formed in the winter of 1789-1790 and chaired by the Duc de la Rochefoucauld-Liancourt, had the vast program implied in its full title: Committee for the Extinction of Pauperism. Poverty, the Committee soon convinced itself, was often caused by sickness or physical handicap: the nation was obligated by its new mandate of fraternity and equality to aid those citizens whom nature had injured or short-changed at birth.

The rights and needs of the blind were argued before the Poverty Committee in conjunction with those of the deaf-mute. And indeed the history of efforts to come to their aid parallels that of the blind quite closely. A school to teach sign language had been founded in Paris by the Abbé Charles Michel de l'Epée (1712-1789) in 1755, the method publicized in his book, *The True Manner of Teaching the Deaf-Mute Confirmed by Experience* (1784).[22] In fact, Valentin Haüy had attended Michel de l'Epée's public sessions in the 1780s and learned much about teaching the handicapped—a lesson he warmly and gratefully acknowledged.[23] Since 1778 the government had been providing an annual stipend of 3,400 livres for the deaf-mute and, in 1785, offered the vacant Celestine Convent into which Michel de l'Epée's students moved in 1790, under the leadership of his successor the Abbé Roch-Ambroise Sicard (1742-1822).

Sicard had a knack for self-advertisement and was keenly aware of the need for publicity if his school was to survive in the Revolution. He therefore lost no time in bringing his needs to the attention of the legislature. On August 24, 1790, and again on January 27, 1791,[24] Sicard appeared before the National Assembly with several of his students to plead their cause. (The handwritten answers that the deaf-mute Massieu gave to the deputies' questions are still extant.)[25] The Poverty Committee was asked to investigate. On July 21, 1791, Prieur de la Marne presented a thorough report[26]—he had been eagerly coached by Sicard.[27]

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[22] Charles Michel, Abbé de L'Epée, *La véritable manière d'instruire les sourds-muets, confirmée par une longue expérience* (Paris: Nyon, 1784).

[23] In "Mémoires sur l'éducation des aveugles," *Mémoires lus* . . . (Paris: 1784), p. 42, Haüy acknowledged his indebtedness to the Abbé de l'Epée and his admiration for him. He added: "I have long and impatiently hoped to pay tribute to the Abbé de l'Epée and I am happy to do so under such favorable circumstances and before a group so capable of appreciating the appropriateness of the compliment." He repeated this tribute, even more explicitly, in his *Essai sur l'éducation des aveugles*, pp. 100-105.

[24] See *Archives parlementaires*, lère série, XXII, 506, and M. Tourneux, *Bibliographie de l'histoire de Paris pendant la révolution française*, 5 vols. (Paris: Imprimerie nouvelle, 1900), vol. 3, nos. 15,269-15,277.

[25] AN, série F^{15} 2584.

The possibility of merging the institution for the blind with that for the deaf-mute had first been mentioned in the Assembly on August 26, 1790, by Périsse du Luc, a member of the Poverty Committee.[28] In fact, the idea of associating a blind man with a deaf-mute so that they might, together, have eyes and ears, was not a novel one: Diderot mentions it in his "Letter on the Blind."[29] However appealing at first sight, the arrangement does not, upon reflection, appear promising. But reflection was evidently no longer possible in September 1791: the Constituent Assembly was facing adjournment and the hard-pressed deputies were glad to discover a device for saving money.

> Yes, gentlemen [the rapporteur exclaimed], the deaf-mute will understand the blind man's conversation and the blind will learn the sign-language of the deaf-mute. . . .
>
> Two souls locked into imperfect prisons will overcome the insuperable obstacle that nature has placed between them.[30]

This idyllic picture was rendered even more appealing by the proposal that funds for this National Institution for the Congenitally Blind [*aveugles-nés*]—as it was to be known—would be taken from those of the Quinze-Vingts, ". . . whose income shows an annual profit more than sufficient for the new establishment."[31] A budget of 13,900 livres for the new school provided for three teachers, two workshop foremen, two governesses, four music masters and eight blind assistants. Thirty scholarships of 350 livres were granted for the first year only. Haüy was confirmed as principal.

Studied carefully, the decree of September '91 reveals some strange and ominous details. To rely on the income of the Quinze-Vingts to finance Haüy's school was illusory since hospital wealth was shortly to be nationalized. In fact, the apposition of Haüy's modern concept of a school for handicapped children and of a medieval paupers' asylum indicated thoughtlessness on the government's part. What is worse, the Celestine Convent had long been designated as the locale into which the deaf-mute—and now the blind—should move.[32] A report in the National Archives reveals why:

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[26] "Rapport sur l'établissement de l'Institution des sourds-muets de naissance, fait au nom des Comités de l'extinction de la mendicité, d'aliénation des biens nationaux, des finances et de constitution, par M. Prieur, député de Châlons, département de la Marne, à l'Assemblée nationale, imprimé par les sourds-muets," in C. Bloch, et A. Tuetey, eds., *Procès-verbaux et rapports du Comité de mendicité de la constituante,* 1790-1791 (Paris: Imprimerie nationale, 1911), pp. 736-45. For discussion of this report by the Poverty Committee, see pp. 113-14, 120-21, 176, 188. See also *Archives parlementaires,* lère série, XXVIII, 249 and 491-92.

[27] See several letters, AN, série F^{15} 2584.

[28] Bloch et Tuetey, *Procès-verbaux,* p. 120.

[29] D. Diderot, "Lettre sur les aveugles," *Oeuvres,* vol. 1, p. 295.

[30] Bloch et Tuetey, *Procès-verbaux,* p. 756.

[31] *Ibid.,* p. 757. The decree of September 28, 1791, provided for the merger; see *Archives parlementaires,* lère série, XXXI, 532.

> The location of this vast establishment lowers its value. It consists of buildings that could easily be adapted to this use, that can only serve as a public establishment or would have to be sold for a pittance. . . .
>
> The architectural lay-out would permit the merger of the two establishments at little cost. They could be organized so as not to interfere with each other.[33]

Article 5 of the decree directs the principal to nominate his staff "jointly with the principal of the deaf-mute." While this might be interpreted to mean that each would choose his own employees, article 6 clinches the argument by decreeing that the two institutions would share one administrator and all expenses would be made jointly "so that the whole will form but one and the same establishment." The arrangement was predicated on harmonious collaboration between Haüy and Sicard: two men who were in fact lifelong deadly enemies.

A human drama of epic proportions underlies the relationship of Haüy and Sicard. Whether a preference shown by the gentle Abbé de l'Epée or some early episode unknown to history initiated a lifelong rivalry, we shall never know. What is certain is that their personalities clashed, and their religious and political attitudes diverged sharply. The scales were eventually tipped by their relative adeptness at making influential friends and gaining administrative and political power. Sicard turned out to be a master at self-advertisement and the aggrandisement of his fame and sphere of influence. And when the right moment came, he struck down Haüy with accuracy and ground him in the dust until his death. He survived him by a scant seven weeks. This dramatic duel will be worth exploring in a separate paper.

The merger initiated in September 1791 lasted over three years, so that some one hundred blind or deaf-mute boys and girls survived the height of the Revolution together, living at the Celestine Convent. The sparse available documents depict children clad in rags, ignored, near starvation. How Haüy and Sicard managed to get along can be inferred from recurrent reports that Haüy murdered Sicard during the September massacres of 1792.[34]

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[32] Louis XVI had assigned the building to the Abbé de l'Epée in 1785.

[33] AN, F^{15} 2584, dated October 15, 1790. This document is signed by Bailly, the Mayor of Paris, J. L. Brousse Desfaucherets, an assistant mayor, and A. C. F. Champion, an administrator.

In the spring of 1795 the unworkable merger had to be reversed. The deaf-mute moved to the former Magloire Seminary,[35] the blind to the Maison des Filles Ste. Catherine, rue des Lombards.[36] Provisions were made for 86 students (one from each department of France) with scholarships of 500 francs. The age of admission was extended to range from eight to sixteen, and the length of stay to eight years. Officially they were now known as the *"Institution nationale des jeunes aveugles-travailleurs."* The building rue des Lombards, in the bustling, densely populated center of Paris, seems a bad choice for handicapped children, many of them in poor health. Galliod, one of them, later commented: ". . . for almost a year we could not resume work, owing to the prevailing scarcity." [37] In contrast, the deaf-mute had been assigned a magnificent building, high on the Montagne Ste. Geneviève, with vast orchards and gardens, where they are established to this day. The blind would not be similarly housed until 1843.

Almost from the start, Haüy issued requests for more (or at least more usable) beds, mattresses, pillows, sheets, blankets, curtains, comforters, towels, and smocks. On June 15, 1796, he warned the Minister of Interior that "our housekeeper has neither bed nor sheets for any additional student you may send us. . . ." On May 31, 1797, he described the "shocking conditions prevailing in the establishment"; on July 13 he asked that the Minister stop all new admissions "until the Treasury pays the overdue scholarships, since no supplier will extend any more credit. . . ." At the same time, his balance sheet showed that from 1792 to 1796, at least, the work done by the blind as printers, teachers, and musical performers seems to have brought in enough money for the food, tools, and salaries of the staff. An "Observation" states: "The Nation has as yet furnished this establishment neither tools nor raw materials."[38]

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[34] The details of their quarrels during this period are explored in A. L'Esprit, "Valentin Haüy, Instituteur des aveugles et théophilanthrope: son séjour dans nos quartiers," *La Cité. Bulletin de la Société historique et archéologique du IVème arrondissement de Paris,* 1917, *64*:254-91; 1918, *65*:5-29, see especially chapter II; Sicard's version of the episode is given in A. Sérieys, ed., *La mort de Robespierre, tragédie en trois actes et en vers. Avec des Notes où se trouvent des particularités inconnues, relatives aux journées de septembre et au régime intérieur des prisons; notamment une relation complète de l'Abbé Sicard, et des anecdotes* (Paris: Monory, 9 Thermidor An IX— 1801).

[35] By the decree of January 5, 1795 (16 Nivôse, An III) the deaf-mute entered into permanent, legal possession of the former Magloire Seminary, which became and has remained the Institution nationale des sourds-muets, 254, rue St. Jacques.

[36] By the decree of July 28, 1795 (10 Thermidor, An III), the blind gained permanent occupancy of the "Catherinettes." The increase in the number and value of their scholarships and the raised salaries of the personnel matched those decreed for the deaf-mute six months earlier. Article XIII of the law provided for the admission of paying blind students. *Moniteur universel,* Convention nationale, Séance du 10 thermidor, an III.

[37] Galliod, *Notice historique,* [11].

Despite his destitution and the misery of "his children," Haüy attempted to run his school according to the By-Laws drawn up by the government: these called for rising at 5 a.m. (6 in the winter), washing and chores, prayer, breakfast; classes till lunch, one hour's recess; classes till dinner, recess and to bed at 10. The last two paragraphs of the By-Laws bear the unmistakable stamp of La Rochefoucauld-Liancourt, their author—ever humane, idealistic, and practical:

> 19. Each student must, before leaving the establishment, have learned a useful trade that will enable him to earn a living. . . . *The teachers should never forget that this is the essential goal of the . . . institution.* [italics mine]
> 20. (Teachers get two months' vacation.)
> 21. Teachers will see to it that their time of rest profits the institution by acquainting various parts of the kingdom with the special programs and explaining their purpose. They shall try to stimulate the creation of similar establishments in their native provinces.[39]

The school was well known throughout France and there certainly was no dearth of interest among visitors from abroad: in fact, Haüy's school for the blind evidently figured on the list of intelligent foreigners touring Paris at the turn of the century.[40] They usually attended the monthly public demonstration at both Sicard's and Haüy's schools, and their comparative judgments often favored the latter, since Sicard tended increasingly toward theory and metaphysics, and spoke at such lengths as to prompt usage of a new verb: *"sicardiser."* One visitor, in 1799, mentions "over sixty students" in Haüy's establishment, "with room for eighty; several are married."[41] A visiting German physician describes the "Café des aveugles" near the Palais Royal, where

> . . . under the arcades, six students from the Institute for the Blind of the well-known Haüy perform an instrumental concert every evening, for which the owner pays them a salary. These are not the usual itinerant performers; they are instrumental virtuosi

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[38] AN série F^{15} 2569.

[39] AN série F^{17} 1145.

[40] See, for example, Georgina Meinertzhagen, *A Bremen Family* (London: Longmans, 1912), p. 225, or A. F. Schweigger, *Ueber Kranken- und Armen-Anstalten zu Paris* (Bayreuth: Lübeck's Erben, 1809).

[41] A. G. Schwenger, *Mémoires sur les aveugles* (Amsterdam: Guérin, 1800), p. xviii, n.

> worthy of sympathy. They know the compositions by heart and interpret them with admirable feeling and precision.[42]

Haüy had given dignity and national stature to his handicapped wards.

In fact it is worth emphasizing that the French National Assembly was the first government thus to embark on a totally new venture in modern history, namely a national school designed for a specific handicap and open to poor boys and girls. It is of course true that the school was small, and would never accommodate but a small fraction of the estimated 20,000 congenitally blind French children in need of aid.[43] It was only a beginning but, equally important, a declaration of principle and of legal responsibility. The nation had established

> a veritable college devoted to the education of the congenitally blind who live in common, for a limited time, under general rules, and who, by the time they re-enter society after eight years at the Institution, have learned how to earn a living by their work.[44]

What discouraged Haüy was that the government was forever discussing principles rather than practical improvements. A nation-wide survey was launched to determine the number of needy blind and deaf-mute children. Only partial answers are extant. The famous Daunou Law of October 1795 integrated the special schools more firmly into the national system of public education, thus accentuating the distinction between Haüy's establishment and the Quinze-Vingts where three hundred persons, also blind, lived on public charity. Title III of the Daunou Law provides for Special Schools. Article I lists schools for astronomy, natural history, medicine, political science, music, etc. Articles II and III read:

> Art. II: There shall also be schools for the deaf-mute and for the congenitally blind.
>
> Art. II: The number and organization of each of these schools shall be determined by special laws, on the recommendation of the Public Education Committee.[45]

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[42] F. J. L. Meyer, *Briefe aus der Hauptstadt* [*Paris*] *und dem Innern Frankreichs,* 2 vols.-in-1 (Tübingen: Cotta, 1802), vol. 1, pp. 82-3.

[43] This seems to be the earliest estimate available. It is dated 1828 and assumes that 1 out of 1,600 French children was blind at birth or soon thereafter. It also argues that congenital or very early blindness is more frequent among the poor because they tend to reject vaccination. P.-A. Dufau, *Essai sur l'état physique, moral, et intellectuel des aveugles-nés, avec un nouveau plan pour l'amélioration de leur condition sociale* (Paris: Imprimerie royale, 1837), p. 150.

[44] Guillié, *Rapport fait à Son Excellence le Ministre Secrétaire d'état au Département de l'Intérieur sur l'état de l'Institution royale des jeunes aveugles, pendant les exercices 1815 et 1816* (Paris: Chanson, 1818), p. 10.

[45] *Moniteur universel.* Convention nationale. Séance du 3 Brumaire, An IV.

The principle of a national obligation to educate these handicapped children was thus transformed into law. In the course of the nineteenth century, the wide gap between the number of prospective blind students and of places available would gradually be narrowed and eventually—in the early twentieth century—closed.

But Valentin Haüy had made enemies in high places. At the beginning of the Consulate, on October 8, 1800, Lucien Bonaparte, the Minister of Interior, ordered a move that completely deprived Haüy's institution of its autonomy: he relegated Haüy's children to the Quinze-Vingts. Youngsters who had been taught a trade and given an education were now to be mingled with three hundred aged beggars.

Haüy protested passionately, in three "Notes" published on November 9, and addressed to his "adversaries who . . . propose to be his reformers." He asked:

> Have you opened a refuge to the unfortunate, as I have? . . . Have you gone into debt to support them in times of crisis? Do you pay them daily, as I do? After teaching your students, have you watched them enter the world, married them to spouses sensitive to their handicap? Have their children hugged yours in bursts of gentle friendship? The collaborators who share your vocation of teaching and comforting, are they your friends? Have you, Administrators, brought happiness to your charges? Have you found useful occupations for them? Has their work relieved the public treasury?
>
> Stop sending the Authorities *secret* memoranda that I have been unable to obtain *officially* in order to reply: publish them, over your signature . . .

And he ended, defiant:

> If I am wrong; if the aim is the destruction and not the reform of this Establishment that Frenchmen emulate and foreigners admire, then . . . I shall rebuild it (to the honor of France) with the aid of sensitive persons who will assist me. . .[46]

"Why has Citizen Haüy addressed a complaint to the Tribunate?" asked Lucien Bonaparte of his deputy, Jean-Antoine Chaptal, on December 23. For an answer he received a copy of Chaptal's stern letter addressed to Haüy that same day, expressing "surprise" at Haüy's public protest. Chaptal found this "highly objectionable," coming from a public servant, denied any intention of "destroying" Haüy's school, and warned that

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[46] V. Haüy, *Première (seconde, troisième) note du Citoyen Haüy, auteur de la manière d'instruire les aveugles, en réponse aux insinuations défavorables répandues dans la société, sur sa conduité politique* (Paris: Institut des aveugles travailleurs 19 Brumaire An IX), pp. 15-16. Haüy sent these *Notes* to all members of the government. Hence their presence in: Institut national des sciences et des arts. *Procès-verbaux. Pièces annexes.* Séance du 5 Nivôse, An IX.

Haüy's actions could only lead to "violence, disorder, and insubordination."

> I conclude, Citizen, [wrote Chaptal] by blaming and formally censuring the announcements and posters you have felt free to publish against the actions of the government.[47]

This letter arrived on Christmas Eve. Haüy did not have the heart to transfer his blind children to the East end of Paris, behind the forbidding walls of what used to be army barracks.[48]

When Napolean Bonaparte heard of the delay, he "expressed his displeasure in the liveliest manner," Chaptal wrote to the hospital administrators. "He wants the transfer completed within twenty-four hours." He added:

> Do realize, Citizens, that the First Consul is most insistent. . . . Give me the assurance that the transfer will be accomplished by the deadline he has indicated. . . .[49]

The frightened officials complied and on February 15, 1801, some sixty blind children were carted off to the Quinze-Vingts where no preparations had been made to receive them.

This shocking episode seems even stranger when contrasted with the humane and enlightened policies that the same Chaptal (who would become Minister of Interior on January 21, 1801) was even then pursuing in modernizing medical care in Parisian hospitals, organizing their administration along scientific lines, and revitalizing the nursing orders.[50] Reasons even more compelling than a temper tantrum of the First Consul must account for this cruel, reactionary policy.

The archives reveal three possible explanations: the first is an economy move. Indeed, a document had been circulating in the Department of Interior since at least 1797 (its arguments may even derive from a protest letter by J.-B. Avisse, the blind poet of Haüy's institution, dated August 28, 1795, when he had become temporarily disenchanted with the headmaster).[51] This document, originally composed by Rondelet and quoted in all subsequent ministerial versions, concludes that teaching the blind a trade was useless, once the student left the institution, because special tools were required; that blind persons need each other's company throughout life; and that Haüy's school should be incorporated into the Quinze-Vingts Hospital so that the Catherinettes might be sold—the proceeds to benefit the blind children.[52]

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[47] These various documents are to be found in AN série F^{15} 2576.

[48] The former Hôtel des Mousquetaires Noirs.

[49] See n. 47 above.

[50] See D. Weiner, "The French Revolution, Napoleon, and the nursing profession," *Bull. Hist. Med.*, 1972, *46*:274-306.

[51] This curious document is to be found in AN série F^{15} 2569; it contrasts with the tone of a short play written by Avisse in 1799 and the fact that he had been at the school all along. See J.-B. Avisse *L'Atelier des Aveugles-travailleurs. Scène destinée à faire l'ouverture de l'exposition publique de leurs travaux; paroles du Citoyen Avisse, aveugle, professeur de grammaire; musique du Citoyen Blaze. Précédée d'une notice historique et instructive sur l'Institution nationale des aveugles-travailleurs* (Paris: Aveugles-travailleurs, An VIII).

The second explanation is even less edifying and makes one wonder how paranoid Haüy really was in hinting at "enemies" and mentioning the Abbé Sicard in this connection.[53] The original transfer decree of October 8, 1800, contains a clause that denotes a shift in power and may thus well furnish an explanation for the Ministry's apparent vindictiveness:

> Art. I: The administrators of the Deaf-Mute will henceforth be responsible for the administration of the Quinze-Vingts.

The Abbé Sicard, Chaptal explained to the Consuls in a letter of January 3, 1801,[54] "was the first to solicit . . . the appointment of . . . administrators so that he could devote himself entirely to teaching. . . . The application of this arrangement to the Institute for the Blind seemed to conform to the principles of good administration." Was it Sicard who suggested that the same three-man Board of Trustees[55] now supervise both institutions? How much influence did he have over their decisions? The fact that in 1805 he was appointed a member of the enlarged five-man board makes one wonder.[56] What is certain is that Haüy now lost all administrative power, and that the government welcomed the opportunity of saving money, curbing Haüy's independence, and destroying what was deemed his subversive political activity.

While Sicard's influence and the desire for economy no doubt strengthened Chaptal's resolve, the third and main cause of Haüy's downfall was of his own making and so incongruous as to seem absurd. Haüy had been a founder of the revolutionary religious sect of theophilanthropy,[57] one of those civil and ethical cults patterned after Rousseau's patriotic festivals and Robespierre's worship of the Supreme Being.[58] To demonstrate his new-found republicanism and help finance his school, Haüy had arranged for his blind pupils to sing at public festivals (instead of masses), and participate in patriotic pageants, for example that of 20 Prairial, Year II (June 8, 1794), Robespierre's triumphal procession and celebration, carefully staged by Jacques-Louis David, the painter,[59] or the meeting of the Council of Ancients under the Directory, on July 14, 1799.

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[52] AN série F^{15} 2576. In a draft of this proposal corrected by François de Neufchateau, Minister of Interior under the Directory, the last provision is crossed off: income from the Catherinettes would go into the public treasury—which is what happened under the Consulate; only Louis XVIII would restore this income to the rightful owners.

[53] References to his "enemy" recur in Haüy's writings; see e.g. *Seconde note du Citoyen Haüy.*

[54] AN F^{15} 2576, letter dated 14 Nivôse, An IX.

[55] In the fall of 1800 the members were Mathieu-Montmorency, Lasalle, and J. L. Brousse Desfaucherets; the board was soon enlarged to include Ad. Duquesnoy and Bonnefous.

[56] Cf. *Almanach national de France* (Paris: Testu, 1806).

His public enthusiasm for Robespierre made Haüy suspect to the Directory and to Napoleon Bonaparte, although the young general attended at least one theophilanthropic meeting.[60] In fact, archival sources reveal that the sect was under police surveillance from 1797 on. Three manuscript reports by police spies, dated Prairial, Year V (June 1797), describe the "real" purpose of the Theophilanthropists:

> Our reports indicate that this attractive façade of pure and edifying morality hides a den of anarchy and terrorism. After the public speeches, the initiate retire to a private room where they applaud everything subversive and *whisper* to each other that some

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[57] Haüy is often described as the chief founder of this cult; see, e.g., L. M. de la Révellière-Lepeaux, *Mémoires,* 3 vols. (Paris: Plon-Nourrit, 1895), vol. 2, pp. 166-67. He was also a radical revolutionary, active at the height of the Terror in the Arsenal Section, and a supporter of Robespierre to the bitter end. The consistently biased French historian Albert Mathiez admires Haüy's "soul of an apostle, enchanted with ideals, incapable of self-doubt, at ease in vast, moral, and intrepid undertakings." *La théophilanthropie et le culte décadaire, 1796-1801; essai sur l'histoire religieuse de la révolution* (Paris: Durand, 1904), p. 90.

[58] The following pamphlet may help illustrate the mixture of secularism with aid to the blind, and Haüy's adherence to both the Christian and revolutionary calendars, and thus help document the hostility he later encountered from Napoleon and from the Restoration Monarchy: *Recueil des cantiques, odes et hymnes pour les fêtes publiques religieuses et morales des théophilanthropes ou adorateurs de Dieu et amis des hommes qui ont lieu les dimanches (vieux style) et les décadis à 11 heures du matin, rue Denis, No. 34, au coin de celle des Lombards* (Paris: An V de la république, 1797 de l'ére ancienne. Imprimé dans la même maison par les Aveugles travailleurs et s'y vend à leur bénéfice). [Bibliothèque nationale, L^{d} 188 8A.]

[59] [J.-B. Avisse et al.], *Recueil des morceaux de musique qui seront exécutés par les aveugles-travailleurs à la Fête de l'Etre Suprême le 20 Prairial, l'An II de la république française une et indivisible: Hymne à l'Eternel; Hymne à L'Etre Suprême; Invocation à la Raison. Paroles du Citoyen Avisse, Musique du Citoyen Mathieu. Petit Choeur Chanté par Neuf Aveugles de l'Institut National à la représentation de la Fête du 10 août sur le Théatre de l'Opéra National.*

[60] *La visite de Bonaparte aux théophilanthropes; sa prière à l'Eternel, et le détail de la liste de tous ceux qui assisteront au banquet civil donné à Bonaparte.* [n.p., n.d.] Bibliothèque nationale, L^{b} 42 1702 8°.

> *firm* step will be taken. They trample on the Constitution of the Year III, call each other good brothers, 3 by 3, that is to say good masons. . . .
>
> Observers are following the Society closely . . .[61]

No one was surprised by the decree issued by the Minister of Police, Joseph Fouché, on 12 Vendémiaire, Year X (October 4, 1801):

> The Societies known by the name of Theophilanthropists shall no longer meet in public buildings.
>
> The Ministers of Finance and General Police shall enforce this decree.
>
> Bonaparte.[62]

That the real target of this measure was not a small dissident sect but its founder was clear to most observers. The Abbé Grégoire, for example, writing in 1828, stated:

> Haüy, of course, returned to the Christian convictions that he should never have abandoned;[63] but his temporary desertion caused his dismissal as director of an establishment that his talents and dedication had created. His writings, classics in their field, speak for him. Should temporary mistakes have served as a shield for the hatred of his enemies or his rivals?[64]

Haüy would go down fighting. His first note of protest arrived in the office of Bouret, the administrator, one week after the transfer of the children, now called "Aveugles de 2ème classe." They had been denied the right to an outing.[65] Haüy duly nominated the skeleton staff that the new regulations allowed,[66] while at the same time suggesting that his former associates join a voluntary mutual-aid society and contribute to its funds by musical performances and teaching. This was immediately vetoed. Thereupon he refused to admit unscheduled visitors to his classes, explaining:

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[61] AN série F⁷ 7338. The Society's published literature is innocuous to the point of boredom. See, e.g., F. M. Poultier, *Discours décadaires à l'usage des théophilanthropes* (Paris: Bureau de l'Ami des Louis, An VI [1798]). Chapter 5, "A la reconnaissance," features Rousseau, the Abbé de l'Epée, Haüy, and Lavoisier.

[62] [J. Fouché], *Lettre du Ministre de la police générale de la république qui fait défense aux Théophilanthropes de s'assembler davantage dans les endroits ci-dessus indiqués par ordre du gouvernement.* Arrêté du 12 Vendémiaire, An X. Bibliothèque nationale, L^{d} 188^{33}

[63] Grégoire knew that Haüy, who had died in 1822, had received last rites.

[64] M. Grégoire, *Histoire des sectes religieuses qui sont nées, se sont modifiées, se sont éteintes dans les différentes contrées du globe, depuis le commencement du siècle dernier jusqu'à l'époque actuelle,* 6 vols. (Paris: Baudouin, 1828) vol. 1, bk 2, p. 379.

[65] Note dated 6 Ventôse, An IX, Archives, Institution nationale des Jeunes Aveugles. The author wishes to thank Madame Petit, Administrator of this Institution, for permitting access to its library and archives.

[66] AN série F^{15} 2576.

> I stopped this infraction and showed the relevant rules to those public servants for whose supervision I am responsible . . . including the concierge who had brought these strangers to the classrooms yesterday afternoon.[67]

This incident made Bouret complain to Chaptal. Haüy's next move provoked a letter from the Minister to the Trustees:

> Citizen Haüy is plotting actively: he sent a detachment of his blind followers to Citizen Bénézech [Pierre Bénézech (1775-1802) a State Councillor] to implore pity concerning the bad treatment to which he is being subjected. . . .
>
> Watch that establishment very closely; maybe a show of harshness will be indicated.[68]

By August, Bouret was complaining again, and on December 29, 1802, Chaptal forwarded to an administrator a protest by the inmates of the Quinze-Vingts, "complaining that they are deprived of their freedom and compelled to do useless work." [69] Chaptal now encouraged Bouret to take "the necessary steps to maintain order." [70] It is hardly surprising that on February 16, 1802, the position of chief instructor was abolished. Chaptal's letter to Bouret could not have been more terse:

> Please inform Citizen Haüy of this decision and convey my regrets that the need for economy precludes retaining the post that had been entrusted to him.[71]

A remnant of decency impelled the government to award the patron-saint of blind children an annual pension of 2,000 francs.

Haüy's institution would have been destroyed had it not been for Dr. Paul Seignette[72] who succeeded Bouret as administrator of the Quinze-Vingts in 1802. He was appalled to see the children's classes reduced to two hours a day. "The rest of the time," writes Galliod, "the students were busy spinning wool in the textile shop set up within the walls of the hospice." [73]

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[67] Archives, Quinze-Vingts, B 109, dossier 6715.

[68] *Ibid.* Letter dated April 18, 1801.

[69] *Ibid.*

[70] Archives. Quinze-Vingts. B 109, dossier 6715.

[71] *Ibid.*, dossier 6722.

[72] Seignette received an M.D. degree from the Faculty at Montpellier in 1791, worked as secretary to the French ambassador in Madrid from 1798 to 1800, and then in the archives of the Ministry of Interior until his appointment as Administrator of the Quinze-Vingts in August 1802. He retired from that post in 1824 and died in Paris in 1835. (AN Série F^{15} 2575.)

[73] Galliod, *Note historique* . . ., [12]. This retrospective report was presented to Haüy when he returned to France in 1817 by Galliod, "in the presence of his associates who confirmed every detail."

> . . . things went from bad to worse [comments an historian]. The textile shop, which had been declining, had to be closed in 1805, and the blind youngsters were then left almost completely idle.[74]

An attempt to put them to work, after 1806, in the tobacco or the weaving shop at the Quinze-Vingts was a failure because they were only allowed to assist the grown-ups (and thus did not master any skill), and because they were not paid.

Seignette understood Haüy's purpose and, with the help of Galliod, the blind music teacher, and of François Lesueur, treasurer since 1794, he worked out a new curriculum that the administration approved in 1806.

> . . . he frequently visited the classes and workshops and promoted the study of all subjects. His presence encouraged the students who valued his advice. . . . The best among them went to him in the evenings when his office turned into a classroom. He helped them eagerly and lent them the classics. One would meet them carrying books back and forth under their arm, in their hands, in their pockets. . . .[75]

He reanimated the orchestra, the choir, the printshop. He seems to have completely overshadowed Bertrand, the new headmaster appointed to succeed Haüy. Guadet describes Bertrand as a man "completely incapable of even understanding his task. He was a poor organizer and as poor a teacher. . . . He started to study Latin and to teach it: soon many students dropped the course. . . . He used the sheet music printed by Haüy's method to light fires: 'What good is all that?' he would say."[76] In a letter to a friend, Haüy later wrote that Bertrand was to be pitied, since "his blind pupils made fun of him because he had so little talent."[77]

Paul Seignette, this attractive, humble man, virtually unknown to history, becomes even more significant when one realizes that he is not the only experienced, kind-hearted, and enlightened administrator who represents the vital link between the charitable Christian care of the old régime, the lay republican welfare system, and the Restoration monarchy. He resembles Bernard Hombron, the administrator of the Foundlings' Hospital, and Madame Lachapelle, the chief midwife at the Hôtel-Dieu, both of whom were instrumental in the creation of the Maternity Hospital in 1797. And if we are to believe Philippe Pinel, he could never have succeeded as psychiatrist-in-chief at Bicêtre, had it not been for the

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[74] J. Guadet, *L'Institut des jeunes aveugles de Paris, son histoire, et ses procédés d'enseignement* (Paris: Thunot, 1849), p. 55.

[75] J. Guadet, *L'Institut des jeunes aveugles,* p. 58.

[76] *Ibid.,* pp. 51, 59, 60, n.1.

[77] To Fournier-Clairon, February 28, 1818. Archives, Institut national des aveugles.

"governor" of the psychiatric ward, Jean-Baptiste Pussin. Seignette assured the survival of Haüy's work.

As for Haüy himself, "disgusted by the state of things," writes Galliod, he withdrew to the private school for the blind that he had founded, rue Ste.-Avoye, at the time of the forced transfer to the Quinze-Vingts. This "Musée des aveugles" was, by both friendly and hostile accounts, a failure. True, he equipped the school well, at his own expense, going heavily into debt (as evidenced by the numerous receipts for repayments, preserved in the Quinze-Vingts archives). He trained two star pupils during this time, Alexandre Fournier, who would be his companion for the next twenty years, and Alexandre Rodenbach, later a well-known writer and public official in Belgium.[78] This was also the period when he became interested in the recently invented semaphore and in telegraphic messages adapted to the needs of the blind.[79] His fame among foreigners persisted, witness, for example, Dr. Joseph Frank from Vienna who wrote:

> Before visiting the Quinze-Vingts, I wished to acquaint myself with the method of teaching the blind. Therefore I first went to a different institution, namely a private one directed by Monsieur Haüy (brother of the famous mineralogist).[80]

The following episode, recorded by Dr. Friedrich Meyer from Germany in 1801-2, seems worth quoting at length, since it confirms documentary evidence:

> Bonaparte recently visited the Quinze-Vingts together with Minister Chaptal. Haüy was only informed of his presence belatedly. The students were dispersed, Bonaparte saw little of their work nor of the school and its beneficial influence on these unfortunates. Minister Chaptal led him to a few blind children and prompted them to sing a song of praise to the Consul. Surprised and moved, Bonaparte voiced this humane suggestion: "the teacher should use music as a means of consolation and entertainment for the aged blind inmates."
>
> "Your humanitarian wish is already met," answered Haüy who had arrived unob-

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[78] Cf., e.g., A. Rodenbach, *Lettre sur les aveugles pour faire suite à celle de Diderot* (Bruxelles: n.p., 1828), and *Les aveugles et les sourds-muets. Histoire—Instruction—Education—Biographies* (Bruxelles: Slingeneyer, 1855), and *Notice sur la Phonographie ou langue musicale télégraphique* (Inventée par V. Haüy et perfectionnée par Sudre) (n p., n.d.).

[79] V. Haüy, "Moyens nouveaux propres, si l'on ne s'abuse, à étendre et peut-être même à perfectionner le service du télégraphe," [1803], apparently published in St. Petersburg as *Memoire historique abrégé sur les télégraphes* (1810). [Letters of inquiry on this subject have remained unanswered.]

[80] J. Frank, *Reise nach Paris, London, und einem grossen Theile des übrigen Englands und Schottlands in Beziehung auf Spitäler, Versorgungshäuser, übrige Armen-Institute, medizinische Lehranstalten und Gefängnisse,* 2 vols. (Wien: Camesianische Buchhandlung, 1804), vol. 1, p. 102.

> served. "On days of rest, passages from the best traditional moral teachings are being read here, interspersed with appropriate songs, thus fulfilling the dual purpose of cheering the blind of all ages and instructing them and their wives and children."
>
> "I did not know of your visit ahead of time," Haüy continued as the Consul was leaving. "What you have seen is only a small part of the activities I have been teaching for twenty years, according to a plan elaborated thirty years ago. I would request that you grant me a few moments of your leisure some day, so as to submit the children's attainments to your examination and judgment."
>
> "Gladly," was Bonaparte's friendly answer, "make an appointment with the Minister."
>
> The opportunity to inform the Consul of the Institution's agony was gone and would not recur soon, since it depended on Chaptal's agreement.[81]

Official ostracism, the lack of funds, popular ignorance of the new hope for blind children worked against Haüy. His detractors depict him as an incompetent administrator, now encumbered by his uneducated and careless second wife. He was unhappy and restless. Rodenbach alludes to "poverty and domestic trouble."[82] He was desperate enough to write to Chaptal on December 20, 1803, asking for attention.[83] The letter seems to have remained unanswered. A letter to Champagny, Chaptal's successor, dated December 22, 1804,[84] refers to Haüy's project of organizing one of his well-known demonstrations at the Tuileries Palace, for Napoleon and Pope Pius VII who was then visiting Paris. The Pope went to the Quinze-Vingts instead; Haüy's initiative remained fruitless.[85] In a letter of October 1805 to Nicolas Froschot, the Prefect of the Seine, Haüy offered to demonstrate his new telegraphic experiments.[86] Nothing seems to have succeeded. Gradually one wonders whether Haüy was so wrong to talk of "years of persecution" by his "enemy."[87] In the end he accepted the invitation of Tsar Alexander I to establish an institution for the blind in St. Petersburg.

After long and careful preparation, Haüy left for Russia on May 2, 1806, accompanied by his wife, their son Juste, and Alexandre Fournier. He entrusted his school to an Alsatian named Heilmann who had married the blind widow of Avisse, the school poet.[88] At the invitation of King

[81] F. J. L. Meyer, *Briefe aus der Hauptstadt*, vol. 2, pp. 191-92.

[82] A. Rodenbach, *Les aveugles et les sourds-muets*, p. 30.

[83] AN série F^{17} 1145.

[84] Archives, Quinze-Vingts, B 109 dossier 6715.

[85] Perhaps the clearest indication of how low he felt his prestige to have sunk is this postscript: "I am the brother of the Abbé Haüy, the mineralogist, member of the Institute."

[86] Archives, Institution nationale des jeunes aveugles.

[87] See, e.g., an autobiographic fragment called "Mémoire historique," AN F^{7} 4306.

[88] J. A. Zeune, *Belisar: Ueber den Unterricht der Blinden* (Berlin: In der Blindenanstalt, 1821), p. 42.

Frederick-William III he stopped in Berlin, demonstrated his method, and met young Johann-August Zeune who would soon open a Prussian school for the blind. Haüy thought it politic to pay his respects to the Comte de Provence, then residing at Mittau and indeed the future Louis XVIII promised to revive Haüy's institution upon regaining the throne.

In September 1806, Haüy reached St. Petersburg where he would struggle for eleven years to run an establishment that never really existed. The meagre documentation[89] suggests that Tsar Alexander I obeyed a generous impulse when he invited Haüy to Russia, or perhaps a wish to publicize his enlightened policies. Little attention and less money came Haüy's way, once he had arrived in the Russian capital. At his departure, the Tsar awarded him the Order of St. Vladimir, fourth class. Almost seventy years old, Haüy made valiant efforts to earn a respectable living, ". . . without neglecting my main occupation," he wrote to his son Juste in 1820.[90]

> I used my free time to teach deaf-mutes, stammerers, and retarded children; then, having addressed to His Majesty the Emperor a printed memoir on the telegraphic art of which he graciously accepted the sponsorship, I taught soldiers how to transmit words by my method. . . . finally I composed little poems [for various Frenchmen] that they addressed to Their Imperial Majesties and to various Russian noblemen. . . .[91]

One imagines the hardships and humiliations of an aged impecunious refugee, in wartime.

After Waterloo and the Congress of Vienna, Haüy longed to return home. He wrote to the Duc de Richelieu, President of the Council of Ministers, recalling Louis XVIII's promise at Mittau, and asking the French government for three favors:

> 1. his reintegration into the Royal Almanach, as Royal Interpreter, and author of the *Essay on the Education of the Blind* and their honorary instructor;

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[89] The only printed document concerning this epoch is A. Skrebitzki, *Valentin Haüy à St. Pétersbourg* (Paris: Noizette, 1884). The author refers to material in the Russian State Archives, but inquiries have so far remained fruitless.

[90] Juste Haüy stayed on in St. Petersburg after his father returned home, married a Mlle de Forville and had a son, Valentin, born in 1822. In 1853, Dufau, then Director of the National Institution for the Blind, petitioned Napoleon III to grant young Haüy a job, seeing that he bore a "famous name." AN Série F ^{1d} II H2.

[91] This letter was first published in "Pièces justificatives," Maxime Du Camp, *Paris, ses organes, ses fonctions, et sa vie dans la seconde moitié du 19ème siècle,* 3e ed., 6 vols. (Paris: Hachette, 1875) vol. 5, pp. 370-75.

> 2. the permission for Alexandre Fournier to re-establish and perfect Haüy's method at the Institution for the Blind in Paris;
> 3. Louis XVIII's sponsorship of the 2nd edition of Haüy's Essay.[92]

Two trustees of the National Institution, Malus and Garnier, had already approached the Minister of Interior, asking that Haüy be re-instated as First Instructor of the Blind. The answer, dated May 13, 1814, is clear: "The motives that determined M. Haüy's retirement seem entirely opposed to his reinstatement in the post he once held." [93] It may be relevant to recall that the other two trustees were Démeunier and the Abbé Sicard.

On August 25, 1817, Haüy returned to Paris in the company of his daughter and her husband, Juste Nicolas Vuillemot, and his star pupil, Alexandre Fournier. "Aged, disappointed, and bitter," he went to live with his brother, the famous Abbé, at the Jardin des Plantes. "The news of his return filled his former students with joy," wrote Galliod; "they all hurried to visit him. . . . Whenever his frailty permitted, he came to visit his students at the Quinze-Vingts and it was an ever-renewed pleasure for him to listen to the music they performed at mass." [94] He hurried to "present his respects to His Majesty Louis XVIII at the Tuileries Palace on December 22, 1817, but malevolence seems to have thwarted the outcome." [95] In a letter of February 28, 1818, he wrote that he wished to have engraved on his tombstone these concluding words of the *Life of St. Athanasius:*

> And he experienced all his life that the wicked will never forgive his victim for not having expired under his first blows.[96]

On August 22, 1821, a ceremony in his honor was held at the Institution des aveugles. One of the young participants was thirteen-year-old Louis Braille who was to make Haüy's institution world-famous. It was to be Braille—not Haüy—who would be buried at the Panthéon with national honors, eventually, on June 22, 1952.[97]

Louis XVIII restored the autonomy of Haüy's institution on February 8, 1815, providing it with the income from the "Catherinettes" and fifty thousand francs a year, and stipulating that a new locale be found.[98] Dr. Guillié, the new monarchist principal, took credit for renovating the school, even though he but continued Haüy's work. A comparison of Guillié's *Essay on the Education of the Blind*[99] and of his *Report*[100] published in 1818, with Haüy's book of 1786 and with subsequent developments, afford the modern historian a basis for evaluating efforts made in France toward the education of the blind from the point of view of personnel, school policy, curriculum, teaching methods, buildings, and budget.

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[92] Archives, Institution nationale des jeunes aveugles. The letter is dated March 21 (April 2), 1817.

[93] Archives, Quinze-Vingts, B 109, dossier 6715.

[94] Galliod, *Note historique,* [22].

[95] AN, série F^7 4306.

[96] P. Henri, *La vie et l'oeuvre de Louis Braille, inventeur de l'alphabet des aveugles (1809-1852)* (Paris: Presses universitaires de France, 1952), p. 26, n. 2.

[97] Haüy died on March 19, 1822 and was buried at the Père Lachaise cemetery. A statue of the two famous brothers was erected at St. Just in 1903.

The successive directors[101] and teachers[102] who put pen to paper have little good to say about each other, often for political reasons. Thus Henri, the twentieth-century biographer of Braille, dismisses Haüy's book for its "thin and superficial notions," continues to condemn the author for his switch from Louis XVI to Robespierre, but grudgingly admits that "in those troubled times his work survived solely because he was adaptable and versatile."[103] Henri also condemns Guillié for refusing to invite the aged, disillusioned, paralytic and deaf Haüy for a parting visit to his school. Guillié's successor, Dr. Pignier, remedied this inhumane attitude just in time, seven months before Haüy's death.

Dr. Guillié directed the medical service of the institution, with the help of an assistant, Dr. Nanche. Pignier was also a physician, this choice being doubtless a carry-over from the fifteen years at the Quinze-Vingts Hospital. In 1837, Pignier dissociated the jobs of director and physician, thus freeing the school from the implication of being a hospital. Guillié insisted on a strict separation of the sexes and appointed the adamant

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[98] Archives Quinze-Vingts, 6715 bis.

[99] Guillié, *Essai sur l'instruction des aveugles ou Exposé analytique des procédés pour les instruire* (Paris: Imprimé par les aveugles, 1817). This book was re-issued twice, in 1819 and 1820, and translated into English by R. Philips (London: 1819) as *An Essay on the Instruction and Amusements of the Blind,* and into German by J.-G. Knie, the director of the School for the Blind at Breslau, as *Versuch über den Unterricht der Blinden* (Breslau: 1820).

[100] Guillié, *Rapport fait à son Excellence le Ministre.*

[101] Guillié, appointed in 1814, was dismissed in 1821. Dr. Pignier, headmaster of the School from 1821 to 1840 was succeeded by P.-A. Dufau, 1840-1855, Boné de Verdier, 1855-1863, and the Baron de Watteville, 1863-1871.

[102] Outstanding among these were J. Guadet (see his *L'Institut des jeunes aveugles de Paris*), P. Henri, the biographer of Haüy and Braille, and Dufau, assistant to Guillié before he became headmaster. His book is unique in that it expresses real affection for the young students. (See his *Essai sur l'état physique, moral et intellectuel des aveugles-nés*). See also E. Guilbeau, *Histoire de l'Institution nationale des jeunes aveugles* (Paris: Berlin, 1897).

[103] P. Henri, *La vie et l'oeuvre de Louis Braille,* pp. 21 and 24.

Mademoiselle de Cardeilhac[104] as headmistress for the girls. In 1817 he wrote:

> Formerly marriages between blind persons were favored, but couples are no longer allowed to live at the Institution, since they are a continual source of misunderstandings and trouble.[105]

Guillié's policy of a strict separation between boys and girls prevails at the National Institution to this day. It differed radically from the easygoing family atmosphere that Haüy made prevail among his "children." In the nineteenth century, the blind youngsters led a Spartan existence, in which periodic supervised outings, and summer vacations if the family could afford them, seem to have been the only lighter moments.

Upon his appointment in 1814, Guillié called for "moral reorganization" of Haüy's institution. This he achieved by dismissing forty-three students and clearing the library shelves of books "offensive to modesty and religion."[106] He replaced these with history, geography, and mathematics. Possibly he was advised in this house-cleaning by his "excellent friend, the Abbé Sicard,"[107] who had welcomed Haüy's replacement in 1802 by Citizen Bertrand,

> . . . an enlightened, moral, and religious man who did not think it necessary to consult these unfortunates before restoring the principles of morality of the Catholic religion, so necessary to the disinherited. . . .[108]

Guillié established seven workshops and classrooms, an infirmary and servants' quarters. Classes were held from 7 in the morning till 8 at night, with schedules that favored a mixture between intellectual and manual training, though all students undertook both. Henri later characterized this regime as "desperately hard."[109] He also pointed out that Guillié made no progress in teaching the blind how to write, it being virtually impossible for a person sightless from birth to trace out the loops and arabesques that constitute legible handwriting. In geography, Guillié used relief maps like Haüy's, and identical looms and tools in the workshops. In music, he discontinued teaching notation, and relied on auditory memory.[110] He favored the so-called "Lancaster System" of mutual teaching and admired Pestalozzi's method.

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[104] "Zélie de Cardeilhac," wrote Haüy to Alexandre Fournier's father, "came to see for herself in my cell how I give initial reading lessons. She saw him, *that old imbecile,* of whom she said: *he is no longer good for anything.*" [sic]. Letter dated February 28, 1818, Archives, Institution nationale des aveugles.

[105] Guillié, *Rapport,* p. 17.

[106] Guillié, *Rapport,* p. 38.

[107] Guillié, *Essai sur l'instruction des aveugles,* pp. 29-30.

[108] Sicard, *Cours d'instruction d'un sourd-muet,* p. lii, n.

[109] Henri, *La vie et l'oeuvre de Louis Braille,* p. 14.

The Institution had invested a great deal of money and effort in the traditional way of printing with embossed letters, but finally had to yield to the superior method Braille devised in 1829.[111] It would take the ingenious Louis Braille (1809-1852)[112] over ten years to obtain the adoption by his own school of his new system of writing. He transformed a complex arrangement of punched dots devised by a retired artillery captain, Charles Barbier de la Serre [113] into a simple system based on a six-dot "cell" to denote the alphabet and numerals. Abroad, in England for example, "Braille" was welcomed as a substitute for various local methods by which ". . . a blind pupil who had been taught at Manchester was unable to read a word written by a blind pupil who had been taught at Bristol." This "absurd state of affairs" was ended in about 1870 by Dr. Armitage, a physician gone blind, who founded the English Braille publishing house, now the Royal National Institute for the Blind. "Thanks to the doctor's energetic lead and far-sightedness [!]," writes Braille's best English biographer, "Braille's alphabet gradually came to be adopted in nearly every country in the world." [114] In the second half of the nineteenth century, when international affiliation became fashionable, charitable organizations followed the trend and blindness became a major international concern. Surely the Braille method sped this development.[115]

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[110] *Ibid.*, pp. 22-24.

[111] L. Braille, *Procédé pour écrire les paroles, la musique, et le plain-chant au moyen de points, à l'usage des aveugles et disposés pour eux* (Paris: Imprimé à l'Institution nationale des aveugles, 1829). The musical notation apparently is based on the system that Jean-Jacques Rousseau had invented in the 1740s. Cf. Henri, *La vie et l'oeuvre de Louis Braille,* p. 59.

[112] The best biography of Braille is by Pierre Henri, himself a teacher at the Institut national des jeunes aveugles. (See n. 96 above.) The best English study is the brief but sound: N. Wymer, *Louis Braille* (London: Oxford University Press, 1957), in the series *Lives of Great Men and Women.* See also the illustrated and well-documented accounts: J. Roblin, *Louis Braille* (London: Royal National Institute for the Blind, [1952]); and H. Lende, "A Servant of Humanity," *Library Journal,* 1952, *77:* 1027-30.

There is an abundance of sentimental trash, such as: J. A. Kugelmass, *Louis Braille: Windows for the Blind* (New York: Julian Messner, 1957); or Y. Pitrois, *Trois lumières dans la nuit: Valentin Haüy, Louis Braille, Maurice de la Sizeranne* (Strasbourg: Edition des sources, 1936).

[113] Ch. Barbier, *Instruction familiaire d'écriture nocturne, pour apprendre de suite aux aveugles à consigner leurs idées sur le papier, recueillir celles d'autrui, lire leur écriture et celle des autres aveugles, sans que l'on soit pour cela obligé de leur enseigner la figure des lettres, etc.* (Versailles; n.p., 1830). Barbier's method was discussed by the Institut de France on December 1, 1823, the reporters being Ampère and Lacépède.

[114] Wymer, *Louis Braille,* pp. 29-30.

While Braille was a young student at the Institution, the crucial change in the students' lives was no doubt their removal from the Quinze-Vingts Hospital. Dr. Guillié achieved the transfer, in early 1816, to the former St. Firmin monastery, a considerable improvement, even though his successors accused him of having chosen the new building badly, motivated by the desire to have complete separate facilities for boys and girls.[116] He undertook important structural changes, installing dormitories with large windows, courtyards for exercise, and two large dining halls. Even so, St. Firmin was cramped and humid, and when two medical inspectors visited in 1821 and 1828, they found that ". . . mortality is high among the students. Their health is endangered by that building." [117] Eventually their report reached the Chamber of Deputies and the needed credits of 1,600,000 francs were finally voted, but only after the Romanticist poet Lamartine, a deputy, had given this report:

> I went yesterday to visit the Institution for Blind Children . . . that narrow, dirty, dark building, those hallways cut in two to form veritable cells dubbed "studios" and "classrooms," those many twisted, worm-eaten staircases that seem a challenge to blindness rather than designed for unfortunates who must rely on touch to find their way. . . . If the whole Chamber had visited that place, the whole Chamber would vote the credits requested by the Minister.[118]

The law for transfer to a new building was finally passed on July 18, 1838, and, in 1843, Haüy's Institution moved into its present palatial home, 56, Boulevard des Invalides. Next door, rue Duroc, rises the privately endowed Institution Valentin Haüy pour le Bien des Aveugles, with classrooms and workshops, counselling and placement services, and a sales organization for the goods manufactured by the blind, as well as a famous Braille Library [119] founded in 1907, and a Museum.[120]

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[115] As was so often the case, concern for the deaf-mute went hand in hand with aid to the blind. See, for example: P.-A. Dufau, "Aveugles et sourds-muets," *Réunion internationale de charité* in *Annales de la charité,* 1855, *11*:583-98. See also: A. Bébian, ed., *Journal de l'instruction des sourds-muets et des aveugles* (Paris, 1826 ff.), and Abbé Carton, ed., *Le sourd-muet et l'aveugle: Journal mensuel* (Bruges, 1837 ff.).

[116] The separation decree that allocated the new building was dated February 8, 1815. The decree of February 20, 1816, provided for the move.

[117] P. Henri, *La vie et l'oeuvre de Louis Braille,* pp. 15-17.

[118] *Moniteur universel,* May 15, 1838.

[119] In 1952 it held 20,000 books, in 130,000 volumes. See N. H. Graham, "The Braille library," *Library Journal,* 1952, *77*:1030-33.

[120] Even before the First World War, many groups were formed to aid the blind. See, for example: J. V. Laborde, *La société d'assistance pour les aveugles et ses créations. La clinique*

Under the Third Republic the blind reaped great benefits from having been wronged under the old régime. Indeed, Louis XVIII had immediately reinstated the Grand Almoner as guardian of the Quinze-Vingts[121] while the Minister of Interior took charge of the Jeunes Aveugles. Charitable Christians now resumed their donations. Many years later, when the office of Grand Almoner had long been abolished, he re-entered this story in an unexpected manner. In 1879, the Society to Aid the Blind was lobbying for higher government subsidies. In the legislative debates, a novel argument was advanced by the friends of the blind: the state owed them a *debt*, because, on the eve of the Revolution, in 1779, their buildings rue de Rivoli and Castiglione had been sold at a huge profit by their official protector, the Grand Almoner, Cardinal de Rohan (of "Queen's Necklace" fame). They were transferred outside Paris and never adequately reimbursed. Historians among the blinds' supporters even located a phrase in Finance Minister Necker's famous *Budget* of 1789, stating that the Hospital of Quinze-Vingts was due 250,000 francs "to constitute perpetual annual payment for *part* of the sales price of the land and buildings formerly owned by the Hospital."[122] By 1903, this obligation had, in the Chamber, become an "I.O.U." and two years later François Mitterand called it a "public debt—none more sacred."[123]

By then, of course, the law of July 14, 1905, on *Aid to the Aged, Infirm, and Incurable* had begun to change the official attitude of the government. It raised from 3,000 to 6,000 the number of blind persons eligible for government help. The 1909 budget of the Ministry of Interior for the blind carried two special provisions: one to create more workshops and studios—a very old preoccupation—the other for medical research to *prevent* blindness—a new and exciting development.

The attitude of the government toward the blind has thus changed

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nationale ophthalmologique des Quinze-Vingts et l'Ecole Braille (Paris: Goupy, 1894); A. Lucis, *Valentin Haüy et son oeuvre; Les jeunes aveugles: Ce qu'on peut faire pour eux et pour préserver les enfants de la cécité* (Clermont· Daix, 1891); L. M. M. B. Monier de la Sizeranne, *La question des aveugles en 1910. Notes et documents* (Caen: Poisson, 1910); and *idem, Le Valentin Haüy: Revue française des questions relatives aux aveugles, 1883-1893* (Paris, 1883 ff.). See also: Ministère de l'Intérieur. Institution nationale des Jeunes Aveugles. *Compte-rendu de la Fête du Centenaire de la fondation de cette institution par Valentin Haüy* (Paris: Noizette, 1884).

[121] By letter of June 22, 1814. AN, série F^{15} 2575.

[122] [Anon.,] *Société d'assistance pour les aveugles. Son histoire, 1879-1915* (Paris: Imprimerie nouvelle, [1916]), p. 83.

[123] *Ibid.*, p. 85. In 1899, Waldeck-Rousseau, then a senator, declared in the presence of the President of the Republic, Emile Loubet, that ". . . the blind of France the creditors of the State (Très bien! Très bien!) and that the Cardinal de Rohan's sale of their property entailed ". . . a natural obligation contracted by the government."

since the days of King St. Louis, largely because of the French Revolution. Whereas in the middle ages a saintly king endowed a hospital for the blind out of Christian charity, in modern times the government recognizes the *right* of the blind to help. This right was claimed for them by the eighteenth-century reformers. The French Revolution—following a familiar pattern—made no original contribution *except* for the momentous step of giving national, even universal significance to a particular act. The French Revolution took an eighteenth-century creative idea and transformed it into a national law, saying, essentially, that we are our blind brother's keeper.

And Valentin Haüy, founder of the School for the Blind, should be venerated, together with Philippe Pinel, proponent of "moral treatment" for the insane, and the Abbé Michel de l'Epée, founder of the School for the Deaf-Mute, as one of the three French eighteenth-century patron-saints of care for the handicapped.

PHYSIOLOGICAL EXPLANATION IN MAGENDIE'S MANIFESTO OF 1809 *

WILLIAM R. ALBURY

In his paper on "The Philosophical Background of Magendie's Physiology,"[1] Owsei Temkin called attention to the striking fact that although François Magendie (1783-1855) joined with most of his contemporary physiologists in embracing the ideas of the philosophy of *idéologie* on such questions as the relationships between matter and life or between body and mind, he nevertheless differed sharply from them in his conception of the status of physiology as a developed science. While most of his immediate predecessors and older contemporaries in France anticipated the perfection of physiology within their own lifetimes, Magendie himself regarded that science as "still in its infancy."[2] The reason which Magendie adduced for this state of underdevelopment was that the generally-accepted manner of explaining the phenomena of life was scientifically unsound. Even "in the best works," he wrote in 1816, "we find [physiology] founded upon mere suppositions, to which every one attaches at pleasure the numerous phenomena of life, thinking that he is explaining them satisfactorily."[3]

Very early in his career, in 1809, Magendie published a paper entirely devoted to this question of physiological explanation, entitled "Some General Ideas on the Phenomena Particular to Living Bodies."[4] Here, in what has been called "the manifesto of the new physiology,"[5] Magendie detailed the theoretical position to which he adhered for the rest of his life. It is on the basis of this paper, then, that I shall attempt to illuminate some of the major differences which separated Magendie's physiology from all previous work in that field.

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[1] Owsei Temkin, "The philosophical background of Magendie's physiology," *Bulletin of the History of Medicine,* 1946, *20*:10-35.

[2] François Magendie, *Précis élémentaire de physiologie* (2 vols.; Paris, 1816-1817); translated by J. S. Forsyth as *An Elementary Summary of Physiology* (London, 1825), I, ix.

[3] *Ibid.,* viii.

[4] *Idem,* "Quelques idées générales sur les phénomènes particuliers aux corps vivans," *Bulletin des sciences médicales de la Société médicale d'émulation de Paris,* 1809, *4*:145-170.

[5] Joseph Schiller, *Claude Bernard et les problèmes scientifiques de son temps* (Paris: Éditions du Cèdre, 1967), 44.

Magendie opened his essay with a thoroughly traditional statement on the logical form of a developed science: "The character of a certain and perfected science is that it has as its base only a small number of principles to which a great number of facts are easily connected."[6] The physical sciences, he continued, such as chemistry, physics and above all astronomy, exhibit this logical form; but physiology cannot lay claim to such a distinction, in spite of the impressive achievements of the last fifty years—that is, since about the time of Albrecht von Haller. To remedy this deficiency, Magendie said that he hoped to set physiology on the same path as that which the physical sciences were following, by proving two points: that the current explanation of the phenomena of life was unsound in several respects; and that all these phenomena could be explained in the same way.

On the first of these points, Magendie criticized "the physiologists"—he did not single out any individual in particular—for invoking a number of special, vital "principles," "properties," "powers" or "forces" to explain the phenomena of life. These explanatory entities—chiefly "sensibility" and "contractility," as variously subdivided by different authors—he attacked on the following grounds.

First criticism: while some of them were said to be inherent in all parts of all living organisms, others were restricted to certain parts of certain classes of organisms. For example, the properties of "organic sensibility" and "insensible contractility" were attributed to all parts of all living organisms and were supposed to be responsible for the nutrition of each of those parts; on the other hand, only certain parts of animals—the voluntary muscles—were endowed with the property of "animal contractility." But, Magendie argued, a vital property, rightly so-called, should characterize life in all its manifestations, and thus such restricted properties as "animal contractility" ought not to be admitted.

Second criticism: even though the properties of "organic sensibility" and "insensible contractility" are not affected by the first criticism—because they are attributed to all parts of all organisms—they nevertheless are wholly imperceptible and thus purely hypothetical. Hence, reasoned Magendie, they offer no real explanation and provide no positive knowledge of the phenomena.

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[6] Magendie, "Quelques idées," 145.

Third criticism: "animal" or "voluntary contractility," in addition to its restricted occurrence, is not really a property of the parts which it characterizes but rather a function—the result of the combined actions of those parts, namely the brain, the nerves and the voluntary muscles. Similarly the so-called "animal sensibility" is a function resulting from the combined actions of the sense-organ, the nerves and the brain.

Fourth criticism: finally, the perceptible but involuntary contractility, such as that exhibited by the heart, ought not to be attributed to a special property, Magendie held, because it is simply the product of the action of the muscle in question, just as bile is the product of the action of the liver, and so on. This concept of "action" will be made clearer as we consider Magendie's proposed alternative explanation of the phenomena of life, to which I shall turn in a moment. But first a word about the identity of "the physiologists" whom Magendie was criticizing.

Following the lead of Claude Bernard's eulogy of Magendie,[7] nearly every subsequent commentator has described the 1809 paper as an attack upon Xavier Bichat (1771-1802) or upon Bichat and his followers. Now it is true that Magendie's criticisms applied to the doctrine set out by Bichat in his *Physiological Researches on Life and Death* and his *General Anatomy*; and it is also true that Magendie specifically mentioned Bichat in his text.[8] However Magendie also referred, directly or indirectly, to at least four other prominent physiologists, none of whom could be characterized as followers of Bichat. They were: François Chaussier, whose *Synoptic Table of the Characteristic Properties and Principal Phenomena of the Vital Force* Magendie cited in his footnotes; [9] Paul-Joseph Barthez, the second edition of whose *New Elements of the Science of Man* appeared in 1806; [10] Charles-Louis Dumas, whose *Principles of Physiology* also had a second edition in 1806; [11] and Balthasar-Anthelme

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[7] Claude Bernard (1813-1878), *Leçons sur les effets des substances toxiques et médicamenteuses* (Paris: Baillière, 1857), 7.

[8] Marie-François-Xavier Bichat, *Recherches physiologiques sur la vie et la mort* (Paris, an VIII—1800); and *Anatomie générale, appliquée à la physiologie et à la médecine* (4 vols.; Paris, an IX—1801) For Magendie's references to Bichat, see "Quelques idées," 164-165.

[9] François Chaussier (1746-1828), *Table synoptique des propriétés characteristiques et des principaux phénomènes de la force vitale* (Paris, s.d. 2° éd.; Paris, an IX), cited by Magendie in "Quelques idées," 150n, 151n, 153n. Another of Chaussier's tables, probably the *Table synoptique des sensations* (which I have not been able to examine) is also cited by Magendie, "Quelques idées," 168n.

[10] Paul-Joseph Barthez (1734-1806), *Nouveaux éléments de la science de l'homme* (Montpellier, 1778. 2° éd., 2 vols.; Paris, 1806). Magendie refers to Barthez's "force de situation fixe" in "Quelques idées," 161.

[11] Charles-Louis Dumas (1765-1813), *Principes de physiologie* (Paris, 1801 [vols. 1 and 2

Richerand, whose *New Elements of Physiology* reached a fourth edition in 1807.[12] We are not dealing, then, with a "Revolt from Bichat," as J. M. D. Olmsted melodramatically characterized it; [13] rather, the 1809 paper was criticizing a whole tradition of physiological explanation reaching back to the work of Haller on sensibility and irritability. These criticisms, as I hope to show, were the result of fundamentally different presuppositions from those upon which the work of the older physiologists was based. So different were they, in fact, that Magendie simply could not understand much of what these older physiologists were trying to accomplish.

We turn now to a consideration of Magendie's own proposals for the interpretation of vital phenomena. First, he claimed, all of these phenomena could be reduced to two principal ones: nutrition and what he called "vital action." By nutrition he meant the assimilation of nutritive matter by each "living molecule" of an organism, and the excretion by these molecules of matter no longer able to make up part of their composition. As to the cause of nutrition, Magendie wrote: "No analogy, not even a probable one, has yet been found between the play of ordinary chemical affinities and the nutritive movement; that is why this movement has always been considered—and still is today—as dependent upon a particular cause which, like planetary and molecular attraction, is unknown in its nature but manifest in its effects. This particular cause has received many denominations, but the best which has been given is that of *vital force*." [14]

The other category, under which Magendie grouped all life-phenomena other than nutrition, was that of vital action. By this term Magendie meant the particular activities which each of the organs exhibits: contraction in the muscles, production of bile in the liver, and so on. These actions, Magendie wrote, are to be attributed to the same vital force as was nutrition, the difference in effects arising from the fact that nutrition is a process of decomposition and recomposition occurring in each living molecule regardless of its situation, while vital action is a consequence of

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only]. 2° éd., 4 vols.; Paris, 1806). Magendie cites Dumas' "force assimilatrice" and "force de resistance vitale" in "Quelques idées," 161.

[12] Balthasar-Anthelme Richerand (1779-1840), *Nouveaux éléments de physiologie* (Paris, 1801. 4° éd.; Paris, 1807). Magendie's discussion of the explanation of nutrition in "Quelques idées," 150-151, exactly describes that found in Richerand's textbook (2° éd.; Paris, 1802), 508 ff.

[13] J. M. D. Olmsted, *François Magendie, Pioneer in Experimental Physiology and Scientific Medicine in XIX Century France* (New York: Schuman's, 1944), 19 ff.

[14] Magendie, "Quelques idées," 149-150.

the organization of these molecules into groups which modify the vital force by their structures.

What Magendie proposed, then, was an interpretation of life based upon a single vital force which was responsible both for the changing chemical composition of each living molecule (nutrition) and for the changing spatial relationships between these molecules (action). But since the molecular movements of nutrition and those which result in vital actions are imperceptible, they cannot be the objects of a science. Instead, Magendie recommended beginning "the study of physiology at the instant when the phenomena of living bodies become appreciable to our senses," and carrying it out by "performing experiments and . . . describing important phenomena with exactitude." [15]

Now what is new in all this? Let me say first of all, emphatically, that it is not the call for experimentation. Although the Montpellier physiologists whom I mentioned above—Barthez and Dumas—were not experimentalists, the representatives of the Paris school—Chaussier, Bichat, Richerand—encouraged, performed and relied upon experiments to provide them with physiological data. It was not the lack of experiments but the fact that they had all been carried out within the explanatory framework of "sensibility" and "contractility" which made Magendie write that "most physiological facts need to be verified by new experiments." [16] How could this same framework appear to the older physiologists to be firmly based and soundly built—indeed to be the very construction of nature itself—while to Magendie it seemed only to be an "unsteady scaffolding" which his new interpretation of life "would permit . . . to collapse of its own weight"? [17]

We should remember first that the theory of sensibility and contractility flourished within an epistemology based upon the analysis of sensations. Just as all knowledge—indeed all mental faculties—arose from sensations variously transformed, so too were the physiological processes regarded as dependent upon sensibility. The same epistemology which authorized the analysis of the living body into anatomical elements—organs, tissues, molecules—also produced an explanation of the processes of these elements which transferred to the physiological level the same priority which sensation enjoyed at the mental level.[18]

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[15] *Ibid.*, 170.

[16] *Ibid.*, 147.

[17] *Ibid.*, 169.

[18] *Cf.* the remark by Pierre-Jean-Georges Cabanis (1757-1808) in his *Rapports du physique et du moral de l'homme:* ". . . la sensibilité physique est la source de toutes les idées et de

To the older physiologists the theory of sensibility and contractility was as soundly based as the Newtonian theory of gravitation; and they did not hesitate to draw this comparison explicitly.[19] Indeed sensibility and contractility functioned as *forces* and were regarded as the physiological equivalents of affinities in chemistry and attraction in astronomy. Magendie's later objection to this claimed equivalence—that Newton had discovered not the fact of attraction but the mathematical law by which it operates [20]—would have carried no weight with the older physiologists since in their epistemology a certain type of linguistic ordering which, to oversimplify we may call "classification," had the same status in the qualitative sphere as mathematics had in the quantitative: both were applications of a more general concept of analysis.[21] This view was made possible by the absolutely fundamental presupposition that the observable order of phenomena perfectly corresponds to and simply represents the hidden order of causes—hence the unprejudiced and skillful analysis of the phenomena into the least elements given to the senses necessarily yields knowledge of the true system of nature. For physiology this meant the progressive subdivision of "sensibility" and "contractility" on the basis of perceptible differences in their modes of manifestation, thus assuring the precision of mathematics in the study of these essentially non-quantifiable phenomena.

This, then, was the epistemological foundation of the theory of sensibility and contractility, without which that theory was truly nothing but an unsteady scaffolding. But at the end of the eighteenth century the epistemology of the analysis of sensations was replaced by another one, of a wholly different configuration, which brought with it an organic conception of life incompatible with the analytic interpretation of living phenomena. It was this new conception of life, for which the most promi-

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toutes les habitudes qui constituent l'existence morale de l'homme: Locke, Bonnet, Condillac, Helvétius, ont porté cette vérité jusqu'au dernier degré de la démonstration. Parmi les personnes instruites, et qui font quelque usage de leur raison, il n'en est maintenant aucune qui puisse élever le moindre doute à cet égard. D'un autre côté, les physiologistes ont prouvé que tous les mouvemens vitaux sont le produit des impressions reçues par les parties sensibles: et ces deux résultats fondamentaux, rapprochés dans un examen réfléchi, ne forment qu'une seule et même vérité." (*Oeuvres philosophiques de Cabanis,* Claude Lehec et Jean Cazeneuve, éds. (2 vols.; Paris: Presses Universitaires de France, 1956), I, 165).

[19] Thomas S. Hall, "On biological analogs of Newtonian paradigms," *Philosophy of Science,* 1968, *35*: 6-27.

[20] Magendie, *An Elementary Summary,* I, 11.

[21] *Cf.* for a discussion of this point, Michel Foucault, *The Order of Things* (New York: Pantheon, 1970), 71 ff.

nent spokesman was the zoologist Georges Cuvier,[22] that underlay all Magendie's criticisms of the older physiologists and all his own physiological proposals.

Magendie's high regard for Cuvierian comparative anatomy was exhibited in his 1809 paper when he wrote, "I consider this part of zoology to be one of the most remarkable and most satisfying things which has ever been accomplished." [23] Nevertheless, it may at first seem paradoxical to link Cuvier with Magendie the relentless experimenter, in view of the former's reputation as an opponent of vivisection. However, Cuvier's early opposition to vivisection was limited to "certain experiments upon living bodies" in which the researcher first removed some vital organ from an animal and then attempted to deduce from the phenomena of the animal's death the function which the affected organ performed in the normal state.[24] Thus when Magendie, J.-J.-C. Legallois and Pierre Flourens subsequently developed techniques for vivisectional experimentation upon the neuro-muscular system, the success of which depended not upon the death of the animal but upon keeping it alive for as long as possible, Cuvier actively encouraged these researches and furthered the careers of those who pursued them.[25]

The new conception of life, which both Cuvier and Magendie shared, had as one of its central features the elevation of functions to a status of priority over anatomical structure. Whereas the older tradition had assumed a strict one-to-one correspondence between structure and function —each part having its special use and, conversely, each function having its special organ [26]—at the beginning of the nineteenth century "function" became understood as a general result to be attained by a variety of possible anatomical apparatuses. Respiration, for example, as Cuvier noted in his *Lessons on Comparative Anatomy*, was a function which different classes of beings performed "by means of organs so varied that their structures offer no points in common." [27] For Cuvier this concept of function provided the key for a new system of classification, while for Magendie it supplied a new organizing principle for the study of physiology.

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[22] *Cf. ibid.*, 263 ff.

[23] Magendie, "Quelques idées," 160.

[24] "All the parts of a living body are connected; they can only act insofar as they all act together. . . . The machines [i.e., living bodies] which are the object of our researches cannot be dismantled without being destroyed; we cannot know what would result from the absence of one or several of their wheels [i.e., organs] and consequently we cannot know what part each of these wheels plays in the total effect." Georges Cuvier (1769-1832), *Leçons d'anatomie comparée* (5 vols.; Paris: 1800-1805), I, v.

[25] Michael Gross, "Cuvier and a Transformation in Physiology in France," read at the Joint Atlantic Seminar in the History of Biology, Boston, Mass., April 7, 1973; and Robert M. Young, *Mind, Brain and Adaptation in the Nineteenth Century* (Oxford: Clarendon Press, 1970), 57-60.

[26] *Cf.* Claude Perrault's programmatic statement of 1667 for the Académie royale des sciences: "à chaque organe trouver sa fonction, pour chaque fonction trouver l'organe." (Quoted in Schiller, *op. cit.*, 26.)

Under the analytic conception of life the vital phenomena manifested by the organism as a whole were considered simply as the sum of the phenomena of the more or less autonomous elements of the living body. Thus, for example, Théophile de Bordeu (1722-1776) regarded each organ as having a life of its own.[28] Bichat pushed the analysis further and isolated twenty-one "simple tissues"—each having a life of its own—which, by their various combinations, comprise the different organs. For Bichat the study of these tissues (general anatomy) was an essential introduction to the study of the organs (descriptive anatomy), while this latter undertaking was only the first step in the study of the functions performed by these organs.[29]

For Magendie, however, it was not the activities of the anatomical elements—tissues or organs—which ought to occupy the focal point of physiology, but rather the "true function," which he defined as "the common end of the actions of a certain number of organs." [30] Hence Magendie's experiments were typically organized so as to take one of these functions as a starting point and to trace it back to all the organs which contributed to it.[31] Some of his earliest experimental work, for example, concerned the function of absorption and was designed to show that it was not solely by means of the lymphatic system that this function was carried out in mammals, but also by means of the venous system.[32] The experimental technique employed by Magendie in this study was precisely that of Bichat's work on asphyxiation, but the conception of the relationship between vital functions and anatomical structures was entirely new. And it was this new conception which liberated physiology from anatomy.[33]

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[27] Cuvier, *Leçons d'anatomie comparée,* I, 34-35

[28] Hall, "On biological analogs," 17.

[29] Bichat, *Traité d'anatomie descriptive* (5 vols., Paris, ans X-XII—1801-1803), I, xiv.

[30] Magendie, "Quelques idées," 164-165. Contrary to Schiller's suggestion (*Claude Bernard,* 28), this definition was not new among physiologists, nor did it represent a break with Bichat. In the first lesson of his physiology course for the year X (1801-1802) Bichat propounded the following: "Une organe ne remplit pas une fonction à lui seul; une fonction est donc le résultat du travail de plusieurs organes. Il y a dans l'économie animale un grand nombre d'organes qui appartiennent à plusieurs appareils." (MS. lecture notes of L. J. Jusserandot, collection of Medizinhistorisches Institut der Universität Zürich.) And in a volume published the same year he defined "appareil" as "un assemblage de plusieurs organes concourant à une fonction," adding shortly afterward that "la nature fait souvent servir les mêmes [organes] à des fonctions toutes differentes." (*Traité d'anatomie descriptive,* I, xi, xix.) However, Bichat, as seen above (footnote 29), retained the view that the study of the anatomical elements and their properties had to precede that of the functions they carried out, a position which Magendie rejected.

[31] *Cf.* Schiller, "The Genesis and Structure of Claude Bernard's Experimental Method," in *Foundations of Scientific Method: The Nineteenth Century,* Ronald N. Giere and Richard S. Westfall, eds. (Bloomington. Indiana University Press, 1973), 151. Even Magendie's famous work on the roots of the spinal nerves, which led to his controversy with the

Now with the primacy of functions over structures came also the primacy among those functions of nutrition, the basic process by which the life of the organism is maintained; both Cuvier and Magendie accorded nutrition this priority of place. Secondly, neither Cuvier nor Magendie could conceive of applying the term "sensibility" in a meaningful way to any process unaccompanied by conscious perception; in other words, they interpreted "sensibility," which was a causal term for the older physiologists, as the function of sensation, which was a result. Thirdly, they shared an exactly parallel interpretation of "contractility" as the function of contraction. Finally, the elimination of sensibility and contractility as causal concepts necessitated a new explanation of the variety of particular processes carried out by the bodily organs.

Magendie's solution to this last problem was that the activity of an organ is a consequence of "the nature, the disposition and the mode of union of the living molecules—in a word, their organization."[34] Such an explanation was simply the transference to the molecules, in their relation to the activity of the organ, of the same principle which Cuvier applied to the organs, in their relation to the activity of the organism as a whole; and Magendie explicitly drew this comparison: "It is, in my opinion, extremely probable that if our means of investigating anatomical structures are developed in the future to a high degree of perfection, we will be able to account for the nuances of life as well as we now account for its great differences. With what marvelous facility do the zoologists succeed in determining, in a rigorous manner, all the phenomena of the

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Scottish anatomist Charles Bell, was conceived in this fashion. Magendie was attempting to determine the role which these roots played in the overall function of sensation rather than studying the properties of the nervous tissue itself.

[32] Magendie, "Mémoire sur les organes de l'absorption chez les mammifères, lu à l'Institut le 7 août 1809," *Journal de physiologie expérimentale et pathologique*, 1821, *1*:18-32

[33] Discussion by Foucault in "Georges Cuvier, Journées d'études," *Revue d'histoire des sciences*, 1970, *23*:74-75; *cf.* remarks of M. D. Grmek and Georges Canguilhem, *ibid.*, 31-32.

[34] Magendie, "Quelques idées," 159.

life of an animal, simply by considering one of its organs—a tarsal bone, for example!"[35]

In Magendie's 1809 paper, then, both his criticisms of the older physiologists and his own proposals for physiological explanation were based upon a new conception of life—one which he shared with Cuvier (and many others) and which was incompatible with the late eighteenth-century interpretation of life. Thus for Magendie physiology was still in its infancy because the collapse of the older theory of sensibility and contractility left only isolated facts which needed to be integrated into the framework of the new conception of life and to be supplemented by a vast amount of empirical work carried out within that framework before any comprehensive explanatory theory could be erected. That is why, in 1816, Magendie compared the status of physiology to that of the physical sciences at the time of Galileo and Bacon rather than at the time of Newton.[36] And it is also why Magendie's lifelong experimental inquiry was a search for exact descriptions of physiological phenomena rather than an inquiry into the nature of the unknown force behind these phenomena.

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[35] *Ibid.*, 159-160.

[36] *Idem, An Elementary Summary*, I, viii.

LISTER, HIS BOOKS, AND EVOLVEMENT OF HIS ANTISEPTIC WOUND PRACTICES*

OWEN H. WANGENSTEEN

and

SARAH D. WANGENSTEEN

Over a period of many centuries dating back to John of Arderne (14th c.) Britain can boast a long unrivalled list of surgeons who left impressive and durable stamps upon their discipline. In the midst of the 19th century came that celebrated scientist-surgeon, Joseph Lister, the greatest of them all. As a child Lister had performed dissections on small animals and fish and decided upon a career in surgery.[1] This was many years before the introduction of anesthesia (1846); he obviously had a very stout heart.

Lister entered University College, London, in 1844 at the age of 17. Here he came under the stimulating influence of enthusiastic and spirited teachers who sharpened his motivation. Notable were the ophthalmologist-physiologist, Thomas Wharton Jones, the great physiologist, William Sharpey, and the chemist, Thomas Graham. As a medical student, Lister displayed a keener interest in research than in his assigned studies. His microscopic research upon the histology of the iris, published in 1853,[2] won praises from his peers and gave great promise of future academic distinction. Upon completion of his medical studies, Lister served as house physician at University College under Walter H. Walshe, and later as house surgeon under John Erichsen, well-known surgical author of that day. By 1852, Lister had already garnered membership in the Royal College of Surgeons of England.

At the suggestion of Sharpey, Lister went to Edinburgh (1853) to spend a few months with the master surgeon, James Syme. He stayed long enough to marry Syme's eldest daughter, Agnes, in April 1856, and to succeed to the Professorship of Surgery at the University of Glasgow in January 1860.

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[1] Sir Rickman J. Godlee, *Lord Lister* (London: Macmillan, 1917), p. 14.

[2] Joseph Lister, "Observations on the contractile tissue of the iris," *Quart. J. Micr. Sc.*, London, 1853, *1:* 8-17; Lister, *Collected Papers*, 2 vols. (Oxford: 1909), vol. 1, pp. 1-8.

It was during his nine-year professorship at the Glasgow Royal Infirmary that Lister began his life's work and mission upon antiseptic surgery. When Syme retired from his Edinburgh professorship because of illness in 1869, Lister was elected his successor. When summoned to King's College Hospital and Medical School in London in the Fall of 1877,[3] Lister felt an obligation to accept, probably motivated by the belief that his presence in Britain's principal metropolis would hasten acceptance of his teachings and practices of antiseptic surgery. Lister retired as Professor of Surgery at King's in 1892 and withdrew from the surgical staff a year later.

Lister's developmental years as a surgeon were spent at University College and at Edinburgh. During his Glasgow years (1860-69) Lister began the productive work that foreshadowed a brilliant career. His professorial years at Edinburgh (1870-77), while struggling for acceptance of his practices, were undoubtedly the most pleasant; the years at King's College Hospital (1877-92) on Portugal Street in Lincoln's Inn Fields, though characterized in the beginning by active opposition within his hospital, as well as in the larger arena of London hospitals, ultimately brought him great fame and a multitude of cherished rewards. No surgeon in history has won so many plaudits of acclamation and hosannas of praise.

The standard reference on Joseph Lister, apart from his collected writings, is Sir Rickman J. Godlee's *Lord Lister,* published in 1917, five years after Lister's death. Godlee, himself a distinguished surgeon,[4] was Lister's devoted nephew and a sensitive, careful historian of his uncle's life and work. He drew on his personal knowledge of Lister as well as on family letters, diaries, and other documents, the whereabouts of most of which are unknown today, though Godlee did secure Lister's "personal papers" (not letters) for the Royal College of Surgeons of England in 1912.[5]

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[3] On February 10, 1877, King's talented operating surgeon, William Fergusson, died. The plan to invite Lister to King's College undoubtedly originated in the minds of Sir William Bowman, distinguished ophthalmologist and scientist, and George Johnson, senior physician at King's. The information unfortunately leaked before the official invitation was made. In the March 10 issue of the *Lancet* for 1877, Thomas Bryant wrote that Professor Lister had very gracefully refused an offer which had yet not been extended him and, added Bryant, Lister is reported to have said clinical surgery in London schools is to a great extent neglected and, when compared with his own system, is "a mere sham." Lister's remarks had been made in Edinburgh in response to a popular student request that Lister remain there. Despite this unfortunate chain of events, when a formal offer came from the Board of King's College, Lister gladly accepted. He was a man of determined purpose.

[4] On a visit to London in September 1973, privileged to examine Lister memorabilia still in private hands, we were struck by the accuracy of Godlee's referenecs and quotations from letters and diaries of the Lister family.

Perhaps the time has come for a definitive study of Lister, with a critical appraisal of his contribution and a less familial approach to the story. A modern biographer will find the problem compounded by the circumstance that the fame of his subject has turned contemporary records into museum pieces. Moreover, there is no central record nor site for Lister materials and memorabilia. Many items are undoubtedly in private hands, held as collector's items.

Search for Lister's Library.

For some years the authors have pursued a neglected source of likely information concerning Lister's interests and activities which, we believe, may add pertinent material to known biographic accounts. Our search concerns the fate of Lister's library, a quest we undertook lightheartedly sixteen years ago without finding any clues on occasional visits to England and Scotland. We knew that books were an important source of pleasure in the Lister household. In telling of Agnes' contribution to the happiness of the Lister's thirty-seven years of marriage, Godlee explains that she was her husband's constant companion on his travels and his patient amanuensis throughout the tedious hours of composition of his surgical papers: "After dinner, if the day's work was over, or even if it was intended to devote the night to experiments, Lister was packed away on the sofa and covered with a shawl, while she read aloud some book, serious or light, read him in fact to sleep as a preparation for bed or for the laboratory." [6]

Believing that the books of so illustrious a surgeon-scientist could not have disappeared completely, we intensified our search in the summer of 1971. Queries dispatched to the librarians of such institutions as the University of Glasgow, Edinburgh, and London; the Wellcome Institute of the History of Medicine; and the Royal Colleges of Surgeons of England and of Edinburgh provided little information.[7]

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[5] Letters from William R. LeFanu, distinguished retired Librarian of the Royal College of Surgeons of England, 8 Dec. 1971; from Ian F. Lyle, Senior Assistant Librarian, R.C.S. (E.), 3 May 1973; and from Eustace Cornelius, Librarian, R.C.S. (E.), 9 May 1973, to Owen H. Wangensteen.

[6] Godlee, *Lister,* pp. 523-524.

[7] The many responders included: Eustace Cornelius, Librarian, R.C.S. (E.), 29 July, 5 Aug. 1971; Sir Charles Illingworth, 10 Nov. 1971; E. R. S. Fifoot, Librarian, Edinburgh

Visits to the distinguished collections of the Royal Society of Medicine and to the Royal College of Physicians in London also proved unrewarding for our search at the time. No trace of Lister's library was found in the towering stacks of Edinburgh's Royal College of Physicians, where James Y. Simpson's library is so carefully preserved, or among the treasures of the Royal College of Physicians and Surgeons of Glasgow.

Everywhere we met with dedicated librarians, anxious to provide help, though usually convinced that the search was futile. Through a succession of fortunate circumstances, important segments of the problem eventually were resolved. To our own astonishment as well as to the Librarians, we have succeeded in putting together a chronicle of Lister's books. The search also provides some sobering reflections on the fate of libraries in general.

Letters to relatives proved unproductive. A grand nephew of Lister replied that he doubted Lister ever had a considerable library at his London residence, 12 Park Crescent, for his father, a frequent visitor there, had never mentioned one.[8] It was, however, a large and commodious house, and a room off the ground floor lined with books could readily have escaped notice.

Lister's will, signed June 26, 1908, is an interesting, typed document, not referred to by his biographers, though Lister's generous legacies to various London hospitals and surgical institutions are known.[9] Characteristic was his request that his name should in no way be associated with these sums. Moreover, in willing his diplomas, medals, and caskets to the University of Edinburgh,[10] he declared that the University Authorities should be "prefectly [*sic*] at liberty to dispose" of them, having the medals melted down or the diplomas destroyed, if they so desired. Sometime after his younger brother Arthur died,[11] Lister added a codicil em-

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University Library, 29 Oct. 1971; Joseph W. Scott, Librarian, University College London, 7 Oct. 1971; Eric Gaskell, Librarian, Wellcome Library, 7 Oct. 1971; Charles Marmoy, Librarian, Thane Library, University College London, 29 Sept. 1972; R. O. MacKenna, Librarian, University of Glasgow, 5 Apr. 1973; and Sir John Bruce, 30 Aug. 1972.

[8] Letter from Cornelius to OHW, 29 July 1971, with enclosed copy of letter from W. A. Lister to Cornelius, 24 Nov. 1971; W. A. Lister to OHW, 22 May 1973.

[9] Lister left £10,000 to each of the following: The Royal Society, King Edward's Hospital Fund, King's College Hospital, and North London and University College Hospital, and £20,000 to the Lister Institute of Preventive Medicine. The will may be seen at Somerset House, London.

[10] These are beautifully arranged in a large case at the entry to the University Library.

[11] A celebrated botanist (1830-1908), whose distinguished work won him membership in the Royal Society (1898). The brothers were on closest terms and were frequently together on trips. Arthur had been named a trustee in the will.

powering the remaining trustees, three nephews, to dispose of his "personal effects not mentioned" in the will, taking only into consideration what they believed to be Lister's personal wishes.[12] Though books are not mentioned in either the will or the codicil, they could have been implied in the latter. Lister died three years later, February 10, 1912. His wife, Agnes, had died in 1893; they had no children.

Following up on a lead from Eustace Cornelius[13] with the helpful guidance of Charles Talbot of the Wellcome, in September 1971 we finally found a few books, approximately 30 titles, once belonging to Lister, in the library of the King's College Hospital, London. These had been given to the library by a physician of the staff, Dr. Harold Wiltshire, upon his retirement in 1935.[14] It was a composite assortment of medical and classical works, including many Sydenham Society publications. Lister's signature and annotations appeared in a number of the volumes but no Lister bookplate.[15] Perhaps such ostentation would have been contrary to Lister's Quaker heritage.[16]

However, and most important, each of the books bore a label stating it came from the library of the late Lord Lister and had been purchased from Henry Sotheran and Company of 140 Strand and 43 Picadilly.

The *Lancet* had run a notice of the Sotheran sale of Lister's books on February 15, 1913.[17] The Sotheran Company today has no record of the sale, all such information having been destroyed by two World Wars.[18] Fortunately the British Museum has the Sotheran catalogue.[19] It lists 482 separate entries, several of which represent more than one volume; many are carefully annotated in the catalogue by the bookseller.

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[12] "I leave all my personal effects not mentioned in my will to my trustees to dispose of as they may judge best by personal distribution to themselves or others or by sale taking into consideration with regard to such personal effects what they may have reason to believe to be my personal wishes with regard to any of them." (7 Dec. 1909).

[13] Letter from Cornelius to OHW, 29 July 1971.

[14] The entries indicated that Wiltshire made his purchases on Feb. 7, 1913. He retired to the Isle of Wight in 1935.

[15] Dr. Charles Newman, Harveian Librarian of the Royal College of Physicians, London, shares our conviction that Lister did not have a bookplate (letter from Newman to OHW, 22 Mar. 1973).

[16] However, the Quaker William Penn had an elaborate bookplate bearing a coat of arms, said to be the first bookplate in America (Roland G. Curtin, "The bookplates of physicians . . . ," *Int. Clin.*, Philadelphia, 1910, s. 20, vol. 2, pp. 238-239).

[17] We are grateful to Dr. Douglas Guthrie of Edinburgh for the *Lancet* notice (letter from Guthrie to OHW, 23 Aug. 1972).

[18] Letter from L. G. Simpson of Henry Sotheran Ltd. to OHW, 20 Sept. 1971.

[19] Henry Sotheran & Co., *Catalogue* (*no. 733*) *of Second-Hand Books . . . including . . . the remaining portion of the Library of the late Lord Lister* (1913).

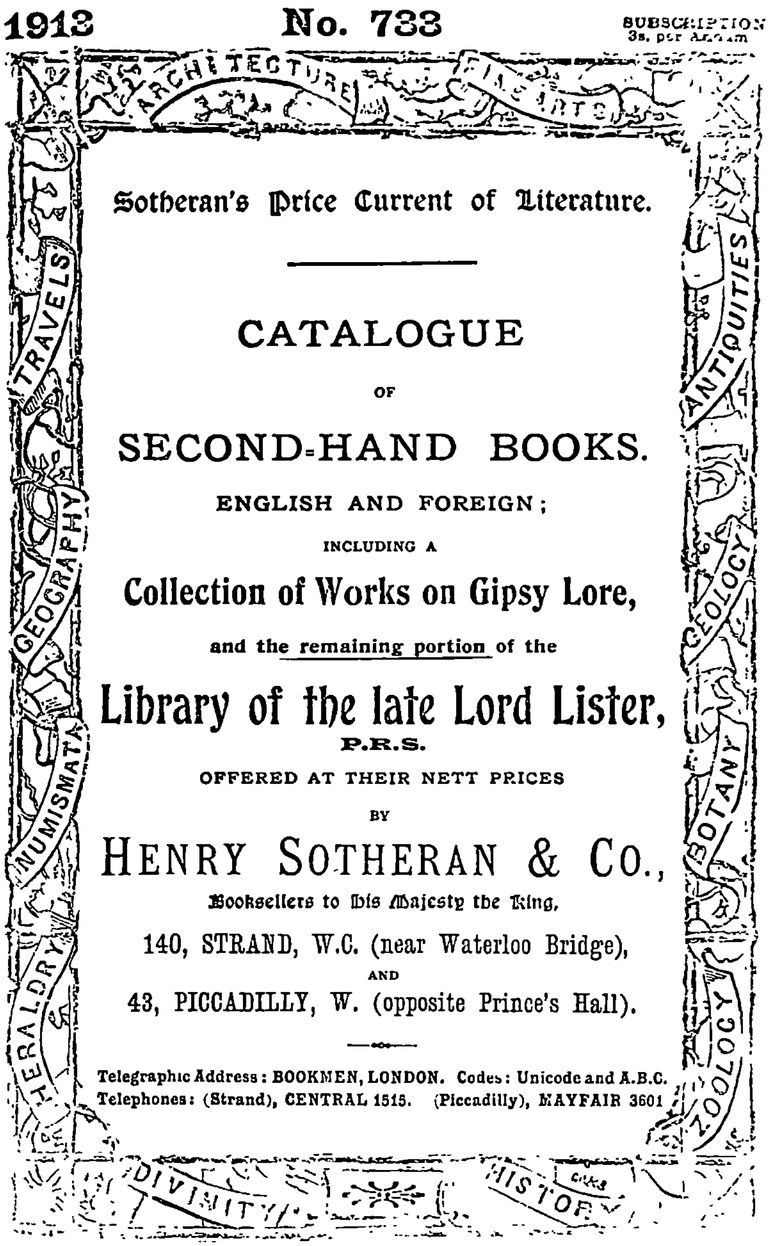
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Fig. 1. Sotheran catalogue of February 1913 announcing sale of the "remaining portion" of Lister's Library.

It is to be noted that the title page (Fig. 1) denotes the sale as representing the "remaining portion" of Lord Lister's library. We are now satisfied that it was the only public sale of Lister's library immediately following his death in 1912.

In 1927 the Wellcome Historical Medical Museum joined many other medical institutions in celebrating the Centenary of Lister's birth. According to the handbook of the occasion,[20] the Museum arranged a magnificent loan exhibit of Lister memorabilia. The walls were hung with pictures, ribbons, tickets, gowns, diplomas, and other honors. There were reproductions of many of Lister's experiments and a section of the Lister Ward of the Glasgow Royal Infirmary which had been demolished in 1924. Among the eighteen display cases were two holding a "Collection of Books, formerly in Lord Lister's Library," supplied by the "Glasgow Lister Memorial Committee." This intriguing item proved to be an elusive one, for we could elicit no information on the Glasgow Committee or on any Lister books from the University of Glasgow or its Royal Infirmary.[21]

However, in August 1972, in the company of Sir Charles Illingworth and with help from Dr. Killoch Anderson, Medical Superintendent of that grand hospital, we met Donald F. Hay, the dedicated Chief Technician of the Pathology Department. We found that for many years Hay has guarded a collection of Lister's books, over eighty titles, in a small museum room of the Infirmary. He has documents to show that they were ordered from Sotheran's Catalogue No. 733, on March 4, 1913, for the Lister Museum of the Royal Infirmary when Professor John H. Teacher, pathologist, was its Honorary Curator. The eighty items are amongst the most precious by today's criteria in the catalogue. They cost the University of Glasgow £26 16s. (ca. $84)! [22]

Besides the books, the Infirmary museum also has instruments, gowns, and other articles pertaining to Lister, and his casebooks for the Glasgow period. Sadly one of these is missing, that of the crucial 1867 period. Perhaps this too will sometime reappear.

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[20] Wellcome Historical Medical Museum, *Lister Centenary Exhibition, Handbook 1927* (London: Wellcome Foundation, 1929), pp. 82-154.

[21] Gaskell was certain the books had been returned to Glasgow (letter from Gaskell to OHW, 7 Oct. 1971).

[22] Letter from A. Ernest Mayland, Convener of the Museum Sub-Committee of the Glasgow Lister Memorial, to Sotheran and Co., 4 Mar. 1913. Hay kindly provided us with a copy of the Museum's Sotheran catalogue on which items not available at the time of the Glasgow purchase, including Dr Wiltshire's selections, had been marked off by the bookseller. He also gave us a list of the Glasgow collection of Lister books

Later, with the help of Eric Gaskell, Librarian to the Wellcome, we delved into the carefully preserved letter files on the Lister Centenary there and confirmed our belief that the books mentioned in the Centenary volume of 1927 actually did come to London as a loan from the Glasgow Teacher Collection.

Late in 1972 we had another unexpected and pleasant surprise. Leonard Payne, Librarian of the Royal College of Physicians in London, recalling our 1971 visit, wrote to alert us of an impending monograph under the editorship of Thomas Hunt concerning the Bicentenary of the Medical Society of London.[23] Payne alluded to a passage in Sir Zachary Cope's preface (p. xi), which indicated that Lister had given the pick of his medical library to the Medical Society of London.[24] Upon reading Sir Zachary's statement, we immediately turned to Godlee's *Lord Lister* and found therein a significant comment which we had overlooked: "The Medical Society . . . seems to have had a particular attraction for him [Lister], as was shown by his bequest to them of the pick of his medical library."[25]

We were chagrined to realize that our search conceivably might have been settled by a brisk walk from the Wellcome to the Medical Society of London. As it turned out, the matter was not that simple.

The Dissolution of a Great Library.

In 1773 John Coakley Lettsom and John Fothergill cooperated to found the Medical Society of London, which over the years acquired a great library. In fact, by 1889 it was described as "unsurpassed in its collection of ancient authors."[26] This, the first regularly constituted general medical society in England, had celebrated its centenary and purchased a handsome building, now called Lettsom House, at 11 Chandos Street, Cavendish Square, a few years before Lister moved to London. Lister's residence in London, 12 Park Crescent, was only a short distance from the new quarters of this venerable Society.

Lister was elected a Fellow of the Society in 1878, a year after he came to London, and in 1904 was awarded Honorary Fellowship. Sir Watson Cheyne said that Lister was "by no means a passive member."

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[23] Thomas Hunt, ed. *The Medical Society of London 1773-1973* (London: Heinemann, 1972).

[24] Letters, Payne to OHW, 12 Dec. 1972, enclosing a copy of Sir Zachary Cope's Preface; and Cope to OHW, 3 Jan. 1973.

[25] Godlee, *Lister*, p. 480.

[26] Charles T Williams, "Opening address of the President," *Trans. M. Soc. London*, 1889, *13*: 4.

His first presentation at the Medical Society of London was in 1883 on "Treatment of Fracture of the Patella," in 1884 and in 1889 he spoke before the Society on antiseptic dressings, and in 1891 gave their Annual Oration, on the subject of coagulation of the blood.[27]

The *Transactions of the Medical Society* for 1912 carries the statement: "The Library Committee has received an intimation from the executors of the late Lord Lister that the medical books in his library have been bequeathed to the Medical Society of London under certain reservations (1912, *35:* liii). Since this gift was not mentioned in Lister's will, it seems likely that it was determined by the codicil, which left the disposal of personal effects to the discretion of the executors; no doubt Lister expressed such a wish to Godlee or the other nephew executors.

In 1913 the librarian reported: "Under the Will of the late Lord Lister the Society has received nearly 2,000 medical books from his library and it is proposed to preserve them as the Lister Bequest in the Council Room of the Society. The establishment of the Lister Bequest will necessitate the provision of book-shelves in the Council Room" (1913, *36:* lii). The next year the Library Committee reported: "The Lister bequest, amounting to nearly 2,500 volumes, has been accommodated in additional bookcases specially purchased for this purpose, and this will be kept distinct from the General Library" (1914, *37:* li). By 1916 Arthur F. Voelcker, Honorary Librarian, reported that the "Catalogue of the Books in the Lister Bequest" was "completed" (1916, *39:* li). As late as 1932 we find a reference to the bequest: "one hundred and fifty volumes of books published before the year 1800 (being part of the Lister Bequest) have now been rebound and are awaiting a suitable bookcase." (1932, *55:* liii).[28]

We found to our sorrow on our first visit to Lettsom House in February 1973, that time and circumstance alter situations. The building, beautifully renovated, was being readied for the Society's 200th anniversary celebration of May 14-17, 1973. The Council Room where, ostensibly, Lister's books were to be kept on prominent display into the endless future, had been moved (Fig. 2). Over the years the Lister bequest, probably because of space problems, became fused with other books and, though Voelcker's report indicated the existence of a catalogue of Lister's

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[27] W. Watson Cheyne, "Presidential address: Lister's work and teaching," *Trans. M. Soc. London,* 1913, *36*: 3.

[28] The reader will wonder, as we have, why the Lister Centenary of 1927 failed to exhibit any of the Lister Collection from the Medical Society of London. The records of the Wellcome on the Lister Centenary are completely silent on this question (letter from Gaskell to OHW, 18 Apr. 1973).

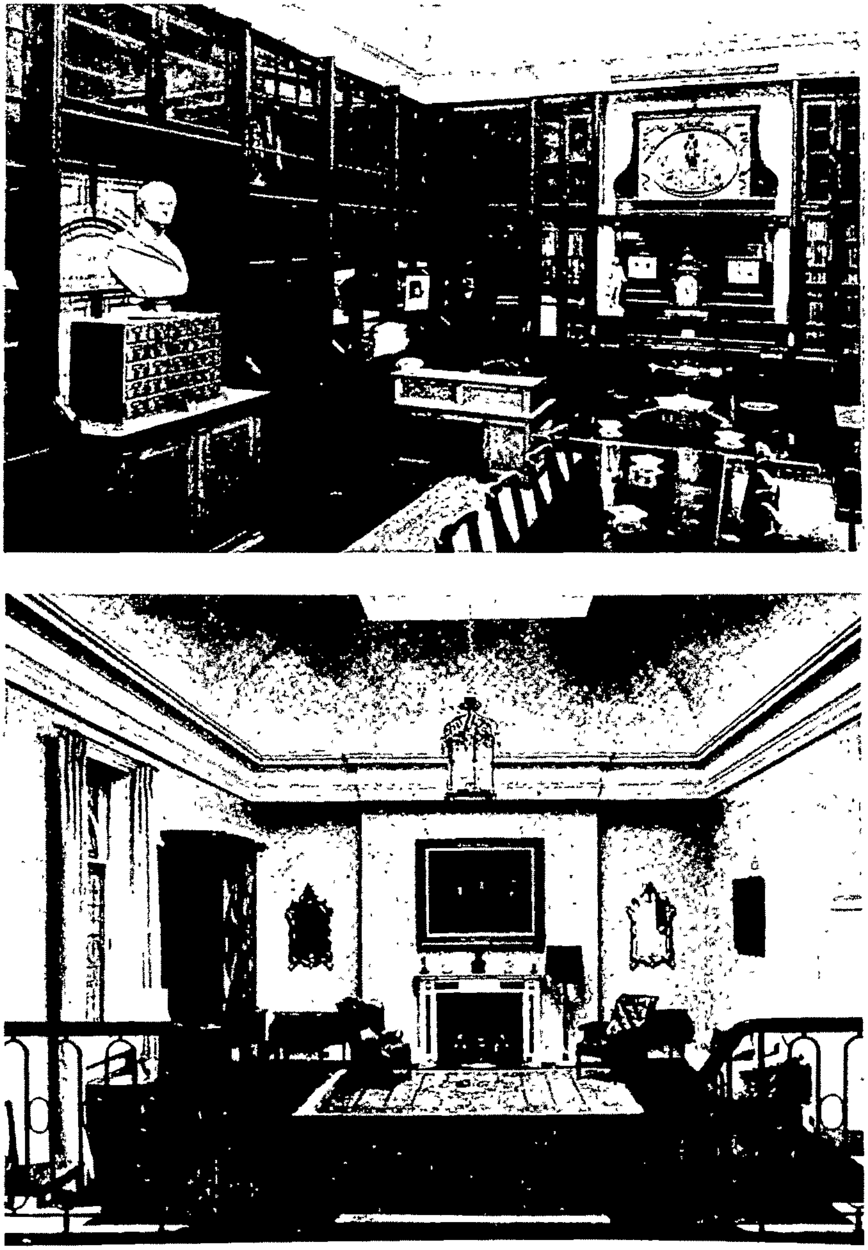

Fig. 2. A The Council Room and shelves of books of the Medical Society of London of the period when the Lister gift of books came.

B The erstwhile Council Room as it appears today.

books, we have been unable to uncover one despite willing help from a number of sources.[29]

The mood of the Council with reference to books and their significance has varied considerably over the years. Directly after World War II, the great historical collection, amassed over a period of 175 years, was keenly appreciated by members of the Council, and considered to be the Society's most important possession. With changes in personnel, and realization that the readers were becoming less historically minded and using the library less, and pressured by the revelation of dry rot in the building's structure, the Council faced the problem of whether to repair the building or to sell their historical collections. Wisely or not they decided on the latter course of action.[30] Which piece to save is often a major decision for surgeons!

An approximate 2400 volumes selected for their rarity went on loan to the Wellcome Library, that agreed to house them over a twenty-year period. Dr. Jason A. Hannah purchased approximately 3500 volumes from the Medical Society for the University of Toronto in 1971-72.[31] A large segment of the Society's collection at the time of dissolution of its library (1966-71) was purchased by members or went to Sotheby and other book dealers. One of the great and tragic mysteries of the odyssey of Lister's books is that apparently no one at the Wellcome or among the many purchasers knew that Lister's large collection was among the dispersed books of the Medical Society's library.[32]

Once more our search returned to that great treasure house, the Wellcome Library, to see if any of Lister's books could be identified among the 2400 volumes held in custodial care for the Medical Society in London.

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[29] Letters from F. Noel L. Poynter, Director, Wellcome Institute of the History of Medicine, to OHW, 11 Apr. 1973; from K. B. Gardner, Principal Keeper, British Museum, to OHW, 9 Apr. 1973; from Alison Gould, British Museum, to OHW, 2 May 1973; from the surgeon Geoffrey B. Woodd-Walker, Hon. Librarian, Medical Society of London, to OHW, 10 May 1973; and Sir Hedley Atkins to OHW, 20 Mar. 1973.

[30] There are several recent accounts of the history of the library of the Medical Society of London See G. B. Woodd-Walker, "The Society's Library," pp. 89-104, in Hunt, *Medical Society of London;* F. Noel L. Poynter, "The Medical Society of London's Library," *Trans. M. Soc. London,* 1966, *82*: 153-167; and Finola Hickey, "The Library of the Medical Society of London, 1773-1969," unpublished thesis [1968-69], Wellcome Library.

[31] The Jason A. Hannah Collection in the History of Medical and Related Sciences has been presented to the University of Toronto and is now appropriately shelved in the new Thomas Fisher Rare Book Library of the University. Dr. Hannah has also endowed professorships in the history of medicine in each of the five medical schools in the Province of Ontario, which hopefully should lend a renewal of emphasis to the study of medical history at both the undergraduate and graduate level throughout North America.

[32] Letter from Poynter to OHW, 11 Apr. 1973.

The collection, still largely uncatalogued, is stored in the Library's vaults. By signatures or, in two instances, bookplates carrying the Society's seal and the words "Presented by the Lister Bequest" we could identify 34 volumes as having been Lister's. A year later in Toronto we had the privilge of examining the fine collection purchased by Dr. Hannah and found 44 more volumes that were given to the Medical Society of London by Lister. Almost all these were publications of the eighteenth or early nineteenth century and bore the Lister Bequest label.[33]

Lister's Books.

As we piece together the bits of evidence from many sources, we believe that, at the time of his death, Lister had assembled a library of approximately 3,000 books, largely scientific and medical. This estimate is based on the inference that all the 2,500 volumes bequeathed to the Medical Society of London were medical in content. In analyzing the 482 titles in the Sotheran catalogue, representing the "remaining portion" of Lister's library, we find that well over 200 of that segment of Lister's books also dealt with scientific or medical subjects.

The other entries in the catalogue comprise a diversified collection, a few children's books, often religious in character, with a generous selection of tomes on theology, and the Quaker faith, poetry, Scotland, and travel. One might have expected to find a wider variety of books on history and general literature, with a liberal admixture of the great English essayists and novelists. Many of the nonprofessional books very obviously had been acquired in Lister's formative years or had come down from his family. Some undoubtedly were dispersed over the years. None of the Listers' books on ornithology, a great hobby and pursuit of their holidays,[34] survived in the collections. The Listers surely would have used some of the many popular books on British birds in print in the last quarter of the 19th century.

The "working library" of books in Lister's special field of interest, though good, was by no means impressive. It is not the collection one might anticipate finding in the possession of a historically minded surgical scholar.

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[33] This bookplate appears, with one exception, only in books published before 1800.

[34] Godlee, *Lister,* pp. 505, 509-510 Perhaps the little dried snakeskin we found pressed between two pages of Lister's copy of Daran's *Observations chirurgicales sur . . . l'urèthre* (1758) is a memento of a Lister holiday (Hannah Collection).

Professional Books.

Surgery was well represented in the collection of Lord Lister's books put up for sale by the Sotheran Company: There were works by both Charles and John Bell, as well as Benjamin Bell's *System of Surgery,* in 7 volumes (1801).[35] Another item (#1226) that of Johann Bilguer on *L'inutilité de l'amputation* (1764), was not a surprise finding in the light of Lister's great interest in amputation. Astley Cooper was represented (#1258, #1259), as was also Benjamin Brodie (#1231). There was a treatise (#1265) by Pierre Dionis (1705).[36] Lorenz Heister (#1295) and Caesar Hawkins (#1294) were included, as was John Erichsen (#1274, #1275), one of Lister's teachers. These texts were published in 1864 and 1869; the latter was a presentation copy to "Professor Lister with the Author's best regards."

Robert Koch's significant book (#1308) summarizing his studies of etiology of wound infections was acquired by Lister in May 1879, the year after its publication.[37] Lister subsequently arranged for its translation into English by W. Watson Cheyne in the Sydenham series (1880). Cheyne's translation (1890) for the same series of Flügge's *Microorganisms* is also listed (#1282).[38]

We failed to find Lister's marked up *Life of Hunter*[39] but we did find his annotated Rothmund, *Ueber Radical-Operation beweglicher Leistenbrüche* (1853) among the Medical Society of London's books at the Wellcome. Lister obviously procured this treatise on hernia, Prof. Franz C. Rothmund's only scientific publication,[40] on his honeymoon when he visited Munich and Rothmund's surgical clinic. The title page bears the label, "J. Lister, Munich Aug/56." Some of the notes have been trimmed in an overzealous rebinding, but Lister's analysis of Rothmund's statistics and other comments are still legible and succinct. As his notes indicate, Lister was aware that Rothmund's cures of hernia with his operation were few. The following February Lister wrote his father that for his first operative clinic before students, his "debut" as a teaching member of Professor Syme's staff, he demonstrated Rothmund's technique of hernia repair.[41]

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[35] These were entries #1218, #1219, and #1217, respectively, in the Sotheran catalogue. Hereafter all such entries will be identified by their specific catalogue numbers. Charles Bell's *Nervous System* (Edinburgh, 1836) had been given "with the author's regards" to James Syme, according to the flyleaf inscription. The inclusion of Syme's books in Lister's library will be discussed later.

[36] This bore Syme's name, with the date 1823. This entry was purchased by the Glasgow Lister Memorial Committee and is now in the Glasgow collection.

[37] *Untersuchungen über die Aetiologie der Wundinfectionskrankheiten* (Leipzig. 1878). The inscription reads "J. Lister Mai 1879."

[38] Purchased by Wiltshire and now in King's College Library.

[39] Godlee had written: Lister's "copy of Palmer's *Life of Hunter* is marked in many places by pencil notes, which show that he was studying it as early as 1855. They are mostly opposite passages dealing with subjects which particularly interested him . . . such as coagulation of the blood, inflammatory fever, the principles of bleeding and their dependence on what Hunter called continuous sympathy, the action of counter-irritants, and local diseases cured by sympathy" (*Lister,* p. 597).

[40] H. Killian and G. Krämer in their monograph on *Meister der Chirurgie und die Chirurgenschulen im Deutschen Raum* (Stuttgart: Thiem, 1951, p. 192) were unable to find any other publications of Rothmund, though he was Professor of Surgery at Munich for 28 years (1843-71).

Also at the Wellcome are three volumes presented to Lister by the Danish surgeon, M. H. Saxtorph, *Clinisk Chirurgi* (Kjøbenhavn, 1878).[42] The first two books undoubtedly are those mentioned by Lister in a letter to his brother Arthur from Scandinavia in 1879. "Thus my friend Prof. Saxtorph of Copenhagen has sent me two volumes of lectures on clinical surgery by himself which have hitherto been absolutely useless through my ignorance of the language. So I thought it worth while to look into it, and got in Christiania a dictionary and a simple little grammar, and soon found the similarity to our own tongue gave it great interest, while at the same time the grammar is extremely simple."[43]

Some of the books in the Wellcome Medical Society collection had belonged to Lister's father-in-law, Syme, including what appears to have been the working copy for a new edition of Syme's *Principles of Surgery* (3d. ed., London, 1842). The book is heavily corrected and annotated in handwriting and contains a number of loose manuscript pages to the same purpose. Since a number of Syme's medical books also appeared in the Sotheran catalogue it seems likely that Lister acquired his father-in-law's library on his death in 1870, though there is no mention of such a bequest in Syme's will.[44] Several rare medical tomes given by Syme in February 1848 to biologist-anatomist Allen Thomson were presented to Lister in 1886 by Thomson's son, John Millar Thomson, as inscriptions dutifully explain. Examples are Malpighi's *De formatione pulli in ovo* (1673) and an early Valsalva, *De aure humana* (1735), both in the Hannah collection. Syme was doubtless slimming his library before moving to London that year to beome Chairman of Surgery in the University College Hospital.

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[41] Lister to his father, 26 Feb. 1957, quoted in Godlee, *Lister*, p. 61.

[42] In volume 3 the author has added a note: "Thank you for the letter but I am sorry you cannot send me the papers I spoke of."

[43] The Listers were en route from Hull to the Sixth International Medical Congress in Amsterdam. "The first Norwegian land to greet us," said Lister, "was the province of Lister Land," adding, "I dare say the source of our Lister ancestors." The province lies at the southwest tip of Norway between Christiansand and Stavanger. The letter, dated 11 Aug. 1879 at Faleide, is in Agnes Lister's handwriting and was evidently dictated (Godlee, *Lister*, pp. 506-507).

[44] Syme apparently had no bookplate, but his "bookstamp" is mentioned in Sotheran's catalogue (#1358). Syme's will does not mention books (Scottish Record Office, H.M. General Register House, Edinburgh).

Among books with Syme's name on the title pages are the important *Experiments* of the chemist Joseph Black (Edinburgh, 1782) (#1117); and John Collins Warren's *Surgical Observations on Tumours* (Boston, 1837) (#1391) "with best respect of J. Mason Warren." It is possible that many of the books published before 1800 in Lister's bequest to the Medical Society were Syme's, for two of the oldest classics in the Sotheran sale were his: Dionis' *L'anatomie de l'homme* (1705) (#1265)[45] and Ridley's *Anatomia cerebri* (Leiden, 1725) (#1358).

Lister Family Books.

Intermingled with Lister's books are some once belonging to other members of his family. A number were his father's, Joseph Jackson Lister (1786-1869), a successful wine merchant and amateur microscopist.[46] He devised the achromatic lens that made the compound microscope feasible and was in a sense, therefore, the founder of modern microscopy. *The Power of Religion on the Mind in Retirement, Sickness, and at Death* (#1556)[47] was presented to him in 1793 at the age of 7 by "his Master as a Reward for his Improvement in Writing." An abridged Locke's *Human Understanding* (#1195),[48] inscribed "J. L. to Isa. Harris, Jr.," seems an appropriate gift from Lister's father to his sweetheart Isabella Harris, Jr. She was mistress of reading at a Quaker School before their marriage.[49] One entry (#1434) was apparently a biographical account of Isabella's mother in the Quaker *Monitor* (York, 1834). It was given by his mother "to her dear son Joseph" in memory of his Harris grandparents "on the 14th of the 12th Month 1834, being the 97th Birthday of his Grandfather John Lister."[50] The close relationship of mother and son is revealed in many of the inscriptions on later gifts.

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[45] Glasgow collection.

[46] Not unlike the Dutchman, v. Leeuwenhoek, also a member of a brewer's family, whose assembly of microscopes and grinding lens won him Fellowship in the Royal Society in 1680. J. J. Lister too became a member of the Royal Society (1832). In a lengthy 8-page handwritten will, signed Nov. 12, 1864, he left among household effects, his books and instruments to his children "to divide or dispose of . . . amongst themselves" (p. 2).

[47] "Exemplified in the Testimonies and Experiences of Men distinguished by their Greatness, Learning, or Virtue" (Birmingham, 1788) No author is given. Glasgow collection.

[48] Glasgow collection.

[49] Godlee, *Lister,* p. 4.

[50] John Lister lived to be 98 (Godlee, *Lister,* p. 1).

One of the most interesting items, in the view of an Edinburgh librarian to whom we showed the Sotheran catalogue, was a seven-volume collection of account books kept by Lister's father from 1807 to 1869, about 1700 pages (#1194). These must have been acquired subsequently by relatives, because Wellcome's Centenary in 1927 had such books on exhibit, loaned by "the Misses Lister." The Wellcome handbook says they contained references to Joseph Lister.[51]

Children's Books.

There are only two strictly children's books. One is the rare first French translation of Joachim H. Campe's "the New Robinson."[52] This fictitious account of a boy castaway was obviously inspired by *Robinson Crusoe,* but tailored for the young. It antedated the *Swiss Family Robinson* by a quarter century. "Joseph Lister" is inscribed on the flyleaf, but the book may have belonged originally to Lister's father. There is no question of the ownership of *Evenings at Home or the Juvenile Budget Opened* (#1099), by Dr. John Aikin and his sister Anna Letitia Barbauld, for "Joseph Lister from his Papa 17th of 4th Mo. 1833," appears on 3 of the 4 volumes. In this popular compendium, first printed in 1792-96, are fables, fairy tales, dialogues on such natural history subjects as botany and geology, and short stories of contemporary interest, all written with an instructive and moral slant but nevertheless often entertaining and readable. One wonders if six-year-old Joseph pondered over the wounded soldier in "The Price of Victory" returning home after twelve months in a hospital where all the patients would have "suffocated with the heat and stench but fever thinned the numbers." It is a graphic account and grimly depicts pre-antiseptic war surgery in the Napoleonic era, as well as the futility of war.

School Books.

Lister's school and college studies can be followed through his signature, dates, and sometimes ms. notes on texts of Latin or Greek classics, history, statics, mechanics, chemistry, and astronomy. In the King's College collection is a worn Latin Homer (#1170) with "Joseph Lister

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[51] Wellcome Museum, *Lister Centenary,* (n. 20 above), p. 138.

[52] Joachim H. Campe, *Le nouveau Robinson,* translated [into French] by Auguste S. d'Arnay (1783) (#1123). Glasgow collection.

1840" on the flyleaf. Lister would have been 13 then and attending Grove House School, Tottenham. Godlee has told us (p. 12) that Lister's father liked to have his children read Latin authors, "not the simplest," with him while he was dressing.

Lister's name and the year 1842 appeared on Hamilton's *Analytical System of Conic Sections* (#1164)[53] as well as notes and diagrams in Lister's writing. One of the earliest of the many academic awards Lister was to receive in his meteoric career was a first prize in the Experimental Class of Natural Philosophy, University College, for the term 1844-45.[54] The prize was signalized by an inscribed copy of Charles Hutton's *Recreations in Mathematics and Natural Philosophy translated from . . . Ozanam* (1840) (#1176). It turned up in the Sotheran sale. The number of mathematics books in the collection suggests that Lister especially treasured them.

School Botany (1839) (#1191)[55] by John Lindley, a professor who was an inspiration to Lister at University College, was procured in April 1844 just after Lister entered that institution. There are other works on flora, for field botany was a lifetime avocation.[56] The subject became of deeper significance as Lister's interest in fungi and bacteria intensified in 1872. He wrote his botanist brother for the name of Berkeley's book on fungi, March 31, 1872.[57]

Religion.

Among the books listed by Sotheran is a Bible, *The English Hexapla* (#1530), given to Lister "on his 21st birthday 5th of 4th month 1848 by his very affectionate brothers and sisters, Isabella Sophia Lister, William Hy. Lister, Arthur Lister, Jane Lister." The choice for such an occasion is indicative of the importance of religion to the Lister family. So too was the number of Quaker books and theological treatises in Lister's library, totaling about a fourth of all the non-medical entries in the catalogue.

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[53] King's College Library.

[54] Mentioned in a brochure of *Testimonials in Favour of Joseph Lister* (p. 23) that appeared in 1866 when Lister was an unsuccessful candidate for the professorship of surgery in University College.

[55] King's College Library.

[56] Godlee (*Lister,* p. 263) says: "Packets of pressed flowers were the invariable and much prized trophies of foreign tours."

[57] Miles J. Berkeley's *Cryptogamic Botany* (1857) (#1111) is in the King's College Library collection. Lister had written: "I have distinctly proved that two kinds of bacteria grow from two fungi. . . . The yeast plant presents a good example of the kind of modifications fungi are liable to from differences of circumstances such as thee say are alluded to in Berkeley's book. Will thee please tell me exactly its title that I may order it?" (Quoted in Godlee, *Lister,* p. 264).

In Lister's era a Quaker resigned membership in the Society of Friends when he married outside the faith.[58] After his marriage to Agnes Syme, Lister joined the Episcopal Church and his name is written in several hymnals (#1545, #1546)[59] of the Church of Scotland. However the many Quaker books in his library would suggest he did not put aside completely the religion of his family, and his mother and father continued to give him Quaker books (#1439, #1449, #1450).

An end-paper notation "Agnes Lister with J. L.'s dear love," dated September 10, 1860, appears in a book (#1557) with the rather startling title: *The Missing Link.*[60] It is Mrs. Ellen H. Raynard's evangelical account of the work of the Bible Society women in London slums.

Travel.

It is understandable that Lister's own travels should be reflected in his library, as for instance, *The Ruins of Pompeii* (#1135) and *Walks In Rome* (#1165). Godlee has said of Lister: "Throughout his life he had a fair share of vacations." Even before his medical education was completed he traveled in Great Britain and on the Continent. Associates remembered "his almost boisterous lightheartedness during . . . brief respites from the engrossing toil" of his Scottish period. Holidays in later years were "longer and more leisurely, but none the less enjoyable."[61]

It is moving to read the inscription "J. & A. Lister June 1856" on a *New Method of Learning . . . Italian* (#1421),[62] for this must have accompanied the young Listers on their honeymoon in Europe. A French pocket dictionary is inscribed "Joseph Lister. From Father Christmas, Jany 1, 74," according to the Sotheran description (#1110).

In his 75th year, Lister began to manifest some of the infirmities of age and was advised to take a sea voyage in the hope of overcoming increasing invalidism. *The Divina Commedia* (#1131) may be the one Lister read on the way to South Africa in 1902. Said Godlee (p. 574),

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[58] Godlee, *Lister*, p. 42.

[59] Glasgow collection.

[60] Charles Lyell (1851) first used the term "missing links" in discussing the origins of geographical strata on the basis of fossil remains. The term since has been more often applied to biological species differences. *Manual of Elementary Geology*, 3rd ed. (London: Murray, 1851), p. 220.

[61] Godlee, *Lister*, pp. 24, 504-505.

[62] Glasgow collection.

"The days were regularly portioned out, with readings at stated times, from the Spanish New Testament, Dante's *Purgatorio,* the *Odes of Horace,* and pages from some English book."

Scotland and Poetry.

Lister's collection of Scottish books was sufficient for a separate listing in Sotheran's catalogue. Poetry could well have been another, for there is a large assortment of poetry, ballads, and colloquial rhyme, many of Scottish origin. Godlee mentions Lister's love of poetry. In writing to Arthur in 1875, Lister spoke of being engrossed by Tennyson's *Queen Mary.* "But it is not Shakespeare by a long, long way."[63]

Judging from the Sotheran catalogue, novels could not have been very popular with the Listers, for there are, surprisingly, almost none in the collection except for a few by Scottish authors—*Rab and His Friends* (#1118)[64] is there. Booth Tarkington's *Gentleman from Indiana* (#1518)[65] was a gift from a doctor in the 1900s. It must always be considered a possibility, however, that some of Lister's books were kept by relatives and not put up for sale.

Presentation Copies.

Of the 482 titles in Sotheran's catalogue of Lister books, nearly 100 carry presentation inscriptions from patients, professional associates, or friends, especially of the last fifteen years of the 19th century when Lister's fame had spread throughout the world. It may surely be assumed that many not inscribed were also gifts, such as the elaborately illustrated tomes on Scotland and art. Sometimes the story of a friendship is revealed. One inscription recalls Lister "in student-days" as "an ideal and a joy-giver" (#1122).[66] Dr. Charles J. Hare remembers "pleasant chats . . . on board the good ship 'Bothnia' from New York . . . Centennial Year" (#1291). Nicolas Senn indicated he was "one of his most ardent admirers" (#1498).[67]

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[63] Letter to Arthur Lister, 234 June 1875, quoted in Godlee, *Lister,* p. 371.

[64] The author, Dr. John Brown (1810-82), was Syme's first house surgeon and an intimate family friend (John A. Shepherd, *Simpson and Syme of Edinburgh,* Edinburgh & London. Livingstone, 1969, p. 45). Dr. Brown was a devoted admirer of Joseph and Agnes who were, he believed, "made for each other and for our good." (letter from Brown to Agnes Lister, 29 Mar. 1877, quoted in Godlee, *Lister,* p. 402).

[65] Glasgow collection.

[66] The book, privately printed in 1877, was *Broken Lights; or Fragments of My Life* by Thomas W. Cameron.

[67] The books from Hare and Senn are in the Glasgow collection.

In the Medical Society's books at the Wellcome there are a number inscribed to Lister, often with warm praise from an eminent contemporary. Included were books from Franz Koenig, Eduard Albert, Edgar M. Crookshank, M. Schede, Henry Smith, Lewis S. Pilcher, Luther Holden, and Emil Adolph Behring.

Attached to a presentation copy of William Macewen's *Osteotomy* (1880) is a letter to Lister from the author telling that he has performed an additional 101 cases "without the slightest accident . . . I see your hand in all of them."

Many of Lister's books must now be on the market or in private possession. His medical books will be difficult to identify because he had no bookplate,[68] though he sometimes pencilled his name and date of acquisition on the flyleaf. Up until his marriage in 1856 Lister, as did his parents, usually followed the Quaker practice of simply numbering the day and month without inserting "pagan" names.

When we first found some of Lister's books at the King's College Hospital Library, we were surprised to note uncut pages in Liston's *Elements of Surgery* (1832) (#1320). Nor were the pages cut in Celsus' *De medicina* (1815) (#1248) or in a presentation copy of Paget's *Clinical Lectures* (1879) (#1347) except for the preface, where the recipient may have quickly glanced for any personal remarks. A fine edition of Landois' *Human Physiology* (1885) (#1314), a gift of the translator, had apparently never been opened.[69] We began to appreciate that, as many of Britain's noted librarians had told us, Lister was not a great reader of books.

Markusovszky's Copy of Semmelweis' Die Aetiologie (1861).

In his pre-professorial years, Lister studied thoroughly the literature of subjects upon which he wrote, a practice he apparently neglected during his active professional years until after his retirement (1893). Unfortunately, the dispersion of his library makes it very difficult to determine Lister's acquaintance with the writings of his antecedents upon the theme of his own life's work.

Among the books that came to the Wellcome from the Medical Society of London upon the dissolution of that great library, was a fine copy of the first edition of Semmelweis (1861)[70] which bore on its title page the signature of Lajos Markusovszky, great friend and contemporary of Semmelweis. Markusovszky died in 1893, outliving Semmelweis by 28 years. Was this one of Lister's books? Or had Markusovszky, or a friend, sent the monograph to the Medical Society? Only discovery of a letter of transmittal or of acknowledgement or a catalogue of Lister's bequest to the Society can answer that intricate question. Possibly a reader of these lines at some future date may help resolve it.

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[68] The Sotheran plate identifies any book in the 1913 sale of Lister's library. Over half of these were of non-medical content.

[69] Lister had not even cut all the pages in his prospective father-in-law Syme's *Treatment of Stricture of the Urethra* (Edinburgh: Sutherland & Knox, 1855). It is inscribed "Joseph Lister, Esq., with the author's best regards." This book is at the Wellcome. In 1856 Syme gave Lister his 4th edition of *Principles of Surgery,* also at the Wellcome.

The Listers in Budapest, October 1883.

Since presentation of this manuscript at the Cincinnati meeting, May 3, 1973, the authors have pursued inquiry into the matter of the Lister visit to Budapest in the week of Thursday, September 27 to Saturday, September 29, 1883. There was a large public gathering at the Hotel of the English Queen celebrating Lord Lister's visit on Saturday evening, October 6. Dr. Lajos Markusovszky presided. Mrs. Markusovszky was present as was Lady Lister. Markusovszky, a leader in the faculty and a greatly respected public servant of Hungary, addressed the honored guests, Lord and Lady Lister, in fluent English. A segment of Markusovszky's address to the large assembled audience was probably in German. It was a convivial meeting, yet a serious one, that lasted long into the evening hours, as related in an October issue of *Orvosi Hetilap* (1883, pp. 1063-64).

Semmelweis' *Die Aetiologie des Kindbettfiebers* appeared in print in October 1860. Markusovszky wrote a long and favorable review of Semmelweis' book which suggests that Markusovszky understood the problem of puerperal fever and Semmelweis' teachings and prophylactic antiseptic practices far better than did the famous German pathologist, Rudolf Virchow, and the foremost European obstetricians (*Orvosi Hetilap,* 1861, pp. 184-186, 225-226, 267-270). (Translations provided by Dr. John J. Alpar, Amarillo, Texas.)

That Markusovszky, a distinguished physician, had more than a passing acquaintance with Semmelweis' methods in obstetrics, can be inferred from the circumstance that Semmelweis issued a certificate of competence to Markusovszky on July 27, 1847, in which Semmelweis indicated that Markusovszky had undergone a course of instruction under Semmelweis' supervision on the "touch examination-techniques" on the mannikin, the cadaver, and upon parturients in labor.[71]

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[70] *Die Aetiologie, der Begriff und die Prophylaxis des Kindbettfiebers* (Pest, Wien und Leipzig: C. A. Hartleben, 1861). Semmelweis' *Vorwort* is dated, "Pest, den 30, August 1860" (p. vi). The book actually appeared at the end of October 1860, according to Gy. Gortvay and I. Zoltan in *Semmelweis: His Life and Work* (Budapest: Ad Akademiai Kiado, 1968), p. 132 Semmelweis' 1861 presentation copy of *Die Aetiologie* to Routh may be seen at the Royal Society of Medicine Library in London He also sent copies to several other distinguished London physicians and obstetricians.

These events, establishing definite links in the story of the Markusovszky-Lister-Semmelweis relationship, the authors believe lend definite support to the belief that the copy of Semmelweis' *Die Aetiologie* bearing Markusovszky's name, which found its way to the shelves of the Wellcome Library, very likely came as a gift from Markusovszky to Lister. Sometime, therefore, between the Budapest public celebration for Lister in September 1883 and before 1896, when Lister capitulated completely to prophylactic antisepsis, Lister gained acquaintance with the teachings and the prophylactic antiseptic practices of Semmelweis. The most logical explanation, it would appear, is that Lister's acquaintance with Semmelweis' work came soon after the visit of the Listers to Budapest in late September 1883, and through representations of Markusovszky to Lister on the important work and teachings of Semmelweis.

Lister and Semmelweis.

When Lister came to know of the incisive, innovative, and perceptive work of Semmelweis upon wound sepsis of the parturient is still uncertain. From his own statement[72] one would infer it was in the mid 1880s, after his visit to Budapest in 1883. Wrench states that when Lister visited Budapest in 1883, he was warmly received and greatly honored. "The occasion was rendered peculiarly happy by Dr. [Theodore] Duka, a Hungarian physician, who brought to Lister's attention the name of his great but unhappy countryman, Ignatz Philipp Semmelweiss [*sic*]." [78] Unfortunately Godlee is completely silent on Lister's Budapest visit. Lister's contemporaries John Simon and James Y. Simpson were well acquainted with Semmelweis' work, which had been publicized throughout Europe and the British Isles by representatives of the Vienna School of Medicine and especially by v. Hebra, Skoda, Haller, and Rokitansky and by Semmelweis' Vienna associates in obstetrics, Drs. Arneth and Routh. There was obviously, therefore, the opportunity for Lister to have known of Semmelweis at the outset of his own work had he been a more ardent student of the literature of wound infection during his early professorial years.

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[71] Joszef Antall, Violo R Harko, and Tivadar Vida, "Gesammelte Handscriften v. Ignac Semmelweis," *Communicationes de Historia Artis Medicinae,* Budapest, 1968, *46-47*: 194.

[72] "When . I visited Buda-Pesth . . . Semmelweis' name was never mentioned, having been, as it seems, as entirely forgotten in his native city as in the world at large. It was some time after this that my attention was drawn to Semmelweis and his work by Dr. Duka, a Hungarian physician practising in London." Letter to Dr. Weckerling, 15 Sept. 1906, quoted in Godlee, *Lister,* p. 141. That Lister's acquaintanceship with Duka was considerably earlier than this statement would imply is revealed in a hitherto unknown letter that we found September 19, 1973, among Duka's papers in the Hungarian Academy of Science, with the help of Ferenc Nemthy, Librarian at the Semmelweis Library. The letter, from Lister in London to Duka, is dated October 11, 1883, shortly after the Listers had returned from Budapest. It reads: "Dear Dr Duka, I am much obliged to you for sending me the illustrated paper. I need hardly assure you how much gratified I was by the extreme kindness of my professional brethren in Budapest. Believe me, very sincerely yours, Joseph Lister."

Carl Haller, in reporting upon Semmelweis' important work on the obstetrical wards of the Allgemeines Krankenhaus, said: "The significance of [Semmelweis'] discovery for hospitals in general and especially for surgical wards is immeasurable and deserves the serious consideration of all men of science." [74]

In the 1886 *Lancet,* Duka wrote of Semmelweis' studies on puerperal fever.[75] Duka subsequently republished this as a small monograph in two separate editions (1888 and 1892) under somewhat different titles, and sent copies to several leading medical men of London.[76] Monday evening, July 25, 1892, Spencer Wells invited a number of London's leaders in medicine to his home to establish a British committee to cosponsor with the International Semmelweis Committee, based in Budapest, an International Monument to commemorate Semmelweis' significant contribution to the management of puerperal fever. The *Lancet* names eight guests, including Lord Lister and Duka.[77]

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[73] G. T. Wrench, *Lord Lister, His Life and Work* (London: Unwin, 1913), pp. 341-342.

[74] Carl Haller, "Aerztlicher Bericht über . . . die k.k. Gebär- . . . im Solar-Jahre 1848," *Ztschr. d k.-k. Gesellsch. d. Aerzte zu Wien,* 1849, vol. 5, pt. 2, p. 538. Within a month after Semmelweis initiated his rigorous plan of prophylactic antisepsis on the obstetrical wards of Allgemeines Krankenhaus, Haller, Adjunct Director of the hospital, wrote (June 1, 1847): "A fresh morning breeze penetrates the healing arts that presages a beautiful future when the inspiration of dedicated and impartial research will have shown the way." (*Ztschr. d. k.-k. Gesellsch. d. Aerzte zu Wien,* 1847, vol 4, pt. 1, p. [2], following p. xxx; cited by R. Chrobak, "Die Semmelweis-Feier," in Dirner Gusztav, *A Nemzetközi Semmelweis-Emlek Budapesten,* Budapest: 1909, p. 178).

[75] Theodore Duka, "Childbed fever; its causes and prevention a life's history," *Lancet,* 1886, *2*: 206-208, 246-248.

[76] Sir Andrew Clark and Dr. Matthews Duncan were among the recipients. *Lancet,* 1892, *2*: 1012 (reprinted in Gusztav, *op cit.,* n. 73, p. 23).

[77] The initial suggestion of an International Monument to Semmelweis apparently originated with Markusovszky The Budapest Committee was formed in 1891; the British Committee in 1892. When completion of the worldwide cooperative effort was signalized in Budapest,

Did Lister's advice of abandoning instillation of antiseptics into wounds with concomitant emphasis upon prophylaxis (1893, 1896) reflect a newly acquired acquaintance with the work of Semmelweis during his final professorial years?

Lister's address to chemical wound antisepsis began in 1865, following advices Lister had received from Dr. Thomas Anderson, Professor of Chemistry at the University of Glasgow. Several articles by Frederick Crace Calvert on medical uses of phenol had appeared in British medical journals. Thomas Spencer Wells (1864) had emphasized the significance of Pasteur's great work on fermentation (1857, 1859), spontaneous generation (1860), and putrefaction (1863) for practical medicine and surgery. Lister began by placing lint dipped in concentrated carbolic acid into the wounds of open fractures. He gradually shifted to more dilute solutions of 10, 5, and 2.5 percent concentrations, often irrigating wounds with dilute solutions of phenol, followed by placement of an overlay wound dressing of phenol. In 1867 Lister's first paper on antiseptic practices appeared in the *Lancet* in five installments,[78] the first being published before the succeeding installments had been written. His second report on the subject was given before the British Medical Association in Dublin September 21, 1867.[79] In 1871, Lister substituted the carbolic acid spray[80] for wound irrigation. And meanwhile Lister had extended his phenol wound management to elective surgery.

In an address delivered to the International Medical Congress in Berlin in 1890, Lister said he had stopped using the carbolic acid spray

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Sept. 29-30, 1909, many of the principals were dead, and Lister was confined to his house by the mounting disabilities of his 82 years. *Lancet,* 1892, *2*: 267, 897, 1012-1013.

[78] "On a new method of treating compound fracture, abscess, &c., with observations on the conditions of suppuration," *Lancet,* 1867, *1*: 326-329, 357-359, 507-509; *2*: 95-96; *Collected Papers,* vol. 2, pp. 1-36.

[79] The address, "Antiseptic principle in the practice of surgery" (*Brit. M. J.,* 1867, 2: 246-248), was reprinted in Lister's centenary, in the Apr 1, 1967 issue of the same journal (pp. 9-12). The reprint provoked "continuing request for reprints . . addressed to Dr. Joseph Lister, University of Glasgow, Scotland." Should this "be regarded as a subtle and graceful complement to Lister's immortality" or does it merely point " a moral to both those who issue and those who receive requests for reprints?" asks C M. Fleming, University of Glasgow, in a letter entitled "Intimations of immortality?" (*Brit. M. J.,* 1967, *3*(1): 53)

[80] Abraham Wallace (1912), Surgical Dresser to Lister in Edinburgh, related it had been Lister's practice to irrigate wounds at operation with a syringe containing a dilute solution of carbolic acid and also at wound dressing changes. Wallace and John Davidson on ward rounds suggested that a carbolic acid spray would be superior to irrigation, a discussion overheard by John Chiene, assistant surgeon. Chiene immediately reported it to Lister, who implemented the suggestion directly. *Univ Coll. Hosp. Mag.,* 1912, *2*: 182.

over the operative field three years before (1887).[81] In abandoning the spray Lister returned temporarily to wound irrigation with antiseptics, a practice he had employed before introduction of the spray (1871).

In his Presidential Address to the British Association for the Advancement of Science at Liverpool (1896), Lister made a retrospective assessment of his own work, and also discussed many recent significant developments in the field of medicine and surgery. He then advised discontinuance of antiseptic wound irrigation and of placement of antiseptics into wounds, leaving "Nature" undisturbed to carry out her best methods of repair.[82] Obviously Lister had veered about completely, eliminating all pretense of trying to produce a nonseptic wound by placement of antiseptic agents in the wound. He had now become an ardent advocate of prophylactic antisepsis. Was this not the basic principle of Semmelweis' teachings and practices (1847-50, 1858-62)?

The conclusion is inescapable: in his final professorial years and in retirement, with greater opportunity for reflection and reading, Lister gained an acquaintance with the teachings and practices of Semmelweis that he had neglected at the inception of his own life's mission. How different Lister's antiseptic wound practices might have been had he become fully alert at the outset of his important work to the significance of Semmelweis' ideas of the prevention of wound infection.

Hand Preparation for Operation.

Lister's emphasis during his active professional years had been upon placement of antiseptics in wounds. He had very little to say about preparation of the surgeon's hands. His preference during his years at King's was to rely solely on dipping his fingers into the "strong solution" (5% phenol and 0.2% $HgCl_2$). One has to search the writings of his professional assistants and successors at King's College, notably Willoughby Lyle and Frederick F. Cartwright to learn of Lister's emphasis (or lack thereof) in the preparation of the surgeon's hands for operation. Lister apparently avoided scrubbing his hands, believing it deepened the crevices of the palms, thus encouraging lodgement of bacteria.[83]

When Lister's first paper (1867) on surgical antisepsis appeared he was 40 years of age. He was almost 70 on the occasion of the 1896 address, which represented the distillate of a life's work with wound management.

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[81] *Brit. M. J.*, 1890, *2*: 377; *Collected Papers*, vol. 2, pp. 336-337.

[82] *Collected Papers*, vol. 2, p. 500.

[83] H. Willoughby Lyle, *King's and Some King's Men* (London: Oxford University Press, 1935), p. 191; F F. Cartwright, "Lister—the man," *Brit. J S.*, 1967, *54*: 409 (Special number).

In 1884 Lister participated in a discussion on war wounds and referred to Carl Reyher's[84] classification of wounds (1878) as "fingerte und unbefingerte" (fingered and unfingered).[85] Reyher had alluded to the stern instruction of Bernhard v. Langenbeck (1868) that in management of war wounds, fingering and probing of gunshot wounds must be studiously avoided.[86] Lister obviously had learned of the great importance of prophylactic aspects of wound antisepsis from both Reyher and Semmelweis.

Wrench has related that King George IV summoned his surgeon, Sir Astley Cooper, to his bedside 24 hours after the operation on the King's scalp for the removal of a sebaceous cyst (1820). Noting looks of disapproval from the King, on return to his quarters Cooper asked his nephew the likely reason thereof. Said the nephew, it would have been appropriate to have worn a clean shirt and to have washed the blood from your hands before visiting the King.[87] How strange it is that surgeons lagged so far behind cooks and public health officials in bringing clean hands to their tasks. The emphasis upon clean hands for accoucheurs and surgeons by Semmelweis in 1847 was an instruction that had long been enjoined upon those responsible for serving food to the public. Most surgeons with a few notable exceptions looked to poor ventilation and overcrowding in large hospitals as the responsible agents for the continued high mortality of their operations. Why did it take so long to turn the mirror of self-scrutiny upon themselves for their shortcomings? Surgeons painfully learned and at great cost to their patients that socially clean hands were not enough.

Some Notable Book Collections of Physicians.

The magnificent book collection of William Hunter resides today in a stately room in the new library of the University of Glasgow, and much of James Y. Simpson's is at the Royal Society of Medicine in Edinburgh. Simpson's 1,500 books included all the well-known works on midwifery published in Britain as well as many from other countries.[88] The collection, the working library of a busy 19th century obstetrician and physician, affords a good picture of medical practice of the period.

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[84] The innovative Russian surgeon Reyher spent several months in Lister's clinic in Edinburgh in 1874, coming from Dorpat where he worked with Ernst v. Bergmann. Reyher's name is spelled "Rahere" in the *Lancet* discussion and Lister labels him a German surgeon. At the 7th International Medical Congress held in London in 1881, Reyher established definitely the significant role of early debridement and antisepsis for wound care in military surgery.

[85] *Lancet,* 1884, *1*: 346.

[86] "Die antiseptische Wundbehandlung in der Kriegschirurgie," *Sammlung Klin. Vorträge* (Chirurgie 45) 1878, p. 1214.

[87] Wrench, *Lord Lister,* p. 88.

Oliver Wendell Holmes presented his medical books to the Boston Medical Library in 1889, calling the 868 volumes, the "stepping-stones of my professional life." They were later absorbed into other collections in the library, but the titles have been preserved fortunately in a list for the inquiring student.[89]

In the absence of other information, bookdealers' catalogues can be invaluable to the researcher, Sotheran's catalogue of the "remaining portion" of Lister's library being an example in point. The sale catalogue (1754-55) of Dr. Richard Mead's library permits an excellent analysis of Mead's anatomical book collection, one segment of that great bibliophile's treasures.[90] Claude Bernard's fine scientific library of more than 1400 entries was described in a Paris bookdealer's catalogue of 1878, a few months after Bernard's death.[91]

The University of Illinois has recently prepared an exemplary catalogue of its Percival Bailey collection of works in neurology and psychiatry, listing each book with a full description.[92] This could well serve as a precept for collections and librarians, and hopefully become common practice.

Summary.

Study of Lister's *Collected Papers* confirms and corroborates his testimony that he was unfamiliar with the work of Semmelweis during his professional years. After retirement, Lister reverted to his earlier, pre-professorial writing practices in which he manifested an intimate acquaintance with the pertinent literature of topics under study. Sometime during this period (1883-92) Lister first became acquainted with the writings of Semmelweis. The conclusion is inescapable: Lister's ultimate advocacy of preventive antisepsis (1893, 1896) was definitely influenced by the innovative practices and teachings of Semmelweis, for whom Lister came to have great admiration and appreciation, as we know from an exchange of letters in 1898 with his former house officer, John Chiene, then Professor of Surgery at the Glasgow Royal Infirmary.[93]

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[88] Alistair L. Gunn, "James Young Simpson—the complete Gynaecologist," *J. Obstet. Gynaec. Br. Commonw.*, 1968, *75*: 260-261; letter from Joan P. S. Ferguson to OHW, 16 Apr. 1973.

[89] Oliver Wendell Holmes, "Address before the Boston Medical Library Association," *Boston M. & S. J.*, 1889, *120*: 129-130; Henry R. Viets, "Oliver Wendell Holmes: his books," *Bull. Hist. Med.*, 1964, *38*: 530-531; letter from Richard J Wolfe, Rare Books Librarian, Countway Library, to OHW, 22 Mar. 1973.

[90] K. F. Russell, "The anatomical library of Dr. Richard Mead (1673-1754)," *J. Hist. Med.*, 1947, *2*: 97-109.

[91] *Catalogue des livres composant la bibliothèque de Claude Bernard dont la vente aura lieu . . . 5 juin 1878, et jours suivants . . .* (Paris: Baillière et fils, 1878). The National Library of Medicine has a copy.

[92] *A Catalog of the Percival Bailey Collection of Neurology & Psychiatry,* compiled by Alexander M Cain (Chicago: Univ. of Ill. at the Medical Center, 1973); it sells for the modest cost of $3.00. Our attention was called to this publication in a letter from Phillip Wade, Librarian, of the Royal Society of Medicine, London, to OHW, 3 Apr 1973.

A lesson emerging from this quest is how necessary it is that a catalogue of important collections of books be compiled and preserved. It is understandable that such collections even in great libraries often lose their identity and cannot always be preserved intact. However, a catalogue placed in some of the great medical libraries of the world will suffice to tell future students something of the original collector's background and mode of attack upon problems.

If the catalogue of Lister's bequest to the Medical Society of London is found, it may provide additional useful information on Lister's study habits and source books. It must be remembered, however, that Lord Lister might not have grieved to see his books dispersed, just as he had modestly stipulated in his will that his medals and honors might be destroyed if deemed expedient.

This surgeon has often said to patrons of the University of Minnesota's Medical Historical Library that great books, well kept, are amongst the most durable things on this earth, rivalling the redwoods in permanence, a thesis now under test in many overcrowded libraries. Possibly this recounting, though discouraging, may lend renewed vigor to affirming that belief.

To the writers' view two significant points emerge from this segment of the study: (1) The importance of books upon the work and professional careers of investigators, including surgeon-academicians. (2) The accountability to posterity of medical libraries and librarians for significant collections of books under their custody.

In concluding this account of our quest for the fate of Lister's books, it is fitting to recall the admonition of Richard Garnett, Librarian of the British Museum, who in writing of the function of national libraries (1879) said: "Every such library is the mirror of its time, and perhaps even its services to contemporaries are of less real account than those which it performs for posterity in preserving the image of the past"[94]—words worth remembering.

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[93] Quoted in Godlee, *Lister,* pp. 660-663.

Acknowledgments.

The authors desire to express their warm thanks for the generous help of many distinguished and willing librarians and colleagues throughout Britain, many of whose names appear in the text of our paper. Their helpful and ready cooperation did much to make segments of this effort possible. Our thanks also are owing to librarians at the University of Minnesota, especially to Mrs. Mary Mueller, to Mrs. Dorothy Hanks at NLM, and to Miss Marion E. Brown of the Thomas Fisher Rare Book Library of the University of Toronto.

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[94] From Richard Garnett, "Public libraries and their catalogues," which first appeared in the *New Quarterly Review,* April 1879. Reprinted in his *Essays in Librarianship and Bibliography* (New York: Harper, 1899), p. 39.

CHRISTIAN FENGER, MEDICAL IMMIGRANT*

GEORGE ROSEN

American medicine was derivative in its origins. Beginning as an element of a culture brought from Europe by the first settlers, it was influenced by European sources throughout much of its evolution. Numerous studies have elucidated the influence of Edinburgh in the eighteenth century, the import of the Paris clinical school in the earlier nineteenth century, and the consequences of the American medical migration to Germany later in the century. According to Dixon Ryan Fox, cultural transfers of this kind occur in four stages.[1] First, a few educated in the mother country emigrate to the new settlement; then, the colonial youth are drawn back to the mother country for training; in the third stage, schools are established in the newly settled country but numerous students and teachers are still educated abroad; the fourth stage is present when the educational establishments of the younger country become independent and attract students to themselves.

The transition from the third to the fourth stage in American medicine occurred chiefly during the last quarter of the nineteenth century and was completed during the early years of this century. A powerful force for change was provided by American physicians who studied in Germany or in the German-language universities of the Habsburg Empire and Switzerland between 1870 and 1914, an experience which imbued them with the traditions and methods of German medicine, and the urge to apply these for the improvement of medical science and practice at home. The role of these physicians as agents of change is well recognized and requires no elaboration.[2] Much less attention has been given to European physicians trained in the German tradition who emigrated to the United States bringing with them progressive ideas and the latest medical advances which they transmitted to American medical students and doctors unable or unwilling to go abroad. Among them were a number who left a permanent imprint on American medicine. This was especially true in laryngology, ophthalmology and pediatrics, and similar though lesser influences may be found in gynecology, surgery, pathology, anatomy, pharmacology and psychiatry.

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*The Seventeenth Annual Samuel Clark Harvey Lecture in the History of Surgery, February 28, 1972.

[1] Dixon Ryan Fox, "Civilization in transit," *Amer. Hist. Rev.*, 1927, *32:* 753.

[2] George Rosen, "Carl Ludwig and his American students," *Bull. Inst. Hist. Med*, *4:* 609-650; T. N. Bonner, *American Doctors and German Universities* (Lincoln, Neb.: University of Nebraska Press, 1963).

Ernst Krackowizer (1821-1875) fled Austria following the defeat of the 1848 revolution, established himself in New York City and became a leading surgeon.[3] In 1858, receiving a laryngoscope from a friend in Vienna, he introduced laryngoscopy to America and was the first in this country to demonstrate the vocal cords. Another pioneer laryngologist and medical immigrant was Louis Elsberg (1836-1885), who more than anyone else in the United States was active in drawing attention to the value of the laryngoscope. Elsberg's interest in the instrument was stimulated by Czermak who also sent him his book on it. He participated in the founding of the American Laryngological Association, becoming its first president, and was also a founder of the *Archives of Laryngology*.[4]

The *American Journal of Ophthalmology*, the first ophthalmologic periodical in the United States, began publication in 1862 in New York, having been founded by Julius Homberger, an immigrant physician from Germany who had studied with von Graefe and Sichel, and who settled in New York City. Similarly, the *Archives of Ophthalmology*, which began to appear in 1869, was established by Hermann Knapp (1832-1911), a student of Helmholtz and professor at Heidelberg, who came to New York in 1868 and became the leading ophthalmologist of the city. Ferdinand Holtz, a student of Knapp in Heidelberg came to Chicago in 1876, and was a prominent specialist there.[5]

The role of Abraham Jacobi (1830-1919) in the growth of pediatrics was even more central. After graduating from the medical school at Bonn in 1857, Jacobi became active in the Communist League, organized by Marx and Engels. His revolutionary activity led to his arrest and imprisonment, but he managed to escape and reach New York where he settled. In 1853 he started to practice, and four years later began to give lectures on the diseases of children at the College of Physicians and Surgeons. In 1860 Jacobi was appointed to the first special chair in pediatrics in this country at the New York Medical College, where in

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[3] Ernst P. Boas, "A refugee doctor of 1850," *J. Hist. Med.*, 1948, *3:* 65-94.

[4] George Rosen, *The Specialization of Medicine* (New York: Froben Press, 1944), p. 38; Arturo Castiglioni, *A History of Medicine*, 2nd ed. rev. (New York: Knopf, 1947), p. 869; Bonner, "American doctors," p. 90.

[5] Rosen, *Specialization of Medicine*, p. 36; H. A. Kelly and W. L. Burrage, *American Medical Biographies* (Baltimore: Norman Remington, 1920), p. 547; R. I. Lloyd, "First American eye journal and its editor," *Amer. J. Ophthal.*, 1932, *15:* 1123-1126; Bonner, *American Doctors*, p. 90.

1862 he established a clinic for children's diseases. Jacobi's leadership and pioneer work in pediatrics was acknowledged when the American Pediatric Society was formed in 1888 and he was elected its first president.[6]

At the first meeting of the American Gynecological Society, founded in 1876, Emil Noeggerath (1827-1925), also an immigrant, read his important paper on latent gonorrhea, in which he established that the infection was not limited to the lower female genital tract but ascended through the uterus to the tubes and the peritoneum, and was therefore an important cause of sterility in women. With Jacobi, he was co-founder of the *American Journal of Obstetrics* (1868), a title which was later expanded to include *Gynecology,* and the journal is still being published.[7]

There were other immigrant physicians who made significant contributions to American medicine, for example, Adolf Meyer in psychiatry and Carl Voegtlin in pharmacology, but many more simply served their patients to the best of their ability and left no legacy more tangible than a reference in local medical journals and newspapers, or in the recollections of grateful fellow townsmen. Indeed, the history of the immigrant physician in the United States still remains to be written. When it is written, however, attention will have to be given to one medical immigrant whose achievement was of a different order, whose contribution was not limited to a special field of medicine, but whose pervasive influence was exercised through "a group of physicians, whom he activated to a modern conception of pathology, to more scientific ways of thinking, and to a form of practice in which the standard of success was intellectual honesty rather than financial return or reputation as a brilliant teacher or operator." [8] That medical immigrant was Christian Fenger, a Danish physician, who came to Chicago in 1877. He was then 37 years of age, and had already had a great deal of experience in pathology, surgery, and medical practice.

II

Christian Fenger was born on November 3, 1840, on an estate owned by his father, Hans Fritz Fenger, situated on the northwest coast of Denmark in Jutland.[9] He was one of a family of twelve children, seven boys and five girls, and the father struggled hard to provide a good education for them, especially for the sons. Upon completion of his secondary schooling in 1859, Fenger's interest in the natural sciences led him to begin the study of engineering at the Polytechnic Institute. After one year, he yielded to his father's wish, and entered the medical school in Copenhagen. Fenger partly earned his expenses by teaching in a high school and tutoring medical and dental students. By 1863 he was able to pass his first medical examination covering anatomy, zoology, botany and chemistry. When the Schleswig-Holstein War between Prussia and Denmark broke out in February 1864, he joined the Danish army, serving as assistant physician at the Augustenborg Lazareth as well as with an artillery unit at Assens. At the end of the war Fenger returned to his medical studies, and in the winter of 1866-67 passed his final examination permitting him to practice.

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[6] Francis R. Packard, *History of Medicine in the United States,* 2 vols. (New York: Paul B. Hoeber, 1931), vol 2, pp 1183-1184; Friedrich Engels, Zur Geschichte des "Bundes der Komunisten, in Karl Marx, *Ausgewählte Schriften,* 2 vols. (Zürich: Ring Verlag, 1934), vol. 2, p. 24.

[7] Castiglioni, *History of Medicine,* p. 859.

[8] James B. Herrick, *Memories of Eighty Years* (Chicago: University of Chicago Press, 1949), p. 174.

During the latter part of Fenger's medical education, he came under the influence of two professors who taught him how to do scientific work. One was the distinguished physiologist and epidemiologist, Peter Ludvig Panum (1820-1885), who in 1864 was appointed to the chair of physiology at Copenhagen, after having taught physiology, general pathology and medical chemistry at Kiel during the preceding five years. Panum had studied with Claude Bernard and introduced new methods in physiologic teaching and research. The other was the clinician Hans Wilhelm Meyer (1824-1895) who initiated a special clinic for ear, nose and throat diseases two years before Fenger graduated. Following Fenger's graduation, Meyer appointed him as an assistant (*amanuensis*) in his clinic and private practice. During this period, Fenger participated in the studies which led to Meyer's important paper on adenoid vegetations in the nasopharynx and their role in the obstruction of the Eustachian tube (1868).[10]

In 1869, Fenger began a two-year internship at the Royal Frederik

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[9] For Fenger's biography the basic source is the short autobiography he wrote on January 1, 1902. It was published in *The Collected Works of Christian Fenger M.D. 1840-1902,* 2 vols. (Philadelphia and London: W. B. Saunders, 1912), vol. 1, pp. 1-5.

[10] L. L. McArthur, "Christian Fenger as I knew him," *Bull. Soc. Med. Hist. Chicago,* 1923, *3:* 51-57 (pp. 52-53). H. V. Meyer, "Om adenoide Vegetationer i Naesesvaelgrummet, de sygelige Tilfaelde, som de frembringe, og disses Helbredelse," *Hosp.-Tid., Kjøbenh.,* 1868, *11:* 171-181; in English as, "On adenoid vegetations in the nasopharyngeal cavity: their pathology, diagnosis and treatment," *Med.-Chir. Tr.,* 2nd ser., 1870, *35:* 191-215; W. M. Gafafer, "Bibliographical biography of Peter Ludvig Panum (1820-1885), epidemiologist and physiologist," *Bull. Inst. Hist. Med.,* 1934, *2:* 259-280.

Hospital, during which he undertook his first studies in medical investigation. In addition to the preparation of a prize essay on subperiosteal operations, he carried out experiments on gunshot wounds of horses at the Royal Veterinary College, leading to the development of instruments for locating bullets in the tissues and removing them.[11] The publication of a paper on the endoscopy of gunshot wounds, embodying the results of these investigations, led to a grant from the Royal Danish Ministry of War which enabled Fenger to participate in the Franco-Prussian War. In January 1871 he was appointed by August Socin (1837-1899), professor of surgery at Basel, to an international ambulance on the battlefield of Bourtaki-Werden (Haute Saône) where he worked until the end of the war. This experience led to a second report on the endoscopy of gunshot wounds.

Then after several months in Vienna studying pathological anatomy and surgery, Fenger returned to Denmark and was appointed in 1871 to the post of prosector at the General Hospital (*Communehospital*). In the course of his duties, he did a large number of autopsies, gained a wide experience in pathology, and wrote a number of papers on gonorrheal rheumatism, partial hydronephrosis, stenosis of the pulmonary orifice and pulmonary artery, and endoscopy of the urethra. At the same time he was gathering material, for the most part in the post mortem room of the General Hospital, for a monograph on cancer of the stomach with particular reference to structure, development and spread. This study, carried out exclusively from a pathologic-anatomical viewpoint, was accepted by the faculty of the University of Copenhagen as a thesis for the degree of Doctor of Medicine, which Fenger received in 1874. Illustrated by his own drawings, and based on his own material as well as on an extensive knowledge of the pertinent literature, the monograph was an important contribution to the pathology of cancer and is still worth the attention of anyone interested in its history.

In 1875, Fenger's growing reputation as a pathologist led to his appointment as instructor in pathological anatomy with an expectation that he would eventually be in line to compete for a full professorship. But when the position was filled without competition, he felt that the road to an academic career was blocked and decided to leave Denmark. Actually, as indicated by his studies of gunshot wounds and his participation in two wars, Fenger wanted to be a surgeon. The study of pathology was in his view preparatory toward this end. However, the professor of surgery was opposed to him because he was a protégé of Meyer, the otolaryngologist, whom the surgeon disliked. With both pathology and surgery blocked, Fenger decided to seek opportunities elsewhere. Assisted by a loan from a friend which he repaid fourteen years later, he set out for Egypt.

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[11] The papers mentioned below are either published or referred to in his *Collected Works,* vol. 1, pp. 6-146.

One of Fenger's brothers, also a physician, had emigrated to Egypt in 1873 and was practising in Alexandria. In the spring of 1875, while his brother visited Denmark to be married, Fenger took over his practice. The following winter, on his brother's return, he moved to Cairo where he was able to obtain an appointment as a district physician (*médecin de quartier*) and to set up in practice. Here he investigated trachoma among children in the public schools and reported his findings to the government. After the Ethiopian War of 1875, an epidemic disease of horses and mules was introduced into Egypt, and all the horses in Cairo died. Fenger, ordered to investigate the disease, performed a number of postmortem examinations, discussed the possible modes of transmission in his report, and proposed several prophylactic measures. From his investigation he concluded that the animals were probably infected by way of the respiratory tract. Consequently he recommended careful isolation of the healthy horses, and also that the animals' heads be covered with cotton masks which had been treated with a solution of carbolic acid and salicylic acid and then dried.[12] In 1876, his studies on schistosomiasis, carried on in a large military hospital, were interrupted when he suffered an attack of dysentery followed by inflammation of the liver. The winter of 1876-77 found him in Mentone where he had gone to recuperate, and at the end of the winter he returned to Cairo. However, the liver complaint reappeared in the spring of 1877, and Fenger was obliged to seek a more temperate climate.

In Cairo, Fenger had been physician to members of the American colony, among whom were several officers brought in to reform the Egyptian army. Advised by the Americans to go to the United States, Fenger resigned his Egyptian post in June 1877 and sailed from Alexandria to New York by way of Liverpool. After landing in New York he remained there for a few weeks expecting to practice ophthalmology, in which he was well trained, but the prospects of success appeared dim and so he decided to go west. Fenger's settling in Chicago was fortuitous. Actually he intended to visit a Major Irgens in Bloomington, whom he knew from Egypt and who had suggested that he locate there, or if it did not suit him to move on to San Francisco, or eventually to Australia. Fenger appears still to have thought of settling and becoming an ophthalmologist in one of these places and was on his way west when he stopped in Chicago.[13]

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[12] Fenger, *Works,* vol. 1, p 146.

III

When Christian Fenger registered at the Great Pacific Hotel in 1877, he was a thick set, florid Danish physician, in his middle thirties, with an uncertain future, little baggage and less money. He carried his belongings in a green carpet bag on which his sister had embroidered his initials. Because of an illness while at the hotel, Fenger requested the medical services of a fellow countryman, S. D. Jacobsen, who took care of him and advised him to settle in Chicago, especially since his funds were practically exhausted. Jacobsen was on the staff of the Cook County Hospital and had charge of the eye and ear department.[14]

Fenger took Jacobsen's advice, opened an office in the Scandinavian section of the city and acquired a small practice. His office is reported to have been in a frame building over a saloon, which was probably the case. A glimpse of these quarters at that period is provided by William E. Morgan, then a student in the office of William E. Quine, a Chicago internist. The latter had sent Morgan to Fenger with a specimen. After mounting the winding back stairs, he found Fenger at work in a poorly furnished, ill-kept room. On his table were a small microscope, some slides, as well as numerous periodicals and manuscripts, with very little evidence of any order.[15]

Many months elapsed before Fenger was able to make a living, but during this period he acquired friends who tried to help him. One of these friends, a politician, appears to have been largely instrumental in obtaining Fenger's first appointment to the staff of the Cook County Hospital. As part of an election deal, he had been promised a post on the surgical staff for the recent medical immigrant, but after the election the promise was not kept. Instead, Fenger got the position of pathologist which was not considered desirable. A remark by Fenger in his autobiography throws an additional light on his appointment and foreshadows its consequences. "In the spring of 1878," he wrote, "I secured by means of borrowed money, a place as physician to Cook County Hospital, and here I commenced to give lectures and demonstrations in pathologic anatomy, a science which was unknown to the physicians there." A corrupt group of county commissioners were in control and a physician had to buy his appointment. The price of a position on the hospital staff was a thousand dollars, and it is likely that Fenger borrowed the money from his friend Jacobsen.[16]

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[13] Coleman G. Buford, "Christian Fenger: a biographical sketch," *Bull. Soc. Med. Hist. Chicago,* 1913, *1:* 196-204 (p. 198).

[14] McArthur, "Christian Fenger," p. 53; Buford, "Christian Fenger," p. 198.

[15] Buford, "Christian Fenger," p. 198.

The situation prior to Fenger's arrival in Chicago has been succinctly and vividly characterized by several of his contemporaries. According to Quine, "there was no microscopic and no clinical laboratory apart from the drug room. Laboratory diagnosis was unknown except such proceedings as were connected with medical examination of urine. Bacteriology and hematology were undeveloped." [17] An equally primitive situation is described by Lyman, one of the men who preceded Fenger as hospital pathologist. "The surgeons," he says, "used their own instruments, and the physicians carried their own stethoscopes. There was a knife and a saw, a chisel and mallet in the dead house; and the interns contrived to keep a few test tubes and a little nitric acid in their rooms. Fehling's solution was then unknown to us." [18] Not only were the conditions primitive but there was also the problem of the relation of the pathologist to the attending staff. The pathologist was not a member of the Medical Board of the hospital so that his "prerogatives" were limited. Furthermore, the attending physicians and surgeons did not want to have their dead patients autopsied by anyone but themselves. As a result Lyman notes, "there was little occupation for a curator [i.e. pathologist] in an institution that could not afford to furnish alcohol and jars for the preservation of specimens. As for a microscope and a laboratory—they were out of the question." Hosmer A. Johnson, who became pathologist in 1870, declared three years later that he would not serve in this capacity unless "he had free access, and the first right, to the material in the Dead House." [19] Nonetheless, the controversy was not settled until the appointment in April 1877 of Isaac Newton Danforth (1835-1911) to the post of pathologist in the new county hospital, constructed after the fire of

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[16] Fenger, *Works,* vol. 1, p. 4; Buford, "Christian Fenger," p. 199; McArthur, "Christian Fenger," p. 53.

[17] William E. Quine, "Early history of the Cook County Hospital to 1870," *Bull. Soc. Med. Hist. Chicago,* 1911, *1:* 15-24 (p. 20).

[18] Henry M. Lyman, "A bit of the history of the Cook County Hospital," *Bull. Soc. Med. Hist. Chicago,* 1911, *1:* 25-36 (p. 31).

[19] Ludvig Hektoen, "Early pathology in Chicago and Christian Fenger," *Proceedings of the Institute of Medicine of Chicago,* 1936-37, *11:* 258-272 (p. 264).

1871 and opened for use in 1876. In this facility Danforth made post-mortem examinations and lectured on pathological anatomy to students from Rush and Chicago Medical Colleges. Students from the Woman's Medical College (later connected with Northwestern University) also attended. Danforth requested that the pathologist be recognized as a regular member of the Medical Board, a request that was granted. Furthermore, he insisted that the physicians keep hands off cadavers from their wards, achieving some success in this effort. However, he was unable to obtain the $200 needed for preserving and mounting pathological specimens which could form the nucleus of a permanent collection.[20] In 1878 the entire medical staff of the hospital was suddenly dismissed and Danforth's endeavors came to an end. But as he observed later, he left a basis on which his successor was able to build. When the new Medical Board met on June 29, 1878, the pathologist to the hospital was made one of its members, and he was given "the exclusive right to make or direct post-mortem examinations, and to lecture on Pathological Anatomy to the students attending the hospital, providing the Attending Physicians and Surgeons retain the privilege of witnessing post mortem examinations, and that fair notice of the time of that occurrence be given to them."[21] This was the situation into which Fenger stepped when he was elected pathologist.

IV

By his own testimony Danforth had been "utterly without the aid or sympathy or support of the Medical Faculty." Fenger's situation was slightly but not much better. As a newcomer, he was assigned a rather unsuitable time for his teaching. L. L. McArthur, then a medical student, described the situation and its effect on him as follows: "Dr. Fenger was given for his clinic the inconvenient hour from one to two—an hour never before assigned to an attending man for a clinic. Twelve to two, in all previous years, was set aside for luncheon, rest and recreation. Following custom, the students failed at first to attend in any large numbers. Thus it happened that Dr. J. B. Murphy and myself, fascinated by his wonderful technic in making a post mortem examination, thrilled by the revelations each necropsy offered, and rewarded by his efforts to convey to us the information that should have gone to the entire class, were constant attendants during these first months, one taking down the findings, the other sewing up the cadaver. Undoubtedly, this experience remained ever after a potent factor in J. B. Murphy's future development, as was later true of so many others."[22]

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[20] *Ibid.*, pp. 265-268.
[21] *Ibid.*, p. 268.

The points emphasized by McArthur, the technical competence, the thoroughness, and the information derived from Fenger's autopsies, soon impressed his colleagues and students. As Frank Billings summed it up, for the first time they "had an opportunity to witness scientifically conducted autopsies and to learn the fundamentals of morbid anatomy and pathology."[23] Illustrative are three cases reported in 1879. As recorded by the secretary of the Chicago Pathological Society, Fenger exhibited to the members "some morbid specimens obtained from a recent patient. When he opened the body, the doctor had no history of the case, except that the patient presented typhoid symptoms. Opening the abdomen, he found infarctions in the submucous tissue of the intestines, in the spleen and in the kidneys. The source of these emboli was found in an ulcerous endocarditis, both sets of semilunar valves presenting an abundant deposit. Upon examining farther, an embolus was found in the left middle meningeal artery, which had caused an extensive infarct; and several small infarcts were found in each retina (illustrating the value of the ophthalmoscope in the diagnosis of heart disease). The doctor thought from the fact that some of the infarcts were breaking down into abscesses, that the cause of endocarditis was septicemia. There was no evidence of acute rheumatism nor of any surgical operation. Careful search, however, revealed a suppurating synovitis at the bottom of a large bunion. Dr. Fenger said this might have been the starting point of the whole disease. As a proof that this was a blood disease the doctor exhibited under the microscope some of the exudate from the heart valves in which were myriads of micrococci."[24] This revelation of bacteria in the vegetations of acute endocarditis was the first demonstration on this side of the Atlantic of the bacterial nature of the condition.

In the same year, a case of generalized miliary tuberculosis with involvement of the choroid was reported by Edward L. Holmes, a Chicago ophthalmologist. Fenger did the autopsy and demonstrated eight minute elevations in the posterior half of the choroid. In his presentation Holmes said: "I believe Dr. C. Fenger, pathologist to the Cook County Hospital, is the first physician who ever made such an autopsy in this part of the country."[25] The third case reported in 1879 was one of diffuse multiple capillary fat embolism of the lungs and brain resulting from a fracture of the shaft of the femur.

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[22] McArthur, "Christian Fenger," p. 54.

[23] Frank Billings, "Christian Fenger," *Surg., Gynec. & Obst.*, 1922, *35:* 365-369 (p. 367).

[24] Ludvig Hektoen, "Notes on the history of bacteriology in Chicago prior to the organization of bacteriologic teaching (about 1890)," *Bull. Soc. Med. Hist. Chicago*, 1937, *5:* 3-21 (p. 4).

Fenger was not a bacteriologist nor did he undertake bacteriologic studies. However, he did stimulate some of his students to investigate such problems. In the summer of 1886, Bayard Holmes, then assistant to Fenger, carried out under his direction an experimental study of the relation of air infection to contact infection in surgical operations. More specifically, he determined by bacteriologic methods how far the necessary aseptic conditions had been secured and maintained in a series of abdominal operations performed by Fenger. This investigation was published jointly the following year as "Antisepsis in Abdominal Operations: Synopsis of a Series of Bacteriologic Studies." This study was a result not only of his scientific interest, but also of the circumstance that he had achieved his goal of becoming a surgeon.[26]

In 1879, Fenger served in the surgical wards of the County Hospital, substituting for various surgeons when they were on vacation. The following year he received a regular appointment to the surgical staff, which he held for the next twelve years. During this period he was actively involved with a variety of surgical problems. One of these was the problem of hospital infections, particularly hospital gangrene and erysipelas. Fenger attacked the situation by introducing Listerian antisepsis. What this involved has been graphically described by L. L. McArthur, who as a Junior, had the responsibility for dressing every open wound or sore. "Thus," he writes, "in the Hospital, all of the leg ulcers, inflamed, ingrowing toe nails, felons et id genus omne, must be dressed under a spray of 5% phenol, washed with 2.5% phenol, covered with eight layers of gauze, one layer of mackintosh, and a ninth layer of gauze outside—a useless layer. This meant dressings from 8 a.m. to 6 p.m. daily—and no trained nurses! But soon there began to be healings 'per primam,' shown at first with pride to the staff and interns and later as something to be expected; finally, if the desired result was not obtained, it was a reproach to someone."[27] By the middle 1880s Fenger had moved from antisepsis to asepsis. He was one of the first in Chicago to use rubber gloves, and to change in the hospital from his street clothes to a clean operating room uniform.[28]

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[25] Hektoen, "Early pathology," p. 271.

[26] Christian Fenger and Bayard Holmes, "Antisepsis in abdominal operations; synopsis of a series of bacteriological studies." *J.A.M.A.*, 1887, *9:* 44, 470-472; Hektoen, "History of bacteriology," pp. 17-18.

[27] McArthur, "Christian Fenger," p. 55.

In that period of "easy informality in unpaid academic appointments," [29] as Esmond Long so aptly put it, Fenger's ability as a teacher and practitioner was recognized by appointment to the staffs of a number of medical schools and hospitals. In 1880 he became curator of the Rush Medical College museum, and in 1884 professor of surgery in the College of Physicians and Surgeons, later a part of the University of Illinois. In 1893 Fenger accepted the chair of surgery at the Chicago Medical College, later the Northwestern University Medical School, and the post of Surgeon to the Hospital of the Sisters of Mercy. From 1882 to 1886 he was also professor of general pathology and pathological anatomy. When the Norwegians in Chicago erected the Lutheran Tabitha Hospital in 1894, he became the surgeon-in-chief. In 1899 Fenger left the Chicago Medical College to become professor of surgery in Rush Medical College, with a surgical service in the Presbyterian Hospital. When the Passavant Hospital and the German Hospital were founded, Fenger became surgeon-in-chief to these institutions. In fact, he did most of his private surgery at the Passavant Hospital.

Fenger made important contributions to the surgery of the gall bladder, the pelvic organs and the kidney, thereby acquiring an international reputation. For the International Medical Congress held at Paris in 1900, he was invited to present a paper on "Surgical Operations in Conditions of Retention in the Kidneys," which was later published in Langenbeck's *Archiv für klinische Chirurgie* in Germany, and in the *Annals of Surgery* in this country.[30] However, Fenger's greatest contribution was not as an operator, even though no surgical procedure was too formidable for him to undertake if he thought it justified by the circumstances. According to James B. Herrick, one of his students, some of his operations were "marvels of completeness. His removal of tuberculous glands of the neck or carcinomatous glands in the axilla were wonderful dissections with every bit of suspicious tissue removed." [31] This thoroughness, based on an extensive knowledge of the pertinent literature and on wide experience, produced highly successful results in many cases, but there were those where the exercise of thoroughness was not necessarily in the best interests of the patient. In cases of perforative appendicitis, for example, Fenger apparently never really learned the principle applied so successfully by his student J. B. Murphy, and which he expressed simply as "I go in and get out as quickly as I can. If the appendix presents in the wound, I take it out. If not, I leave it alone. I put in a drain and quit."[32] On the other hand, Fenger's almost compulsive thoroughness in such cases tended to get in the way of sound surgical judgment, leading his student and assistant T. A. Davis to the opinion that he was too thorough a surgeon for the abdomen.[33] On the whole, however, Fenger was a safe surgeon, careful but not rapid.

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[28] T. N. Bonner, *Medicine in Chicago 1850-1950* (Madison, Wis.: American History Research Center, 1957), p. 87.

[29] Esmond R. Long, *A History of American Pathology* (Springfield, Ill.: Charles C Thomas, 1962), p. 173.

[30] Fenger, *Works,* pp. 1037-1050; *Annals of Surgery,* 1901, *33:* 369.

[31] James B. Herrick, "Christian Fenger as I knew him, 1885-1902. A study in personality," *Proceedings of the Institute of Medicine of Chicago,* 1940-41, *13:* 318-329 (p. 325).

However, a quality of thought and practice which was disadvantageous in some situations was a virtue in others. The painstaking thoroughness with which Fenger operated was an aspect of his approach to medicine, to understand each case rationally and scientifically and to correlate the clinical findings with the underlying pathology. In this sense, one may apply to him John Finney's comment about Halsted, that he was a "head surgeon" rather than a "hand surgeon," that he was a "better surgeon than operator."[34] For even as a surgeon, Fenger was still very much a pathologist. Coleman Buford, also one of his students, said in 1912, that "he interpreted his pathology as he proceeded" with an operation. "He remained in the wound longer than do those of today in search of pathologic facts which are now common knowledge. He saw more pathology in a wound and had more to relate about it than all the surgeons I have ever known combined."[35] This extensive knowledge was derived from several sources. Fenger collected specimens of every type of surgical pathology from necropsies and operations. Thus he was able not only to discuss a wide range of subjects but to present numerous specimens in support of his position. For this reason the pathological discussions in Fenger's surgical papers are important not alone for surgeons but equally for pathologists. In addition, Fenger was also an omnivorous reader of the medical literature, despite considerable difficulties in obtaining books and periodicals. "In 1880," he said, "with the exception of one or two private libraries, there were no medical libraries to which physicians could have access. There were probably not more than two or three sets of the Great German year books, such as Schmidt's

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[32] *Ibid.*, p. 325.

[33] *Ibid.*, p. 326.

[34] *Ibid.*, p. 325.

[35] Buford, "Christian Fenger," p. 200.

Jahrbücher and the Virchow-Hirsch Jahresbericht, in Chicago, and these were not available for the general practitioner except by favor. Some specialists had good libraries in their own line, but that was all."[36] As a result he purchased the books and periodicals that he required, with special attention to bacteriology, pathology, internal medicine, diseases of the genito-urinary system, and nervous and mental disorders. Fenger was fluent in a number of languages and was consequently able to cover a much wider spectrum of medical and scientific resources than his colleagues. After his death, his medical library of about 6000 items was presented to Rush Medical College.[37]

The knowledge obtained from these sources was applied to teaching, practice and research. As a teacher, his systematic, thorough discussion of a case involved a presentation of the history and the symptoms, a detailed examination of the patient, followed by a discussion of the relevant anatomical, physiological and pathological features, as well as a review of the pertinent literature. This was followed by a discussion of the indications for and against different methods of surgical intervention. In this way Fenger brought pathology into the clinic and impressively laid bare its basic relation to diagnosis and therapy.[38]

However, Fenger's effectiveness as a teacher was not limited to his clinical lectures. His influence extended to interns in the hospital, to his assistants, and to practitioners who called upon him for consultation. For example, on Friday nights during the winter months he met with his assistants, the interns and the pathologist from the Passavant Hospital, as well as others who were invited. At these sessions Fenger examined microscopic sections from his private patients, and sometimes those brought by visitors, commenting on the cases and giving instruction in microscopic diagnosis.[39]

His ability as a diagnostician was impressive and provided the young physicians who came in contact with him with a fund of stories to liven up their recollections of Fenger. Despite the difficulty of separating fact from fiction, the anecdotes are useful in that they do reflect aspects of Fenger's personality as a physician and a man. Herrick, the cardiologist, recalls the case of a woman in the medical ward that had puzzled him and

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[36] Hektoen, "Early pathology," p. 270.

[37] Buford, "Christian Fenger," p. 202; Hektoen, "Early pathology," p. 270.

[38] Some idea of Fenger's clinical presentation may be obtained from T. A. Davis, "Surgical clinic of Christian Fenger, M.D.," *Chicago Clinical Review*, 1893, *1:* 376; in Fenger, *Works*, vol. 2, pp. 590-598.

[39] Buford, "Christian Fenger," p. 202.

Ludvig Hektoen, another of Fenger's students who succeeded him as pathologist at the Cook County Hospital. They consulted Fenger, who examined the patient briefly, made the diagnosis of multiple myelomata, and walked off. The two young men looked at each other and Herrick asked "What in the world is a myeloma?" "Damned if I know," Hektoen replied, "but I'm going to find out." He did and they reported the case.[40]

Herrick also describes how when he was senior house surgeon, he assisted Fenger in an operation for hernia. Evidently puzzled by the unusual relation of the anatomical landmarks, Fenger tried to elucidate the situation. Suddenly his face lit up and he said, "I know now. This is a properitoneal hernia of Krönlein," and went on to explain some of the peculiarities of the condition. In the course of the operation he accidentally cut the spermatic cord. "Ha," he commented, "that testicle will not be worth much to that man, but he has a good one on the other side." After the operation while changing his clothes, Fenger continued his analysis of the case and the operation with his young colleagues. "It was a sight typical of many other postoperative occasions," says Herrick, "to see him stripped to the buff or in his underclothes, smoking his cigar, talking of the hernia described by Krönlein, drawing it in diagram on paper, walking about the room, answering questions. . . . Surely we boys were receiving inspiration and an education though we may not have been at that time fully aware of the significance of it all."[41]

Clearly, Fenger taught both by precept and example. Not only did he endeavor to arouse intellectual curiosity, and to demonstrate how to think logically in medicine, but he also set an example of application, responsibility and hard work. It was not uncommon for him to begin operating at one hospital at 8 a.m., then proceed to two others later in the morning and the early afternoon, reach his office at 3 p.m., have a light lunch and see private patients. Possibly there were consultations in the late afternoon. After dinner and some time with his family, he retired to his library at about 9 or 10 p.m. where, smoking cigars and drinking lemonade, he read, preparing for his clinical lectures or operations. For these he also drew diagrams and charts which he used for demonstration.[42]

His assistants helped Fenger with private patients, writing histories, making preliminary examinations, doing clinical laboratory work, taking care of postoperative dressings, and accompanying him to consultations, operations in private homes, and out of town when necessary. The facilitation of the surgeon's work by means of organization was only in its early stages. According to Bayard Holmes, "Major operations were performed in private houses of every sort, and there was no such thing as a properly equipped operating room in Chicago." Under such circumstances the labor and the time involved in Fenger's activities was relatively greater than it would be today, and his assistants learned to do likewise. Not only were they encouraged to work hard, but to learn as well that this involved reading, investigation, and a thorough grounding in the nature of any problem with which they dealt. Just as Fenger did not hesitate to admit an error or to acknowledge ignorance, so he inculcated the principle of intellectual honesty in his students.[43]

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[40] Herrick, "Christian Fenger," p. 323.

[41] *Ibid.*, p. 321.

[42] Buford, "Christian Fenger," p. 201-202.

V

When Fenger was sixty in 1900, some 500 physicians comprising students and friends honored him by a testimonial dinner, thus indicating his outstanding influence in Chicago and the Middle West. On March 7, 1902, at the age of 62 he died of pneumonia. Fenger lived in Chicago 24 years during a crucial transition in American medicine. During this period he played a central role in advancing medical science and practice from the rather primitive state in which he found it when he arrived in 1877 to a point where by the end of the century Chicago was being recognized as an important medical center. Fenger brought to the Middle West the advanced ideas and practices of scientific medicine in which he had been trained in Europe, and found that many medical men in Chicago were prepared to learn from him. In transmitting professional and scientific knowledge and attitudes, Fenger's personality and character increased his influence. His kindness, friendliness, honesty, modesty and lack of dogmatism, his competence as well as his deep-rooted devotion to the search for truth gained him the admiration and loyalty of many students and practitioners. His students included eminent surgeons such as J. B. Murphy, W. J. Mayo and Lewis L. McArthur, noted pathologists among whom were Ludvig Hektoen, H. Gideon Wells, E. R. Le Count and Howard T. Ricketts, and internists of whom the best known are Frank Billings and James B. Herrick.

According to Esmond Long, the influence of Fenger on pathology in the West was as profound as that of Welch in the East, although in a different way.[44] Thomas Bonner has described him in a sense as the Osler of the Middle West.[45] These comparisons have a certain aptness, for Fenger's position in the history of American medicine is based on original contributions to medical knowledge derived from an extensive pathological and clinical experience, as well as far-reaching influence as a teacher. But Christian Fenger may also be seen as an immigrant pioneer whose efforts played a major role in opening up the Middle West to modern scientific medicine.

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[43] Hektoen, "Early pathology," p. 270; Herrick, "Christian Fenger," pp. 323-324.

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[44] Long, *American Pathology*, p. 172.

[45] Bonner, *Medicine in Chicago*, p. 88.

CORRESPONDENCE AND REPORTS

COMMENTS ON "BERLIN CORRESPONDENCE"

After the appearance of his article, "The Berlin Correspondence in the JAMA During the Hitler Regime" (*Bull. Hist. Med.*, 1973, *47*: 297-305), Dr. Harry Bloch received letters commenting on the article. Of particular interest are the following excerpts.—Ed.

I was editor of the *Journal of the American Medical Association* (JAMA) during the entire period covered by your paper. Our Berlin correspondent at that time was Dr. Paul Wolff, who was Jewish by birth but whose father, who was a physician, had converted as did also Paul Wolff. I visited Dr. Wolff in 1931 at which time he shared editorship of the *Deutsche medizinische Wochenschrift* with a German who later informed the Hitler government of Wolff's original ancestry so that Wolff had to flee Germany. He went first to Geneva, Switzerland, and then to Buenos Aires. In Buenos Aires he became associated with narcotics control and still later returned to Geneva, Switzerland, where he worked for the World Health Organization in the narcotics field. He died some years ago. He was accurate in his letters to the *Journal* and we published them exactly as they were sent, which may have played a part in his ultimate abandonment of life in Germany. Incidentally, in 1940 the German government forbid the entrance of the JAMA into Germany, and it was not again permitted until after the war.

I was associated at that time with encouraging refugee physicians in their desire to practice medicine throughout the world. We did have problems with physicians who assumed the papers of executed doctors and came as spies to various portions of South America. By 1936 it was difficult to secure from any German university or medical school any information about Jewish graduates. A compilation of the lists and lives of these refugee physicians who began to emigrate from Germany as early as 1933 is a list of tragedies almost unbelievable.

Morris Fishbein, M.D.
Editor
Medical World News

I spent several months in Europe in my formative surgical years, several of which were spent in Germany (1927-28). I came to know many young surgeons who in later years became distinguished professors of surgery. In Heidelberg I came to know well a chap with whom I corresponded periodically throrgh the years. He later became a famous professor in Munich. He and his wife visited Sally and me here, and we in turn visited them in Munich. One day during such a visit we took the tram downtown and got a taxi and drove out to Dachau where the gas chambers were. At dinner that evening our host protested how could there be such a place only five or six miles away. It is impossible!

Owen H. Wangensteen, M.D.
Regents' Professor of Surgery
University of Minnesota

ANNOUNCEMENTS

NATIONAL NEWS

American Institute of the History of Pharmacy

In collaboration with the University of Wisconsin School of Pharmacy, the Institute will host a "Teachers' Conference on Newer Methods and Media in Teaching the History of Pharmacy" to take place August 2-3, 1974, on the University of Wisconsin-Madison campus. The program will include discussions by Glenn Sonnedecker and John Parascondola of Wisconsin on the application of multimedia and audio-tutorial teaching techniques, and Robert Mrtek and his colleagues from the University of Illinois will demonstrate the use of the laboratory in teaching the history of pharmacy.

Clio Medica

For three years Dr. Erna Lesky has served as editor of *Clio Medica,* the journal of the International Academy of the History of Medicine. Dr. Lesky has now given up her editorship and at the last meeting of the International Academy (September, 1973, in Vienna), Dr. Lester S. King was chosen to succeed her. The first issue under his direction is that of March, 1974. *Clio Medica,* although the official journal of the Academy, opens its pages to all medical historians. Contributions on social, cultural, literary, and philosophic aspects of medicine are particularly welcome. Manuscripts may be submitted in English, German, or French. The editorial office is c/o American Medical Association, 535 North Dearborn Street, Chicago, Ill. 60610. Manuscripts and books for review should be sent directly to Dr. King at that address.

College of William and Mary in Virginia

The Department of History in conjunction with the Institute of Early American History and Culture. the Earl Gregg Swem Library, and the Department of Anthropology, is offering 14-month, full-time apprenticeship programs in editing historical books and magazines, operation of a historical library, and historical archaeology. The programs run from July 1 of each year until August 31 of the succeeding year. Some financial assistance is available. Further information can be had from the Director of Graduate Study, Department of History, The College of William and Mary, Williamsburg, Va. 23185.

Joint Atlantic Seminar for the History of Biology

The tenth annual meeting of the Joint Atlantic Seminar was held at the University of Pennsylvania, Philadelphia, on March 16, 1974. Papers were presented by Howard M. Smith, Daniel Todes, Jonathan M. Silver, Nancy Mautner, Alice Levine Baxter, Douan B. Kottler, Pauline Mazumdar, and Theodore J. Kalikow. The day-long session closed with a discussion: "Directions in Teaching and Research in the History of Biology."

Medical Historical Film

A new historical motion picture, *Ambroise Paré, Military Surgeon* (19 minutes), has recently been produced at the Yale School of Medicine. The 16 mm. sound film, in color, which was shown at the Charleston meeting of the American Association for the History of Medicine, considers sixteenth-century French wound surgery, describes Paré's important innovations in the treatment of battle injuries as well as in diet and rehabilitation, and presents a detailed case history. Contemporary pictures of surgical procedures, instruments, injuries, and of battle scenes illustrate the narration.

The film is especially designed to interest medical and nursing students, members of the health professions, biologists, and medical historians. Made under a grant from Teledyne Aqua Tec, the motion picture was produced by Dr. Thomas R. Forbes, Department of Anatomy, and directed by Miss Susan Wheeler, Communications Media Group, Yale University School of Medicine, New Haven, Conn. 06510. Prints at $105 each may be ordered from the Communications Media Group.

Two other films in sound and color, *John Hunter—Enlightened Empiricist* (12 minutes, $60) and *Vesalius, Founder of Modern Anatomy* (14 minutes, $70) are also available from the same source.

National Library of Medicine

The N.L.M. has announced an exhibit, "Witchcraft and Medicine," to be on view March 25-July 19, 1974. Compiled from the library's historical collections, the exhibit includes books as well as reproductions of woodcuts and engravings dating from 1484, the year of Pope Innocent VIII's bull against witchcraft, to 1793, when the last witch burning took place in Poland.

Ohio Academy of Science

The annual History of Science Meeting of the Ohio Academy of Science and the Ohio Academy of Medical History took place March 16, 1974, at Ohio State University, Columbus. The morning session was devoted to a symposium, "Medical Botany during the First Half of the Nineteenth Century in Eastern United States." Participants were George E. Gifford, Jr. (New England area), Dorothy Lansing (Philadelphia area), and Eugene H. Conner and Ronald L. Stuckey (Ohio Valley). Emanuel D. Rudolph was moderator. John Burnham presided over the afternoon session which included papers by Ralph W. Dexter, Stanley L. Block, Mrs. Katherine T. Barkley, Mrs. Lael Ely Bradshaw, and Cecil Striker.

Rare Book Acquisitions

The Owen H. Wangensteen Historical Library of Biology and Medicine, Bio-Medical Library, University of Minnesota, has acquired a fine copy of the first edition of Andreas Vesalius, *De humani corporis fabrica libri septem*. The volume was presented by Mr. Earl Bakken, Chairman of the Board of Medtronic, Inc., and founder of the Medtronic Archiv on Electrostimulation. Mr. Bakken's gift is an outstanding addition to the Wangensteen Library's rich collection of anatomical works. The gift was accepted for the library by Minnesota President Malcolm Moos at a ceremony held on January 21, 1974.

The New York Academy of Medicine is now in possession of the library of rare books of Dr. Robert Levy. Reflecting its eminent donor's interest in cardiology, the collection is especially strong in the writings of William Harvey, above all a superb copy of the rare first edition of Harvey's *Exercitatio anatomica de motu cordis et sanguinis in animalibus*. With the addition of the Levy Collection, the Academy now has one of the greatest Harvey collections in the world. The new accessions will be housed in the Rare Book Room and will soon be placed on display.

Smithsonian Institution

The Smithsonian Institution Seminar Series in Paleopathology were conducted in the Department of Anthropology, National Museum of Natural History, January 6-March 15, 1974. The program was under the directorship of Donald J. Ortner. Lecturers, in addition to Dr. Ortner, were J. Lawrence Angel, Saul Jarcho, Lent C. Johnson, Paul H. Keyes, Massond Majd, Walter G. J. Rutscher, T. Dale Stewart, Elias G. Theros, Douglas H. Ubelali, E. A. Uehlinger and David V. Von Endt.

University of Kansas

The Jager Room in the Clendening Medical Library was dedicated on February 23, 1974. The room will house the generous gift of Dr. Thor Jager, a library of over 1,000 rare books, manuscripts and letters. The collection is composed largely of historical works on pathology. Of special interest are books, papers, and manuscripts which belonged to Rudolph Virchow.

University of Wisconsin

On March 27, 1974, Guenter B. Risse chaired an interdisciplinary symposium, "Experimentation and Health Care: Rights and Responsibilities," sponsored by the Department of the History of Medicine and Its Program in Medical Ethics. Papers were presented by George Rosen, John A. Robinson, Norman C. Fost, and David Mechanic. A panel discussion, moderated by Dr. Rosen (History), included Professors Robertson (Law) and Mechanic (Sociology), and Dr. Robert E. Cooke (Medical Ethics).

Award

Dr. David Musto, Professor of Psychiatry and History at Yale University and author of *The American Disease: Origins of Narcotics Control,* on December 9, 1973, was presented with the annual Research Award of AVANT—The Association of Voluntary Agencies on Narcotics Treatment.

Opening for Visiting Lecturer

The Institute for the History and Philosophy of Science and Technology at the University of Toronto has an opening (created by a leave) for a visiting lecturer for 1974-1975 (and possibly longer). Duties will include teaching (graduate/undergraduate) and research. Salary will be near the lower end of the scale for Assistant Professors, but is negotiable. Quality of the applicant will be given greater weight than subject preferences, but the Institute is interested in extending its coverage broadly in times, disciplines, topics, and aspects of science and technology. Applicants with Ph.D.'s in or nearly in hand should write to Professor K. O. May, Director, IHPST, University of Toronto, Toronto, Ontario M5S 1A1. Include a complete vita, a statement of research interest and teaching subjects, and three references whom you have asked to send letters of recommendation.

PROGRAMS OF MEETINGS

Bay Area History of Medicine Club, San Francisco

October 16, 1973: Dinner meeting, co-sponsored by The Janus Foundation. "Early American Medical Literature," John B. Blake.

November 14, 1973: "Privileged Communications," Ilza Veith; "History of Chess as Therapy," Norman Reider.

January 16, 1974: "On the Fringes of Medicine for Two Hundred Years in California," Kevin McGettigan.

February 13, 1974: "Incidents in the History of High Altitude Physiology," Ralph A. Kellogg.

College of Physicians of Philadelphia, Section on Medical History

February 11, 1974: "Surgery at the University of Pennsylvania: Personal Recollections of Holders of the Johns Rhea Barton Professorship of Surgery," Robert H. Ivy.

March 12, 1974: Dinner in honor of Samuel X. Radbill. "Medicine and Pharmacy in French Political Prints," William H. Helfand.

George W. Corner History of Medicine Society, Rochester, N.Y.

January 10, 1974: "'Once Taken, Never Forsaken.' A Short History of the J. C. Ayer Co.," John King.

February 21, 1974: "Joseph Babinski Fails His Examination," Richard Satran.

March 5, 1974: "In Pursuit of the Fugitive, or The Importance of Medical Ephemera to the Historian," Gertrude Annan.

Handerson Medical History Society, Cleveland, Ohio

January 14, 1974: "Pharmacy in the Western Reserve 100 Years Ago: A Demonstration of Equipment from the Dittrick Museum," Patsy A. Gerstner.

March 4, 1974: "Indigenous Medicine of the Yorubas," Solomon Oyesola.

Johns Hopkins Medical History Club, Baltimore, Md.

March 13, 1974: "Dr. John Shaw Billings, Versatile Savant of American Medicine," Martin M. Cummings.

Josiah C. Trent Society for the History of Medicine, Durham, N.C.

March 21, 1974: Annual Gardner Lecture in the History of Surgery, "Surgery in the Middle Ages," Charles Talbot.

University of Virginia Medical History Society, Charlottesville, Va.

February 21, 1974: "Early American Medical Literature: Medical Americana to 1800," John B. Blake.

March 25, 1974: "Medical Care in Hospitals During the Middle Ages," Charles H. Talbot.

Washington Society for the History of Medicine, Washington, D.C.

January 17, 1974: "States Rights Medicine: A Southern Medicine for Southerners," John Duffy; "The Santa Fe Trail in 1860 from the Diaries of a Military Surgeon," Peter D. Olch.

March 14, 1974: "The Younger John Winthrop (1606-1676) as a Collector of Medical Books," Ronald S. Wilkinson; "The Quest for a National Health Department, 1906-1912," Manfred J. Waserman.

William Snow Miller Medical History Seminar, University of Wisconsin-Madison

February 20, 1974: "Oral History Project in Wisconsin Medicine," Guenter B. Risse, Donna Taylor, Joan Waite.

March 20, 1974: "Rural Medical Practice in Medieval England," Phil Teigen.

NEWS FROM ABROAD

Vienna

The International Academy of the History of Medicine held its fourth Symposium in Vienna, September 17-19, 1973. The theme of the Symposium was "Vienna and World Medicine" and the lectures presented concerned the medical relationship of several European countries with Vienna. Medical historians from ten European countries contributed papers: M. Florkin (Belgium), V. Moller-Christensen, E. Snorrason (Denmark); P. Huard, M. Klein, J. Théodoridès (France); N. Mani, K. E. Rothschuh, A. Terzioğlu (German Federal Republic); J. K. Crellin, F. L. N. Poynter (Great Britain); L. Belloni, L. Premuda, B. Zanobio (Italy); G. A. Lindeboom (Netherlands); S. Szpilczynski (Poland); A. Albarracín-Teulon, P. Lain Entralgo, J. M. López Piñero (Spain); and L. Glesinger (Yugoslavia). Contributors from the United States were George Rosen and Rudolph Siegel. Officers of the Academy are Noel Poynter (President), Erna Lesky (Secretary), and M. D. Grmek (Treasurer).

BOOK REVIEWS

Martin Kaufman. *Homeopathy in America: the Rise and Fall of a Medical Heresy.* Baltimore: The Johns Hopkins Press, 1971. x+205 pp. $10.00.

A modern history of homeopathy, as Charles Rosenberg pointed out ten years ago, has long been needed. Martin Kaufman, a student of Professor John Duffy, has now given us one very important part of this history: *viz.*, the relationship between the orthodox medical profession and the homeopaths from the 1820's to virtually our own time. It is the story, to a large extent, of every unorthodox sect, as Kaufman notes. Beginning with a good description of the heroic medicine practiced in the early nineteenth century, Kaufman manages to use different sources than those used by earlier historians going over the same ground. He then discusses the rise of the homeopaths, who in most respects replaced the Thomsonians as the leading unorthodox group of American medical practitioners. As Professor Shryock has said, the shift in the status of homeopathy from the dignity of a system to the heresy of a sect, was a turning point in American medical thought of the nineteenth century.

Not only does Kaufman very clearly portray the problem posed for the regular profession by homeopaths, but he also vividly describes the division in the ranks of the homeopaths themselves. Beginning in the 1860's, a split between the pure Hahnemannians and the eclectics begins to become a heated argument at times. What Kaufman does not elaborate upon is that a similar split may be seen in the history of almost all sects, as illustrated by the fight between the ten-finger osteopaths, who stressed the importance of a laying on of hands and manipulation, and those derisively referred to as the three-finger osteopaths, who used their digits to work a syringe.

Other parts of the history of homeopathy have an all too familiar ring. Early in the twentieth century, some doctors attempted to merge the systems of allopathy and homeopathy. The editor of one of the most influential national journals, Dr. George Simmons of the *J.A.M.A.*, had been a homeopathic physician who turned to orthodoxy. His denunciations of his former sectarian colleagues only served to stiffen the position of those homeopaths who wanted to maintain their integrity and autonomy. The succeeding fight within both parts of the medical profession is very well told. As late as 1911, Dr. William Tod Helmuth noted in his *Encyclopedia Britannica* article on homeopathy that it was in the United States that the system "principally flourishes." So there is ample reason for a book on homeopathy and this is a welcome one. Dr. Helmuth, incidentally, a leading homeopathic practitioner and educator in New York city, is strangely absent from Kaufman's account. Nor is mention made of Paul Wershub's *One Hundred Years of Medical Progress,* a mistitled book which nevertheless is in part a valuable account of the New York Medical College Flower and Fifth Avenue Hospitals, once an important homeopathic institution.

A final comment needs to be made, however, lest future historians of this subject or perhaps Professor Kaufman himself be discouraged from still further work.

This book is a social historian's look at homeopathy as a sect, how it managed to flourish and grow, and it discusses the internal and external battles that ensued. There are a number of things we do not learn from this book: Just how exactly did homeopathic practitioners practice? How did they dilute their drugs, and how did they convince their patients of their efficacy? What sorts of fees did they charge? What was the curriculum in their schools, and how did it compare to other schools? Did the homeopathic hospitals flourish and did they differ from other hospitals? These are just a few of the questions that remain to be answered. They are not raised in criticism of the present book—rather, to stimulate further work.

Review by GERT H. BRIEGER, M.D., M.P.H., Ph.D., Associate Professor of Medical History at Duke University Medical Center, Durham, N.C.

Edgar A Kahn. *Journal of a Neurosurgeon.* With a Foreword by Bronson S. Ray. Springfield, Ill.: Charles C Thomas, 1972. viii+172 pp. Ill. $8.75.

This delightful little book presents the memoirs of a second generation neurological surgeon who for several decades taught neurological surgery at the University of Michigan Medical Center.

Dr. Kahn's interests, particularly in his earlier days, were wide. Coming from a family of architects, Dr. Kahn was early exposed to a professional career in that field but chose medicine. During his medical training, Dr. Kahn visited many places and met many persons of interest in the world before and after World War II. Of these travels he gives a rich and vivid account.

The trials and tribulations of a neurological surgeon in the early days of the development of the specialty are well described. Dr. Kahn tells stories of his many associates who worked with him, both as trainees and as colleagues. His surgical triumphs and failures briefly but vividly illustrate the work of the neurosurgeon in the early days. This homey discussion of the life of Dr. Kahn gives us an insight into the everyday experiences of the devoted neurological surgeon. His triumphs in some hopeless cases and his despair when things went wrong emphasize the courage the pioneering surgeons needed to maintain their activities.

As an American volunteer early in World War II, Dr. Kahn suffered frustrations and misfortunes during the German invasion of France and the retreat of the French populace and military services. Later, as a neurosurgeon with the American Army, he served with the University of Michigan Hospital Unit as Chief of Surgery. His varied experiences with war injuries make interesting reading. An episode which he describes of the removal and disposal of an unexploded 20 mm antiaircraft shell from the spinal canal of a wounded soldier illustrates his courageous temperament.

This autobiography by one of the outstanding and most energetic neurological surgeons of the last few decades cannot be considered a history of neurological surgery, although Dr. Kahn gives many glimpses into the development of brain surgery. The book may be of much more historical interest 50 to 100 years from now that it is at the present time. Many interesting anecdotes in Dr Kahn's life give a penetrating insight into twentieth-century neurosurgery as performed by a very humanistic individual who has experienced the triumphs of success and the despairs of failure in the operating room. Although the book has a rambling style, it proceeds at a rapid pace, so typical of the author whose thoughts come tumbling out much faster than he can articulate them. The book is wholeheartedly recommended to both physicians and laymen who may be interested in reading of the experiences of a neurosurgeon in the formative years of his specialty.

Reviewed by A. EARL WALKER, M.D., Professor Emeritus of Neurological Surgery, The Johns Hopkins University School of Medicine, Baltimore, Md.

A. M. Cooke. *A History of the Royal College of Physicians of London.* Vol. 3. Oxford: Clarendon Press for the Royal College of Physicians, 1972, xxiv, 801-1247 pp. Ill. $17.00.

In this third and concluding volume Dr. Cooke completes the task which Sir George Clark took up in his first two. The history of the Royal College of Physicians is here traced from the Medical Act of 1858 to the National Health Service of 1948. Ten appendices are included, notable among them some copious extracts from Lord Moran's presidential addresses to the College in the years 1941 to 1948.

In the preface Dr. Cooke warns the reader that he is a "professed non-historian." This is excessively modest. For he has written a useful account of the College in the nineteenth and twentieth centuries. What is lacking, however, is Sir George Clark's happy union of social and institutional history: his skillful setting of the life of the College in the framework of English social and professional history. Dr. Cooke has kept too close to the College archives. Oddly, he appears not to have consulted such basic works as those by Charles Newman on medical education, Brian Abel-Smith on English hospitals since 1800, and Richard Titmuss on the wartime origins of the National Health Service.

Dr. Cooke's parochial treatment of the College and its history is probably rooted in his conservatism. It is plain that he does not take kindly to medical and social reformers. The word "fanatic," for example, is applied to Sir John Simon (p. 821). William Stead, the humanitarian journalist, is described as employing methods that were "unorthodox, crude, and unwise" (p. 907). And "radical reformers" generally are instructed that "the biological instinct of self-preservation is not easily suppressed" (p. 862). A more generous view of modern society and politics might well have helped Dr. Cooke take a broader view of his subject.

Reviewed by DAVID SPRING, Ph.D., Professor of History, The Johns Hopkins University, Baltimore, Md.

Esther Fischer-Homberger. *Hypochondrie.* Melancholie bis Neurose. Krankheiten und Zustandsbilder. Mit einem Geleitwort von Professor Dr. med. E. H. Ackerknecht. Bern: Verlag Hans Huber, 1970. 152 pp. Front. Fr. 25/DM 23.

This book presents—although not clearly indicated by its title—a far-reaching historical study of the manifold interpretation "Hypochondriasis" has received in the course of centuries. Following a short opening chapter, where the polyglot author deals with antiquity, especially with the work of Galenus, the main parts are dedicated to the later development during the 17th, 18th, and 19th centuries. On a few concluding pages, reference is made to Breuer and Freud.

Dr. E. H. Ackerknecht summarized on an "Introductory Page" Esther Fischer-Homberger's main contribution to this topic of medical history as follows: "Esther F.-H. has discovered the surprising fact, that during the 17th century Melancholia, thanks to the devaluation of 'Black Gall', was no longer interpreted as a somatic disease, but as a psychiatric condition, (Zustandsbild); however, a new disease 'Hypochondriasis' appeared on the scene, where the familiar complex of symptoms is once more interpreted as a somatic disturbance . . . . During the 19th century, another mutation occurred. . . The disease 'Hypochondriasis' was replaced by nosophobia, while the role of hypochondriasis . . . was usurped by hysteria and after the middle of the century by neurasthenia. Finally, another interpretation occurred, where neurasthenia was transformed into a Freudian neurosis, i.e. to a sex-psychogenetic disorder . . . ." This study demonstrates, writes Ackerknecht, that clinical titles and symptoms, pathologies, do not as a rule, certify objective entities, yet present rather reflections of our own modes of thinking and knowing.

Reviewed by ERWIN W. STRAUS, M.D., Clinical Professor of Psychiatry, University of Kentucky, Lexington, Ky.

John W. Florin. *Death in New England: Regional Variations in Mortality.* Chapel Hill: University of North Carolina, Department of Geography, 1971 (Studies in Geography, No. 3). viii + 172 pp. Ill. $4.00.

This book is an effort to answer essentially epidemiologic questions, as the title indicates. The approach is through elaborate statistical technics developed principally by geographers. The results are largely depicted by cartographic illustrations. The author has read widely on the subject and done a great amount of work with a vast accumulation of mortality data. The results are not impressive; the available data are perhaps too crude to warrant the involved statistical and analytic procedures employed.

There is first a description of New England in terms of demographic characteristics in successive periods, presented in a series of maps; then a descriptive account of disease problems and control measures in the seventeenth and early eighteenth centuries; then a trend surface analysis and mapping of mortality covering the period from 1750 to 1850 for selected Massachusetts towns. Lacking age-adjusted mortality rates for this early period, the average age of death was used as an index to the force of mortality This is of dubious value, contrary to some quotations in the book which imply that it is equivalent to the expectation of life; only in a completely stationary and unchanging community would this be true. The age of the living—affected as it is by migration and prior age-selective mortality, as well as the current force of mortality—helps to determine the average age of death. Thus if there has recently been extensive migration of young families with many children into an area the average age at death will be lowered even if the force of mortality is unchanged. An unfortunate omission is the failure to state which towns, or even what number of towns, were used in the analysis. The author finds that the Labovitz model of the "economic development of a society based on temporal variations in the level of territorial differentiation" fits the data, but that the small towns around Boston, rather than Boston itself, were the seminal points for an improving mortality pattern.

The following sections deal with the pattern and components of mortality change since 1850, and do employ mortality rates. Data presentation is mainly in the form of cartograms with the relative size of each political unit based on population size rather than areal extent. To this reader the technic as employed in this book is confusing, and not at all the best way of presenting the findings. "Principal components analysis," used frequently in geography in recent years, is the analytic method chosen because of its use in "heuristic analyses of complex, multidimensional phenomena, with principal components analysis employed to produce some parsimony of description through the identification of basic dimensions within the data matrix." The reviewer was unable to follow the explanation of the method and has some doubt that others without prior experience with it could do so. The author concludes that after a point of maximum spatial differentiation in 1880, spatial differentiation has diminished markedly. The diffusion pattern, as in the earlier periods, originated in the suburban south and spread into the cities and north into the more rural areas. The suburban areas surrounding major urban centers were the first to show a decline in mortality, suggesting that they shared the economic and medical benefits of the cities, without suffering the deleterious effects of the urban environment. These conclusions are of considerable interest, but it is not easy to understand how they were reached, owing to the failure to present the original data and to the complicated analytical methods.

There are a number of typographical and spelling errors, and some errors of fact; for instance, on p. 42 observations by Noah Webster are attributed to Daniel Webster, and cholera and typhoid fever are characterized as diseases of early infancy on p. 103. At some points the author is uncritical in his acceptance of views quoted from other workers. Thus, he quotes on p. 123 a list of five causative factors in diabetes, taken from the 1935 edition of a textbook; only one of these factors (obesity) is today widely regarded as being of importance, and later editions of the same textbook deal with the etiology of diabetes in a better way.

It should be granted that this monograph attempts a herculean task—perhaps an impossible task, considering the paucity of reliable population data until recently, the changes in nosography, the gross incompleteness and inaccuracy of death certification during the period, and the extreme complexity of the factors which influence the mortality experience of human populations. A less ambitious approach, embodying a less diffuse hypothesis, with simpler methods, might have succeeded better.

Reviewed by PHILIP E. SARTWELL, M.D., Professor of Epidemiology, The Johns Hopkins School of Hygiene and Public Health, Baltimore, Md.

Thomas Rogers Forbes. *Chronicle from Aldgate. Life and Death in Shakespeare's London.* New Haven and London: Yale University Press, 1971. xx+251 pp. $10.00.

The population of England grew significantly in the sixteenth century. By 1558, when Elizabeth I became queen, the old city walls of London could no longer contain its burgeoning population. Since walls as a defense device had become obsolete the city undertook no further protective encirclements, forcing many people to locate outside the walls. This overflow spread to the adjacent areas, the well-to-do erecting their houses, often elegant and pretentious, westward along the Thames toward Westminister. The poorer classes settled beyond the east wall in the vicinity of St. Catherine's Docks and, in substantial numbers, without Aldgate in the Ward of Portsoken. For this latter community the parish church of St. Botolph-without-Aldgate was the hub around which their religious and social life revolved. It is from a series of record books maintained by clerks of this parish and preserved among the manuscripts of London's Guildhall Library that Professor Thomas Rogers Forbes of the Yale University School of Medicine has selected the basic materials for *Chronicle from Aldgate.*

These records, covering the years 1558 to 1625, are far more complete than the parish registers kept in most English parishes after 1538. The clerks, whose names have survived in most instances, were alert and observant of the entire pageant of human affairs, noting not only births, marriages and deaths, commonly entered in such monuments, but also the conditions of life and the consequences of human frailty and folly. They have provided a catalog of trades and professions practiced in Tudor England and, in cases for which occupational identification of a parishioner was impossible, they have usually assigned a social rank, as e.g., gentleman, widow, singlewoman, common drunkard, roisterer, beggar, masterless man, rogue and vagrant. Identifications of fatal diseases and accidents abound.

It is doubtful that St. Botolph's-without-Aldgate should be considered a typical London parish. Although it could boast of some prosperous burghers it appears to have encompassed more than its share of the wretched, the hopeless, the down-and-out. Its social problems were pervasive and lingering. John Stow, whose knowledge of Elizabethan London was unsurpassed, described Aldgate simply as "that horrid entrance to the City." Furthermore, he wrote, "[It] is no small blemish to so famous a City to have so unsavory and unseemly an entry or passage thereunto. . ." He saw the area outside the gate as a place of "filthy cottages . . . inclosures and Laystalles [dungheaps]." Life was harsh and graceless. The parish housed hundreds of working people who staffed the shops and stalls of the inner City. Its proximity to the docks of the Thames brought sailors, pirates and mere vagrants into its streets. Poverty, disease, violence and death were always close at hand. Coroners were busy men.

Professor Forbes has selected as the basis of his book hundreds of entries from the St. Botolph registers. These he has spliced together with his own cogent comments and organized around some six themes or subjects, *viz.*, (1) trades and occupations, (2) vital statistics, (3) diseases and public health, (4) pestilences, (5) accidents and violence, and (6) care of the indigent sick. There is patently something of value for a variety of readers, but especially for demographers and students of the history of medicine and public health. The author's statistical tabulations and graphs are very useful.

Professor Forbes did not approach his task without careful preparation. The appended booklist, copiously cited within his text, indicates a cognizance of and reliance upon much of the significant literature in demography and medical history. He was privileged to consult established scholars in both England and America. It was pleasing for the reviewer to note in the preface a special acknowledgement for the assistance rendered by Dr. A. E. J. Hollaender, Keeper of the Manuscripts at the Guildhall, who through his generous assistance has endeared himself to so many American scholars.

This is an engaging book, even for the layman or casual reader, who is certain to enjoy this useful and pleasant introduction to an important historical source. A glossary of archaic or otherwise troublesome terms has been provided. For the more serious student of the medical and social history of Tudor England the book presents through the innumerable excerpts from the sources and the author's commentary a digest of an immensely valuable historical document. The illustrations, which include maps of East London for the years 1572 and 1604, a 1636 plague bill, and seven pages of facsimile reproductions from the parish registers themselves, add significantly to the interest of the book.

The flaws in the book are few indeed. This reviewer would have preferred the conventional footnote mechanics employed in the social sciences to the citing parenthetically of author and date of publication commonly used in journals devoted to the so-called "hard sciences." The principal appeal of this work will doubtless be for the historian.

Reviewed by E. A. HAMMOND, Ph.D., Professor of History, University of Florida, Gainesville, Fla.

THE BULLETIN RECENTLY RECEIVED *

Erwin H. Ackerknecht. *Therapeutics, from the Primitives to the 20th Cenutry.* With an Appendix: History of Dietetics. New York: Hafner Press; London: Collier Macmillan, 1973. x + 194 pp. $12.50.

John P. Bennet. *Chemical Contraception.* New York and London: Columbia University Press, 1974. 230 pp. Ill. $20.00.

Audrey B. Davis. *Circulation Physiology and Medical Chemistry in England, 1650-1680.* Lawrence, Kan.: Coronado Press, 1973. vii + 263 pp. Ill. $8.50.

Stacey B. Day (Editor). *Ethics in Medicine in a Changing Society.* Minneapolis: University of Minnesota Medical School, 1973 (Symposium Proceedings of the Bell Museum of Pathobiology, University of Minnesota Medical School, Jan. 19 and 26, and Feb. 2, 1973). 72 pp. $4.50.

Stacey B. Day. *Tuluak and Amalulik.* Dialogues on Death and Mourning with the Inuit Eskimo of Point Barrow and Wainwright, Alaska. (With the editorial assistance of Thomas Dillon Redshaw). Minneapolis: Bell Museum of Pathobiology, 1973. xv + 176 pp. Ill. $3.75 (paperbound), $6.50 (case bound).

Michel Foucault. *The Birth of the Clinic.* An Archaeology of Medical Perception. Translated from the French by A. M. Sheridan Smith. New York: Pantheon Books, 1973. xix + 215 pp. $8.95.

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

Patsy A. Gerstner. *The Care and Exhibition of Medical History Museum Objects.* Cleveland: The Cleveland Health Sciences Library, 1974. (Health Sciences Information Series, vol. 1, no. 6). ix + 47 pp. $1.75.

José María López Piñero. *El análisis estadístico y sociométrico de la literatura científica.* Valencia, 1972 (Cuadernos de Documentación e Informática Médica, I). 82 pp. Ill.

John P. McGovern and Chester R. Burns (Editors). *Humanism in Medicine.* With a foreword by Wilburt C. Davison. Springfield, Ill.: Charles C Thomas, 1973. xv + 113 pp. Ill. $9.75.

Christian Meyer. *Ludwig Wills (1834-1912). Sein Leben und sein Werk, in besondere in seiner Bedeutung für die Entstehung der klinischen Psychiatrie in der Schweiz.* Zürich: Juris Druck & Verlag, 1973 (Zürcher medizingeschichtliche Abhandlungen, N.R. Nr. 97). 43 pp. Fr. 10.

William B. Ober (Editor). *Great Men of Guy's.* With a foreword by Lord Brock. Metuchen, N.J.: Scarecrow Reprint Corp., 1973 (New York Academy of Medicine, History of Medicine Series, No. 42). xxiv + 392 pp. Ill. $12.50.

E. D. Phillips. *Aspects of Greek Medicine.* New York: St. Martin's Press, 1973. 240 pp. Ill. $12.95.

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NEW
FROM CALIFORNIA

Medicine in Seventeenth-Century England
A Symposium Held at U.C.L.A. in Honor of C. D. O'Malley
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SCIENCE, POLITICS AND SPONTANEOUS GENERATION IN NINETEENTH-CENTURY FRANCE: THE PASTEUR-POUCHET DEBATE

JOHN FARLEY[*] AND GERALD L GEISON[**]

Introduction

Between 1859 and 1864, Louis Pasteur engaged in a celebrated debate over spontaneous generation with Félix Pouchet, a respected naturalist from Rouen and a corresponding member of the French Academy of Sciences. Traditional accounts of this debate have focused almost exclusively on the experimental issues dividing the combatants.[1] In our view,

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[*] Professor Farley's research was supported by a Canada Council Research Grant and conducted in part in the Department of the History of Science at Harvard University He thanks both for their support and encouragement.

[**] Much of Professor Geison's research was carried out during a sabbatical year at the Institute of the History of Medicine, Johns Hopkins Medical School, where he was supported in part on a trainee grant from the National Institutes of Health. The rest of his support during that year was provided by Princeton University. A summer research grant from the History Department at Princeton also assisted his research. He wishes to thank these institutions and their personnel for their support and encouragement.

[1] See esp. William Bulloch, *The History of Bacteriology* (London: Oxford Univ. Press, 1938), pp. 92-106; Emile Duclaux, *Pasteur: the History of a Mind*, trans. by Erwin F. Smith and Florence Hedges (Philadelphia: W. B. Saunders, 1920), pp. 85-111; and René Dubos, *Louis Pasteur: Freelance of Science* (Boston: Little, Brown, 1950), pp. 165-177.

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this approach ignores the very real significance of the extra-scientific, political aspects of the debate. It illegitimately suggests that the debate was resolvable solely at the level of experimental fact and that Pasteur's ultimate triumph can be ascribed entirely to his experimental skill and devotion to the true principles of the "experimental method." It is these assumptions which have filtered down into the modern biological community through those notorious "historical" sections which one finds in many elementary and not-so-elementary textbooks. Without much exaggeration, one can say that every one of these textbooks which finds room for some historical "humanism" gives an account of Pasteur's experimental victory over the myth of spontaneous generation.

And while Pasteur has been uncritically celebrated for his methodology, Pouchet has been castigated for his *a priori*, "metaphysical" approach, as proof of which repeated use has been made of the following passage from the preface to his *Hétérogenie:* "When by meditation it was evident to me that spontaneous generation was one of the means employed by nature for the reproduction of living things, I applied myself to discover the methods by which this takes place."[2] By quoting this passage, while making a passing allusion to the religio-philosophical implications of the debate, standard accounts have conveyed the impression that Pouchet's belief in spontaneous generation derived from his (presumably unorthodox) philosophical and political position. By reading beyond his preface, and by discussing what the objects of his "meditation" in fact were, we shall show how utterly misleading that impression is. More generally, we believe that our reexamination of the Pasteur-Pouchet debate reveals the direct influence of extrinsic factors on the conceptual content of serious science.[3] At the very least, we can add a new dimension to the standard accounts of that debate as we seek to refute the simplistic view that Pouchet "meditated" and was wrong, while Pasteur "experimented" and was right.

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[2] Félix A. Pouchet, *Hétérogenie, ou traité de la génération spontanée* (Paris, 1859), p. vi. This passage is quoted in Bulloch (n. 1), pp. 92-93; Duclaux (n. 1), pp. 104-105; and Dubos (n. 1), p. 165.

[3] Although assertions of social influence on the conceptual content of mature science are neither new (having long been associated with Marxist or quasi-Marxist historians) nor rare (indeed they are again quite fashionable), cogent demonstrations of such assertions have been conspicuous by their absence. A notable exception is Paul Foreman's recent study of the adoption of acausality by physicists in Weimar, Germany. See P. Foreman, "Weimar culture, causality, and quantum theory, 1918-1927: Adaptation by German physicists and mathematicians to a hostile intellectual environment," *Hist. Studies in Phys. Sci.*, 1971, *3:* 1-115.

The Scientific and Political Background to the Pasteur-Pouchet Debate

Although advanced in a variety of more or less sophisticated forms, the doctrine of spontaneous generation rests ultimately on the notion that living organisms can arise independently of any parent, whether from inorganic matter (abiogenesis) or organic debris (heterogenesis). Following an erratic historical career in which it long enjoyed the support both of natural philosophers and of Christian theology, only to be declared heretical by both in later eras, this doctrine reached its zenith of popularity during the first three decades of the nineteenth century, particularly in Germany when the early parasitologists and the *Naturphilosophen* argued forcefully in its favor.[1] In France, too, spontaneous generation received support through the writings of the materialist Cabanis, the transformist Lamarck, and the putative *Naturphilosophen* Geoffroy St. Hilaire and his student Antoine Dugès. But the popularity of spontaneous generation was short-lived in France. There, by its presumed association with the doctrines of materialism and transformism, it become not only scientifically discredited, but also politically, socially and theologically suspect.

This tendency to associate spontaneous generation with transformism derived in large measure from the eventual commitment of Lamarck and Geoffroy St. Hilaire to both notions. In the full-fledged version of his theory of transformism, Lamarck insisted that a continuous spontaneous generation was necessary in order to replenish the lowest forms which had evolved into more complex organisms. Without such continuous spontaneous generation, he argued, the earth would be devoid of primitive life. Especially after Geoffroy revealed his allegiance to similar ideas, the French tended to associate spontaneous generation with any evolutionary theory.

Beginning about 1802, Georges Cuvier launched a vigorous campaign against the doctrines of Lamarck and Geoffroy, culminating in his celebrated debate with Geoffroy in the 1820s and early 1830s. Most witnesses to this debate awarded the palm of victory to Cuvier. The scientific evidence which he marshalled against transformism is too well known to be elaborated here, but among its central features were his emphasis on discontinuities in the known fossil record; his widely acclaimed taxonomic scheme, which denied unity of type in favor of four independent *embranchements;* and his concept of the "correlation of parts," which restricted variation within narrow bounds and on the basis of which he produced impressive reconstructions of extinct organisms from one or very few surviving fragments.[5]

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[1] For details of this period see J. Farley, "The spontaneous generation controversy (1700-1860): The origin of parasitic worms." *J. Hist. Biol.*, 1972, *5*: 95-125

But Cuvier's attack gained additional force from less directly substantive sources. As Toby Appel's ongoing analysis of the debate emphasizes,[6] the outcome also turned on the question of scientific style, with Geoffroy seeking to defend the broadly philosophical aims of traditional natural history, while Culvier seemed to represent the sober, cautious position that science should deal only with strictly limited problems and "positive facts." In spite of this posture, Cuvier did not hesitate to buttress his scientific arguments against Geoffroy with religio-philosophical and political supports forged for him by his influential post in the *Académie des Sciences* and by events in the national arena. With the rise to power of Napoleon Bonaparte, followed by the Restoration under Charles X, Cuvier hastened to associate his opponents and their doctrines with the speculative and supposedly pantheistic *Naturphilosophie* of the German enemy and with the materialism of the late eighteenth-century *philosophes* and *idéologues,* on whom the public placed much of the blame for the chaos and terror of the French Revolution. It scarcely helped Geoffroy or spontaneous generation that Cabanis, a known advocate of the doctrine, had also been a major figure in the educational program initiated by the National Assembly during the Revolution.[7] Nor does it seem to have made much difference that Geoffroy repeatedly and explicitly tried to dissociate himself from *Naturphilosophie,* materialism and impiety.[8] Whether consciously or not, Cuvier and much of the public displayed a convenient disregard for the complexity of the relationships among spontaneous generation, transformism, pantheism, *Naturphilosophie* and materialism. What mattered was the public perception and *belief* that spontaneous generation somehow belonged with these politically and religiously dangerous doctrines, and ought therefore to receive its share of guilt for the turmoil of the recent past.

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[5] See W. Coleman, *Georges Cuvier: Zoologist* (Cambridge: Harvard Univ. Press, 1964).

[6] T. Appel, *The Geoffroy-Cuvier Debate and the Structure of Nineteenth-Century French Zoology* (unpublished Ph.D. thesis, Princeton University, in preparation).

[7] L. Pearce Williams, "Science, education and the French Revolution," *Isis,* 1953, *44:* 311-330.

[8] For examples of Geoffroy's attempts to defend himself from charges of allegiance to *Naturphilosophie,* materialism and impiety, see *Notions synthetiques,* pp. 26, 33, 82, 110; *Comptes Rendus,* 1837, *5:* 183-194; *ibid.,* 1839, *7:* 489-491; and "Hérésies panthéistiques," *Dictionnaire de la conversation et de la lecture,* 1836, *31:* 481ff.

A generation later, when Louis Pasteur waged his famous battle against spontaneous generation, the scientific and political situation bore a remarkable resemblance to that which had obtained during the Geoffroy-Cuvier debate. In the scientific arena, the similarities reflect in part the continuing influence on French scientists of Georges Cuvier, dead since 1832. The full length and breadth of the shadow cast by Cuvier has yet to be determined, but it is already clear that French biologists long displayed a nearly slavish obeisance to his principles and precepts, including his cautious attitude toward theory. Although by 1860 belief in universal providential catastrophes had been replaced by a naturalistic concept of localized mountain erogeny, French geologists remained convinced that little or no continuity could be established between organisms in different geological strata.[9] Unable to explain the sudden appearance of distinct new fossil species, most biologists and geologists ascribed the phenonenon to Divine Will, to an unknown natural cause, or avoided the question entirely. Any suggestion that these fossil species or the earliest ones known could have arisen spontaneously from non-living substances was considered absurd because of their complexity. In the wake of Cuvier's work, the doctrine of the transmutation of species still seemed ridiculous at worst, or an unprovable philosophical speculation at best.

In the political arena, France had again entered a period of conservatism following the republican experiment of the 1830s and 1840s.[10] As Cuvier had launched his campaign against transformism and spontaneous generation during the First Empire, so did Pasteur launch his—more strictly against spontaneous generation—during the Second. Napoleon Bonaparte's nephew, Louis Napoleon, had been elected president of the Republic in 1848, thanks in part to the support of the Catholic Church which effectively controlled the votes of the newly enfranchised French peasants. In 1850 the new president had signed the notorious Falloux Law which allowed religious teaching in the state schools and gave the Church the right to its own secondary schools. In 1852, his power having been fortified by the *coup d'état* of 1851 and by the associated plebiscite, in which he won an overwhelming popular mandate, Louis Napoleon declared himself Emperor, once again with the general support of the Catholic Church. Thus, from the outset of the Second Empire, religious issues were simultaneously political issues. The forces of Church and State united in the face of the common enemy—republicanism and atheism. For opposition to the Church and State came not only from republican or liberal ranks but also from positivists, materialists and atheists, all of whom associated themselves with the scientific movement of the nineteenth century. Indeed, for many the new scientific movement became a sort of religion in its own right and Taine looked "forward to the time when it will reign supreme over the whole of thought and over all man's actions." [11]

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[9] The naturalistic basis of French geology by the mid-19th Century is discussed in M. J. S. Rudwick, *The Meaning of Fossils,* (London: MacDonald, 1972), chapter 3.

[10] Our brief summary of the politico-theological issues during the Second Empire derives chiefly from D. G. Charlton, *Secular Religions in France, 1815-1870* (Oxford· University Press, 1963); A. Dansette, *Religious History of Modern France* (New York: Herder and Herder, 1961); and G Wright, *France in Modern Times* (Chicago: Rand McNally, 1966).

In response to this liberal undercurrent, the Church became increasingly authoritarian and reactionary, culminating in the Papal Encyclical of Pope Pius IX in 1864, which emphasized the dangers of religious tolerance and of accommodation with the forces of liberalism and republicanism. In Guérard's words, "God, the Pope, property, law and order were all attacked by the same enemies; practically all Catholics became reactionaries and all reactionaries . . . stood as defenders of the Pope and the Church." [12] Even such a Protestant as François Guizot, the historian and politician, joined in the defense of the Catholic Church against the materialist attack, which he regarded as an attack upon the whole of Christian religion. In a book of 1861, he insisted that "under the blows which [the materialists] bring against Christian dogma, the entire religious edifice collapses and the entire social edifice shakes; the Empire, the essence of religion itself, vanishes." [13]

This climate was further exacerbated by the appearance of Clémence Royer's translation of Darwin's *Origin of Species* in 1862 and of Ernst Renan's *Vie de Jesus* in 1864. The latter attempted to rewrite the life of Christ on the basis of historical criticism and scientifically verifiable events. The former was even more significant since Royer adhered simultaneously to every doctrine the conservative forces loathed: atheism, materialism and republicanism. Her preface to the *Origin* was an extended diatribe against the Catholic Church, which she described as a "religion spread by an ignorant, domineering and corrupt priesthood" and which she identified as the major cause of all societal ills. It is scarcely surprising, then, that Darwinian evolution was regarded in France as a politico-theological doctrine allied with the forces which threatened Church and State. Nor is it surprising that so many French critics of Darwinian evolution focused on the issue of spontaneous generation. For besides its historical association in France with evolutionary theories, spontaneous generation was perceived as a threat to the belief in a providential Creator.[14]

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[11] Quoted in Dansette (n. 10), p. 311.

[12] Albert L Guérard, *French Prophets of Yesterday A Study of Religious Thought under the Second Empire* (New York: D. Appleton, 1920), p. 31.

[13] François Guizot, *L'Église et la société chrétiennes en 1861* (Paris, 1862) 4th ed., p. 18.

Against this background, the outcome of the Pasteur-Pouchet debate over spontaneous generation carried implications of enormous importance to the political fabric of the Second Empire, as had the Geoffroy-Cuvier debate for the First Empire and the Restoration which succeeded it. The anatomist Richard Owen, who lived through both debates, long ago emphasized their similarity. "The analogy of the discussion between Pasteur and Pouchet, and that between Cuvier and Geoffroy, is curiously close," he wrote in 1868. In part, this analogy rested on the circumstance that "Pasteur, like Cuvier, had the advantage of subserving the prepossessions of the 'party of order' and the needs of theology." Moreover, suggested Owen, Pouchet might soon win for his position on the 'origin of monads' the sort of vindication that Geoffroy had already won for his position on the origin of species—"a suggestive and instructive fact in the philosophy of mind and the history of progress." [15] That Owen misjudged Pouchet's ultimate fate is of little concern to us here. Nor can we pursue further the parallels between the Geoffroy-Cuvier debate and the Pasteur-Pouchet debate. Our central concern is the latter debate itself, especially because of the extent to which experimental evidence is supposed to have settled it.

Félix Pouchet's Views Prior to the Debate

When the debate between them began, Pasteur was 37 years old, while Pouchet was nearly 60.[16] Pasteur had only recently entered the study of biological problems, before which his training, interest and expertise lay in the fields of crystallography and chemistry. Pouchet, on the other hand, entered the debate after a long career in traditional biology, his major interest having been animal generation. At what point precisely he became an advocate of spontaneous generation is less than clear. In his two major scientific publications in the 1840s, *Théorie positive de la fécondation des mammifères* (1842) and *Théorie positive de l'ovulation spontanée et de la fécondation des mammifers et de l'espèce humaine* (1847), he seemed rather to deny the doctrine. In both books he set forth the same "fundamental laws of Physiology," among them that "in all the animal kingdom, generation occurs by means of eggs which preexist at fecundation."[17] He admitted that lower forms did reproduce by means other than eggs, but he mentioned only the traditional processes of budding and fission in his discussion of these other means. Furthermore, Pouchet in 1848 endorsed Christian Ehrenberg's claim that infusoria were "complete organisms," which contained in miniature the complex organ systems of higher organisms, including sexual organs.[18] By aligning himself with Ehrenberg, Pouchet placed himself in opposition to those protozoologists, notably Félix Dujardin, who insisted that infusoria were simple undifferentiated organisms which might arise spontaneously. And even though Pouchet's views on egg formation bear some similarity to Theodor Schwann's notion of exogenous cell formation—which some microscopists in the 1840s used in support of the spontaneous origin of single-celled organisms—Pouchet made no reference to spontaneous generation and seemed more interested in the process of ovule production and ovulation *in vivo*.

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[14] Clemence Royer, *De l'origine des espèces par sélection naturelle* (Paris, 1862). Details of the French Darwinian debate and its association with the issues of spontaneous generation are given in J. Farley, "The initial reactions of French biologists to Darwin's *Origin of Species*," *J. Hist. Biology*, 1974, *7* (in press).

[15] Richard Owen, *On the Anatomy of Vertebrates*, vol. 3 (London, 1868), p. 814.

[16] Félix Pouchet (1800-1872) was the son of a highly respected Rouen manufacturer. After obtaining his M.D. from Paris in 1827, he accepted the directorship of the Natural History Museum of Rouen and also filled the chair of zoology at the local medical preparatory college. In 1843 he received the Legion of Honour.

During the decade which followed the appearance of his works on animal generation, Pouchet published one book, a 650-page *opus* on Albertus Magnus and the history of science in the Middle Ages.[19] Then, in 1859, he brought out his long and immediately notorious *Hétérogenie, ou traité de la génération spontanée,* in which he announced his advocacy of spontaneous generation. In what manner this issue had become the focus of his interest remains obscure, but it is obvious that he had begun the work several years before its actual publication. Perhaps he had been attracted to the question by the controversy over parasitic generation, which reached its peak during the early 1850s. In any case, Pouchet spent a significant portion of his book insisting that his views on spontaneous generation had nothing in common with the atheistic and dangerous versions of the doctrine so familiar from the past. Indeed, *Hétérogenie* opened with a 137-page historical and metaphysical justification for a belief in spontaneous generation, and Pouchet insisted throughout that his version of the doctrine was in complete accord with orthodox biological, geological and religious beliefs.

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[17] F. Pouchet, . . . *de l'ovulation spontanée* . . . Law 2.

[18] F. Pouchet, "Note sur les organes digestifs et circulatoires des animaux infusoires," *Comptes Rendus,* 1848, *28:* 516-518.

[19] F. Pouchet, *Histoire des sciences naturelles au moyen âge, ou Albert le Grand et son époque considérés comme point de départ de l'école expérimentale.* (Paris, 1853).

Heterogenesis, he argued, was not the chance doctrine of the ancient atomists; instead it supposed that "under the influence of forces still inexplicable, and which . . . will remain inexplicable, a plastic manifestation is produced, whether in the animals themselves, or elsewhere, which tends to group molecules and to impose on them a special mode of vitality from which finally a new being results." This "plastic force," which takes part in the organization of animals and plants, "can also manifest itself in plant and animal debris." But, argued Pouchet—and this is the most distinctive feature of his version of spontaneous generation—it is *not* adult organisms which are thereby spontaneously generated, but rather their *eggs:*

> Spontaneous generation does not produce an adult being. It proceeds in the same manner as sexual generation, which, as we will show, is initially a completely spontaneous act by which the plastic force brings together in a special organ [the egg] the primitive elements of the organism.[20]

In the second chapter of the book Pouchet comes to grips with the religious arguments put forth against the doctrine of spontaneous generation. He agreed that the first appearance of life was "a true spontaneous generation operating under divine inspiration," but to deny any further spontaneous generation was an "illegitimate fear, for if the phenomenon exists, it is because God has wished to use it in his design." "Where is the verse in the sacred text," he asked, "which tells us that he imposed on himself never to resume his work? Or where is it said that after this rest, he has broken his molds and annihilated his creative faculty?" He argued that God, having fashioned the germ of things, had also imposed laws of matter and of life which determined when the organizational forces gave rise to new beings. "The laws of heterogenesis," he insisted, "far from weakening the attributes of the Creator, can only augment the Divine Majesty."[21] In keeping with his vitalistic conception of spontaneous generation, Pouchet denied the abiogenic production of life, for only "organic molecules" and not inorganic matter was endowed with a *"force plastique":*

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[20] F. Pouchet, *Hétérogenie,* pp. 7-9.

> The succession of life on the surface of the globe links matter in a narrow circle from which it cannot escape. It is successively attracted and repelled by these incessant phenomena. But the organic particles, sometimes united to form organisms, and sometimes free in space, are no less animated with a latent life, which appears only to wait for their grouping to be visibly manifested. It seems that for organic molecules, there is no death, only a transition to a new life.[22]

By the time *Hétérogenie* appeared, Pouchet had been forewarned of one of the arguments which would be advanced against his conception of spontaneous generation. For in 1858 he had published a brief but widely noticed paper purporting to offer experimental proof of spontaneous generation.[23] For now, we wish only to draw attention to the response of Henri Milne-Edwards to this paper. Against Pouchet's position, Milne-Edwards argued that "brute matter cannot organize itself in such a way as to form an animal or plant," and that the "life force has been passed on successively through an uninterrupted chain of being since creation."[24] Pouchet immediately responded by denying that he had ever suggested that organisms could be produced "by the action of those general forces on which depend the chemical combinations in the organic world,"[25] and by promising to examine whether the geological record did in fact show an uninterrupted succession of beings or whether "each fragment of the globe is not constructed majestically against that."[26] This issue provided the subject matter of chapter 6 of his *Hétérogenie,* entitled "geological proofs."

In this chapter Pouchet sought to demonstrate that his belief in heterogenesis was compatible with current geological thinking. This aspect of his belief was to grow more significant in the 1860s, for he maintained that heterogenesis was compatible with the theory of successive creations but not with evolutionary transformism. These arguments carry a greater cogency than one might expect. He agreed with Pictet that "the theory of successive creations is the only one which agrees with the law that the species are entirely different from one strata to another."[27] As suggested above, the problem of the origin of these successive species was by now considered outside the scope of geology. But Pouchet's theory of heterogenesis provided a mechanism for the successive creation of new species, since the "plastic force" could be retained in organic debris and give rise to the first primordia of new creations. Moreover, Pouchet now had an answer to the objection that such complex and complete new forms could hardly be produced by heterogenesis—for it was the undifferentiated *eggs* of such forms and not the adult organisms themselves which were produced in this way. Then, linking his geological defense of heterogenesis with his earlier religious defense, Pouchet portrayed as irrational the belief that "this great work, so frequently repeated, . . . ought to stop." Instead, he argued that just as the intensity and universality of geological catastrophes had diminished over time—the last universal catastrophe being that which raised the Andes and caused the Mosaic Flood—so the power of generating new beings "no longer attains the same proportions as in ancient times." Just as present "catastrophes" were limited to local minor upheavals, so the plastic force was now limited to the production of the eggs of infinitely small organisms.

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[21] *Ibid.,* pp. 97-98.

[22] *Ibid.,* pp. 127-128.

[23] F. Pouchet, "Note sur des proto-organismes végétaux et animaux, nés spontanément dans l'air artificiel et dans le gaz oxygène," *Comptes Rendus,* 1858, *47:* 979-84.

[24] H. Milne-Edwards, "Remarques sur la valeur des faits qui sont considérées par quelques naturalistes comme étant propres à prouver l'existence de la génération spontanée des animaux," *Comptes Rendus,* 1859, *48.* 24.

[25] F. Pouchet, "Remarques sur les objections relatives aux proto-organismes recontrés dans l'oxygène et l'air artificiel," *Comptes Rendus,* 1859, *48:* 149.

[26] *Ibid.,* 152.

Such, in brief, was the theory of spontaneous generation which Pouchet offered in developed form in his book of 1859. What makes it particularly interesting is its essentially vitalistic and providential character. Although it made partial use of Theodor Schwann's allegedly "mechanistic" views on exogenous cell formation, Pouchet's theory placed far greater emphasis on the role of a *"force plastique"* in the spontaneous genesis of eggs, whether in the ovaries of higher organisms or out of organic debris in the case of primitive forms. In fact, Pouchet emphatically opposed materialistic doctrines and explicitly denied the possibility of abiogenesis. He went to great pains to insist on the compatibility of his views with Christian teaching and with the "laws of successive creations" endorsed by most French geologists and biologists. Against this background, it seems manifestly absurd that Pouchet should have been associated with the forces of materialism, transformism and atheism, but in the political climate of the Second Empire that is exactly what happened. Like Geoffroy before him, Pouchet found his name attached to heresies which he explicitly repudiated.

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[27] F-J. Pictet, *Traité de paléontologic ou histoire naturelle des animaux fossiles*. 2nd ed. (Paris, 1853), p. 93.

Pasteur and Spontaneous Generation before 1859

By the time Pasteur turned his attention to the problems of fermentation and spontaneous generation, his prior work in crystallography had already convinced him that life was intimately associated with molecular asymmetry (observable as optical activity) and could not be produced artificially by ordinary chemical procedures. The precise basis and origin of these beliefs is the subject of some controversy,[28] but Pasteur's commitment to them seems undeniable from 1852, and he may even have held them implicitly from the outset of his scientific career. For even then it was clear (especially from the work of Biot, Pasteur's own mentor) that optical activity was generally present in organic substances and uniformly absent from inorganic compounds.

In any case, Pasteur made his early reputation by establishing a link between *molecular* asymmetry (or optical activity) and *crystalline* asymmetry (or hemihedrism) in the case of tartaric acid and its compounds. Even more impressive was his demonstration in 1848 that racemic or paratartaric acid (an isomer of tartaric acid) was composed of two optically active acids, one identical with ordinary (right-handed) tartaric acid, and the other a hitherto unknown acid identical with tartaric acid except for the left-handed direction of both its optical activity and crystalline hemihedrism. Because it was composed of equal portions of these two acids, whose opposing optical activities cancelled each other out, racemic acid had no observable optical activity.[29] These results led Pasteur to propose his "law of hemihedral correlation," according to which crystalline hemihedrism was a correlate of optical activity, and he soon sought to extend this law to other groups of substances.

Although this effort proved rather more onerous than Pasteur probably expected, and though a series of apparent "exceptions" to his law soon surfaced, he managed to resolve nearly all of these difficulties to his own satisfaction. As late as 1856, he insisted that there existed only one legitimate exception to his law, one that he had himself discovered. This exception was optically active amyl alcohol, which displayed no crystalline asymmetry and which crystallized in such a fashion as to persuade Pasteur that any "hidden" asymmetry it might possess could never be discovered.[30] By his own account,[31] it was his determination to study thoroughly this exception which directed his attention to fermentations, in several of which active amyl alcohol appears as a by-product. From the outset, he doubted the standard view that the optical activity of amyl alcohol could be traced to the sugar (also optically active) which served as the starting material in fermentations. Pasteur believed that the molecular structure of amyl alcohol differed too much from that of sugar for its optical activity to originate there. Given his tendency to associate optical activity with life, it seems natural that Pasteur should then have adopted the position that fermentation (and its optically active products) must depend upon the activity of living microorganisms. In his initial paper of 1857 on the lactic-acid fermentation, he described the appearance of a grey deposit which increased in amount as the fermentation progressed and which consisted, like brewer's yeast, of minute globules. He more nearly assumed than proved that this "lactic yeast" was a living organism.[32]

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[28] See esp. Dorian Huber, *Louis Pasteur and Molecular Dissymmetry: 1844-1857* (unpublished M.A. thesis, Johns Hopkins University, 1969).

[29] Pasteur Vallery-Radot, ed., *Oeuvres de Pasteur* (hereafter cited as *O.P.*), 7 vols. (Paris, 1922-1939), I, pp. 77-80, 83-85, 86-120.

In a series of subsequent papers, culminating in a long memoir of 1860,[33] Pasteur extended this view of fermentation as a result of vital activity to ordinary alcoholic fermentation, which had always been viewed as the archetypical fermentative process and which had been the chief battleground for the debates between chemical and biological theories of fermentation. With these papers Pasteur largely overwhelmed the previously dominant chemical theory and laid the basis for the doctrine of specific ferments by showing that the same medium could give rise to different fermentations depending on the nature of the microorganisms sown into it—thus brewer's yeast induced only alcoholic fermentation, while "lactic yeast" induced only lactic acid fermentation.

Implicit in the notion of the specificity of fermentative microorganisms is a commitment to an ordinary sort of generation for them. Only if they arise by an ordinary sort of reproduction, it would seem, could they retain the specific hereditary properties which must account for the specificity of their actions during fermentation.[34] On this ground alone, Pasteur may have been predisposed from the outset against the spontaneous generation of fermentative microorganisms, but the argument never emerges clearly and explicitly in his works. Apparently of more immediate concern to him was the allegation, stemming from Liebig, that yeast (and presumably other "ferments") was a by-product of fermentation rather than its cause. For those willing to accept yeast as living but unwilling to assign it any causative role in fermentation, this suggested the possibility that yeast arose "heterogenetically" from the organic substances in the fermenting medium. Although at this time Liebig himself denied the vitality of the new yeast which might appear during fermentation, he may have given further inspiration to the heterogenesists by his suggestion that certain "lowly organized animalcules" could vegetate in a medium once fermentation was underway.[35]

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[30] *Ibid.*, I, pp. 284-288.
[31] *Ibid*, II, pp. 3-4.
[32] *Ibid*, II, pp. 3-13.
[33] *Ibid*, II, pp. 51-126.

Writing in 1861, Pasteur traced his interest in spontaneous generation to his work on fermentation, and more particularly to his recognition that the ferments were living organisms:

> Then, I said to myself, one of two things must be true. The true ferments being living organisms, if they are produced by the contact of albuminous materials with oxygen alone, considered merely as oxygen, then they are spontaneously generated. But if these living ferments are not of spontaneous origin, then it is not just the oxygen as such that intervenes in their production—the gas acts as a stimulant to a germ carried with it or already existing in the nitrogenous or fermentable materials. At this point, to which my study of fermentation brought me, I was thus obliged to form an opinion on the question of spontaneous generation. I thought I might find here a powerful support for my ideas on those fermentations which are properly called fermentations [36]

In fact, to preserve and buttress his physiological theory of fermentation, Pasteur needed to address the problem of the origin of the microorganisms he considered responsible for the process. For if these organisms arose heterogenetically in a medium already undergoing fermentation, their causative role in the process could scarcely be maintained. But if they arose only from pre-existing organisms or precursor "germs," their causative role could be more confidently affirmed.

From this point of view, Pasteur's subsequent campaign against the heterogenesis of fermentative microorganisms seems to flow naturally from the internal logic of his research. It seems hardly less natural that he should soon extend this campaign by implication to an indictment of "spontaneous generation" in general. What is remarkable, however, and what does deserve immediate examination, is the fact that Pasteur could so vigorously prosecute the case against Pouchet and heterogenesis (and against spontaneous generation in general) while harboring in silence his own belief in the possibility of abiogenesis and his own earlier attempts to produce life artificially.

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[34] Cf. Duclaux (n. 1), pp. 86-87.

[35] See Bulloch (n. 1), pp. 54-55

[36] *O.P.*, II, p. 223.

Indeed, that Pasteur ever made such attempts may itself seem remarkable in view of his conviction that asymmetry, optical activity and life were intimately associated, and that none of them could be produced artificially by ordinary chemical procedures. From this perspective, Pasteur looks for all the world like an ordinary "vitalist," eager to maintain a barrier between the living and non-living. And yet Pasteur himself tried more than once to leap this barrier. The way out of this apparent paradox is to recognize the crucial import in Pasteur's mind of the phrase "by ordinary chemical procedures." For his own attempts to leap the barrier between the living and non-living bore no resemblance to ordinary chemical procedures. They derived instead from his notion of "asymmetric forces," the intervention of which he considered essential to the production of asymmetric molecules (and hence of life). Like his conviction of the intimate association between asymmetry and life, Pasteur's belief in asymmetric forces has an uncertain origin, and the two notions may well be coeval, both dating from 1852 if not before.[37]

In that year, Pasteur discovered that optically active bases could react with racemic acid in such a way as to favor the crystallization of one only of the left-handed and right-handed forms which together compose the racemic acid.[38] In other words, he discovered the capacity of optically active substances to make a choice, as it were, between two asymmetric forms in a racemic substance. In December of 1857, by which time he was deeply involved with his studies on fermentation, Pasteur reported a discovery which suggested that living microorganisms could possess this same "discriminatory" capacity. He had found that a microorganism responsible for the fermentation of ammonium paratartrate selectively metabolized the right-handed component of the paratartrate while leaving the left-handed component intact. In his initial very brief announcement of this discovery,[39] he referred only to its practical value as a means of separating left-handed from right-handed components in racemic substances. But in March of 1858, he emphasized that this discovery, by its connection with the biological process of fermentation, demonstrated for the first time that molecular asymmetry (represented by the microorganism) could intervene to modify "chemical reactions of a physiological sort." [40]

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[37] Cf. Huber (n. 28).

[38] *O.P.*, I, pp. 203-241.

[39] *Ibid.*, II, p. 21.

In two famous lectures of 1860, "On the Asymmetry of Natural Organic Products," Pasteur elaborated more fully on the significance of these discoveries of 1852 and 1857, this time with explicit reference to the concept of asymmetric forces. The fact that racemic substances could be separated into their left- and right-handed components under asymmetric conditions (whether chemical, as in the case of optically active bases, or physiological, as in the case of living microorganisms) led Pasteur to speculate on "the mysterious cause which presides over the asymmetric arrangement of the atoms in natural organic substances:"

> Why this asymmetry? Why any particular asymmetry rather than its inverse? . . .
>
> Indeed, why right or left [substances]? Why not only non-asymmetric [substances], like those in dead nature.
>
> There are evidently causes of this curious behavior of the molecular forces. To indicate them precisely would certainly be very difficult. But I do not believe I am wrong in saying that we know one of their essential characteristics. Is it not necessary and sufficient to admit that at the moment of the elaboration of the immediate principles in the vegetable organism, an asymmetric force is present? For we have just seen that there is only one case in which right-handed molecules differ from left-handed ones, the case in which they are subjected to actions of an asymmetric order.
>
> Can these asymmetric actions be connected with cosmic influences? Do they reside in light, electricity, magnetism, heat? Could they be related to the movement of the earth, the electrical currents by which physicists explain the terrestrial poles? [41]

In other words, Pasteur boldly compared his "asymmetric forces" to physical forces at work in the universe at large. Moreover, at one point in these lectures, Pasteur even suggested that "it seems logical to me to suppose that [artificial or mineral substances] can be made to present an asymmetric arrangement in their atoms, as natural products do." [42] From these two passages, considered in isolation, it might be supposed that Pasteur was here pressing his belief that asymmetric molecules (and thus life) could be produced artificially under the influence of physical asymmetric forces—that abiogenesis could occur under purely "mechanistic" conditions.

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[40] *Ibid.*, II, pp 27-28.

[41] *Ibid.*, I, p. 341.

[42] *Ibid.*, I, p. 337.

Nonetheless, the dominant thrust of the 1860 lectures was to emphasize the distinction between (asymmetric) "living nature" and (symmetric) "dead nature," and to insist that asymmetric molecules could not be produced from symmetric starting materials by ordinary chemical procedures. Indeed, Pasteur so closely linked asymmetry and "living nature" as to deny that *symmetric natural* substances (including oxalic acid and urea) could be considered "natural" in the same sense as asymmetric substances. For him, symmetric natural substances should be considered "excretions rather than secretions, if I may so express it."[43] Nor in fact did he express much conviction that the barrier between living and non-living would soon (if ever) fall. If it seemed "logical" to suppose that symmetric molecules could become asymmetric, it remained to be discovered how this could be accomplished.[44] If it seemed plausible to ask whether asymmetric forces might be related to physical forces in the universe, it was "not even possible at present to offer the slightest suggestions" as to the answer.[45] If it was "essential" to conclude that "asymmetric forces exist at the moment of the elaboration of natural organic products," it was equally clear that these forces "would be absent or without effect in our laboratory reactions, whether because of the violent action of these phenomena or because of some other unknown circumstance."[46] In the end, the molecular asymmetry of natural organic products remained "perhaps the only well marked line of demarcation that we can at present draw between the chemistry of dead nature and the chemistry of living nature."[47]

Because Pasteur used his concept of asymmetric force in such a context, and because he stated it so tentatively and elusively, others regarded it as little more than another "vitalistic" attempt to erect a barrier between the living (or asymmetric) and the non-living (or symmetric). No doubt his speculations would have attracted vastly more interest had he emphasized that while he believed asymmetric forces to be "absent or without effect in our laboratory reactions," he did not consider them beyond the scope of experimental inquiry. Almost certainly, his lectures would have created a sensation had he then described his own earlier attempts to modify and even to create life.

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[43] *Ibid.*, I, pp. 333-334.

[44] *Ibid.*, I, p. 337.

[45] *Ibid.*, I, p. 341.

[46] *Ibid.*, I, p. 342.

[47] *Ibid.*, I, p. 343.

In fact, however, it was not until 1883 that Pasteur briefly described for the first time in public his attempts to "imitate nature" and to "introduce asymmetry into chemical phenomena."[48] As early as 1852, while still in Strasbourg, he had tried to bring asymmetric influences to bear upon crystallization by means of powerful magnets built according to his own specifications. On the basis of manuscript evidence, it seems clear that these attempts were made at least in part upon *inorganic* substances, more specifically upon the elements sulphur, potassium, copper, hydrogen, oxygen, chlorine and carbon (in the form of diamond).[49] Of less interest here are Pasteur's attempts to reverse the direction of asymmetry in natural plant substances (whether by magnetic influences or by a heliostat), for these sought to modify existing asymmetric molecules rather than to create them from symmetric (inorganic) materials. His friend and patron, Biot, tried early on to dissuade Pasteur from these sorts of experiments,[50] and Pasteur himself admitted to his father late in 1853 that "one has to be a little mad to undertake what I am now trying to do."[51] That he nonetheless gave such work high priority is clear from his wife's remark in a letter, also to his father, "that the [experiments Louis] is undertaking this year will give us, should they succeed, a Newton or a Galileo."[52]

In fact, of course, Pasteur did not succeed in creating asymmetry or life and temporarily abandoned these experiments. But he continued to believe that abiogenesis should be possible under some such experimental conditions. He thus came into the debate over spontaneous generation faced with a curious scientific dilemma. On the one hand, his work on fermentation led him to discount the possibility of heterogenesis, while on the other his theoretical views on asymmetry and life led him not only to believe in the possibility of abiogenesis but actually to attempt such a feat experimentally. If it seems illogical simultaneously to believe that life can be produced artificially from inorganic elements but not from a rich organic soup, it is essential to recall that Pasteur reached this paradoxical position as the result of two quite separate research problems and to emphasize the distinction in his mind between symmetric chemical influences and asymmetric physical forces. Nevertheless, and this is the central point, Pasteur could deny the possibility of spontaneous generation only by suppressing part of his own scientific beliefs.

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[48] *Ibid.*, I, p. 376.

[49] See *Ibid*, VII, p. 23. Although these manuscript notes date from 1870, the experiments they project are so similar in conception to those of 1852, that it seems fair to conclude—in the absence of contradictory evidence—that they are essentially identical. It is particularly noteworthy that Pasteur refers to the Ruhmkorff (magnetic) apparatus in these 1870 notes, as he does when first describing his Strasbourg experiments of 1852. Cf. *ibid.*, I, p. 376.

[50] See Pasteur Vallery-Radot, ed., *Pages illustres de Pasteur* (Paris: Hachette, 1968), pp. 10-11.

[51] See Pasteur, *Correspondance,* 4 vols. (Paris: B. Grasset, 1940-1951), vol 1, p. 326.

[52] *Ibid.*, vol. 1, p. 324.

The Debate

The Pasteur-Pouchet debate began in private and quite politely, in contrast to the stormy reception given Pouchet's initial 1858 memoir on the subject by members of the French *Académie,* especially Milne Edwards. In this memoir Pouchet described the appearance of microorganisms in boiled hay infusions under mercury after exposure to artificially produced air or oxygen.[53] A year later Pouchet's controversial book *Hétérogenie* appeared, which elaborated in great detail a coherent view of nature in which the doctrine of spontaneous generation formed a central theme. These publications appeared just as Pasteur was reaching the conviction that fermentation depended on living organisms which could not arise heterogenetically. In February of 1859, in a note on the lactic acid fermentation, Pasteur asserted that the lactic "yeast" in his experiments always came "uniquely by way of the atmospheric air." On this point, he wrote, "the question of spontaneous generation has made an advance." [54] This note prompted a letter from Pouchet, which has apparently not survived, but Pasteur's reply has: "The experiments I have made on this subject," he began, "are too few and, I am obliged to say, too inconsistent in results . . . for me to have an opinion worth communicating to you." Nevertheless, he repeated the conclusion he had just announced in his published note and advised Pouchet that if he repeated his experiments with the proper precautions, he would see "that in your recent experiments you have unwittingly introduced [contaminated] common air, so that the conclusions to which you have come are not founded on facts of irreproachable exactitude." Thus, wrote Pasteur, "I think . . . you are mistaken—not for believing in spontaneous generation, for it is difficult in such a question not to have a preconceived idea—but rather for affirming its existence." He concluded by apologizing for "taking the liberty of telling you what I think on so delicate a subject which has taken only an incidental and very small part in the direction of my studies." [55]

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[53] See Pouchet (n. 25).

[54] *O.P.*, II, pp. 34-36.

Within a year, however, the question of spontaneous generation had taken a central—indeed dominant—place in Pasteur's research. Beginning in February 1860, Pasteur presented a series of five notes to the *Académie* on the subject, the results of which were brought together in his prize-winning essay, *Mémoire sur les corpuscules organisés qui existent dans l'atmosphère,* published in the *Annales des sciences naturelles* in 1861. Recognizing that the existence of atmospheric germs was as yet undemonstrated, Pasteur set out to show that air did contain living organisms and to deny that "there exists in the air a more or less mysterious principle, gas, fluid, ozone, etc., having the property of arousing life in infusions." [56]

Pasteur showed, by a series of brilliant and well-known experiments whose details need not concern us here,[57] that the microorganisms which appeared in a variety of decomposing media arose not from the presence of air alone but from particles in it, which he supposed to be living germs. To this, the heterogenesists objected that the smallest quantity of air invariably sufficed to induce all of these varied decompositions. So if, as Pasteur claimed, each decomposition resulted from a specific germ carried in the air, then the atmosphere must be so thick with a variety of germs as to appear foggy or even solid. Pasteur answered this objection by showing that flasks of boiled yeast-water exposed briefly to air in different localities and at different altitudes did not always putrefy and indeed at high altitudes very rarely did. Thus of 20 flasks he exposed to air at 2000 meters on a glacier in the French Alps, only one underwent subsequent alteration.[58] In 1863 Pouchet travelled with two collaborators (Joly and Musset) to the Pyrenees to repeat Pasteur's experiments precisely except for the substitution of hay infusions for Pasteur's yeast-water. All eight of their flasks underwent subsequent alteration, as would be expected if the organic infusion required only oxygen to engender life.[59] In the face of

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[55] *Ibid.,* II, pp. 628-630.

[56] *Ibid.,* II, p. 246.

[57] For lucid accounts of these experiments, see Bulloch (n. 1), pp. 96-102; and Duclaux (n..1), pp. 85-104.

[58] *O.P.,* II, pp. 202-205.

[59] F.A. Pouchet, N. Joly, and Ch. Musset, "Expériences dur l'hétérogenie exécutées dans l'intérieur des glaciers de la Maladetta," *Comptes Rendus* 1863, *57:* 558-61.

Pasteur's contemptuous attitude toward these Pyrenees experiments, Pouchet and his collaborators insisted on the accuracy of their results and issued a challenge which ended in the appointment by the *Académie* of a second commission on spontaneous generation, the first commission having completed its work just two years earlier.

The Académie des Sciences and the Pasteur-Pouchet Debate

In the highly centralized structure of post-revolutionary French science, the outcome of any scientific controversy was determined chiefly by the reaction of the Paris-based *Académie des Sciences.* This body typically responded to controversies by appointing commissions to adjudicate between the conflicting parties in order to arrive at a presumably objective decision, which thereby became the quasi-official viewpoint of the French scientific community. To a very large extent, the victory of Pasteur over Pouchet was determined by the response of the two commissions set up in the 1860s to examine the question of spontaneous generation.

The controversy aroused by the appearance of Pouchet's *Hétérogenie* in 1859 no doubt stimulated the *Académie* to propose a prize of 2500 francs to be awarded in 1862, "to him who, by well-conducted experiments, throws new light on the question of so-called spontaneous generation." The commission appointed to award the prize consisted initially of Geoffroy St.-Hilaire, Serres, Milne Edwards, Brogniart and Flourens, but before a judgment could be rendered Geoffroy died and Serres was dropped from the panel. Their places were taken by Claude Bernard and Coste, thereby producing a panel unanimously unsympathetic to spontaneous generation from the outset.[60] Milne Edwards and Bernard had already responded critically to Pouchet's initial experimental paper of 1858; Brogniart and Flourens were disciples of Cuvier; and Coste opposed Pouchet's embryological views on the origin of infusorians in hay infusions. In addition, all of them, with the possible exception of Coste, were Catholics. Nonetheless, according to Georges Pennetier, Pouchet and his collaborators entered the competition, only to withdraw when some members of the commission announced their decision before even examining the entries, thereby leaving Pasteur to receive the prize uncontested, on the strength of his 1861 memoir.[61]

Pouchet's subsequent experiments in the Pyrenees and the resulting challenge aroused indignation in the *Académie,* most of whose members felt that the issue was closed. Flourens expressed these views publicly in 1863, by insisting that "the experiments of M. Pasteur are decisive." [62] Nevertheless, the *Académie* named a new commission in 1864, whose members were again united in their agreement with Pasteur: Milne Edwards, Brogniart, Dumas, Balard and, incredibly enough, Flourens himself. Balard, one of two members of the new panel not on the earlier commission, had been Pasteur's mentor in chemistry and had long taken an active interest in his career. Moreover, according to Duclaux, Balard played an important direct role in Pasteur's work against spontaneous generation by suggesting to him his famous "swan-necked" flask experiments.[63] The second new member of the 1864 commission, Jean-Baptiste Dumas, was perhaps even more likely to be predisposed toward Pasteur's position. Like Balard, Dumas had long and actively promoted Pasteur's interests and may even have felt a special additional bond with his protégé since he, like Pasteur, had been subjected to abuse at the hand of Liebig. Dumas, moreover, was an important political figure in the Second Empire, having been appointed minister of agriculture and senator by Louis Napoleon, and later occupying the presidency of the Paris Municipal Council.

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[60] See Georges Pennetier, *Un debat scientifique, Pouchet et Pasteur (1858-68)* (Paris, 1907), p. 10.

[61] *Ibid.*

In the face of this patently biased commission, Pouchet and his collaborators displayed a rather precipitous loss of nerve and engaged the commission in a long and complicated dispute over the timing and nature of the experiments they would be allowed to present before it. In general, Pouchet and his collaborators sought to expand the scope of the inquiry and of the experimental program, while Pasteur and the commission insisted that the issue be very strictly confined to the question, does the least quantity of air invariably suffice to induce decomposition in fermentable media.[64] In the end, Pouchet and his collaborators once again withdrew in the belief that they would be denied a fair hearing.[65]

But the biased composition of these commissions and the uncritical acclaim which they accorded Pasteur's experiments formed only one aspect of the "official" position of the French scientific community on spontaneous generation. Concurrently the French scientific elite devoted considerable effort to a refutation of Darwinism precisely on the basis of Pasteur's overthrow of spontaneous generation. In fact, Flourens—who had succeeded Cuvier as Perpetual Secretary of the *Académie* at the latter's own request—published his *Examen du livre de M. Darwin sur l'origine des espèces* in the very year the second commission was constituted. The major tenet of the book was that Darwinism depended on the occurrence of spontaneous generation and could no longer be maintained since "spontaneous generation is no more. M. Pasteur has not only illuminated the question, he has resolved it." [66] Other leading scientists rallied to the cause and did so in terms which left no doubt as to the political and religious danger of evolutionary ideas.[67] In this way the issue of spontaneous generation became intimately linked with political and theological issues in the public mind. In 1860 Ernst Faivre remarked with passion that the problem of spontaneous generation "excites at the moment the best minds, for it touches science, philosophy and religious beliefs." The destruction of spontaneous generation, he concluded, "is capable of lifting us from the consideration of physical laws to that of general truths, which enlighten our reason and confirm our religious beliefs." [68]

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[62] M.J.P. Flourens. *Comptes Rendus,* 1863, *57:* 845

[63] Duclaux (n 1), p. 107.

[64] On this episode, see Bulloch (n. 1), pp. 103-105; Duclaux (n. 1), pp. 104-109; Pennetier (n. 62), pp 10-12; and *O.P*, II, 321-327, 637-647.

[65] Pennetier (n. 62), p. 12 In 1877, H. Charlton Bastian also complained about the biased composition of the *Académie* Commission appointed in that year to adjudicate between him and Pasteur on spontaneous generation. See H.C. Bastian, "The Commission of the French Academy and the Pasteur-Bastian Experiments," *Nature,* 1877, *16:* 277-279.

Remarks such as this suggest that members of the French scientific community may have chosen Pasteur over Pouchet at least in part for socio-political reasons, especially since many who joined the two-pronged attack against Darwinism and spontaneous generation were dubiously qualified to do so. Their complete disregard for Pouchet's insistence on the orthodoxy of his version of spontaneous generation leads one to wonder if they had even seriously considered his work before denouncing it. Most importantly, the powerful *Académie des Sciences* reacted to the issue of spontaneous generation in ways that seem at least partly political in character—not only by appointing obviously biased commissions to consider the issue, but also by failing to perceive the oddly superficial way in which these commissions carried out their charge (see p. 192 below). In the end, one suspects that the *Académie* and the French scientific community allowed "external" factors to shape their judgment at least as much as the internal scientific issues themselves.

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[66] M.J.P. Flourens, *Examen du livre de M. Darwin sur l'origine des espèces* (Paris, 1864), 170.

[67] See Farley (n. 14).

[68] E. Faivre, "La question des générations spontanées," *Mémoires de l'Académie des Sciences, Belles Lettres et Arts, Lyon,* 1860, *10:* 172.

The Influence of External Factors on Pouchet

But did such factors influence the actual scientific research of the contending parties themselves? Did either Pouchet or Pasteur permit external factors to shape their own scientific judgment? Insofar as Pouchet is concerned, the question in one sense reduces to whether his insistence on the orthodoxy of his version of spontaneous generation was sincere, or merely a façade behind which he hid unorthodox views on evolution, materialism, etc. In opposition to the latter view stands his background as a member of a well-known Protestant family and his detailed familiarity with scripture. His treatise on Albertus Magnus hardly seems the work of a covert atheist. But above all, his views on spontaneous generation, incorporated into a vitalistic, catastrophic view of nature are far too coherent and sustained to be dismissed as mere façade.

Throughout the 1860s, Pouchet repeatedly emphasized that his views were not those of an evolutionary atheist. In 1862, he wrote the geologist Desnoyers of his beliefs that "geologists are necessarily heterogenesists," and that "to prove that a given century is not and will not be able to produce new organisms, is tacitly to advance that in the past all is derived from a single and unique creation." [69] This letter merely restated in forceful fashion the views in his book of 1859, although his geological arguments perhaps gained in relative significance as his experimental clash with Pasteur proceeded. He now claimed, for example, that the solution to the problem of spontaneous generation could come only from an examination of creation in its entirety and not from chemists examining the contents of their phials. In 1864, following the appearance of the French edition of the *Origin of Species,* he affirmed that "the fixity of species, as put forward by Flourens, is the most important and best demonstrated fact in natural history, notwithstanding the assertions of Darwin." [70] In 1865, he published his popular *L'univers, les infiniment grands et les infiniment petits,* also based on catastrophic geology and the concept of the fixity of species. In 1868, in a preface to

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[69] F. Pouchet, *Les créations successives et les soulevèments du Globe. Lettres à M. Jules Desnoyers* (Paris, 1862), letter 1, 5 Jan. 1862.

[70] F. Pouchet, *Nouvelles expériences sur la génération spontanée et la résistance vitale* (Paris, 1864), 199.

Pennetier's *L'origine de la vie,* he again insisted that heterogenesis followed logically from the doctrine of successive creations.

Only in 1870, two years before his death, did Pouchet seem to modify his views slightly, as revealed in a letter to Darius Rossi, one of the few French supporters of Darwin. In his book, *Le Darwinism et les générations spontanées,* Rossi sought to refute Flourens' notion that Pasteur had disproved spontaneous generation and thereby dealt a mortal blow to the Darwinian theory. Instead, Rossi argued that the denial of spontaneous generation was the work of scientists "jealous of conserving certain immutable religious traditions that are believed to be endangered by taking the side of science." [71] He concluded that heterogenesists must accept the transformation of species and Lamarck's explanation for the continual occurrence of primitive forms.

In his letter to Rossi (which the latter included in his book), Pouchet now agreed that spontaneous generation could be used in support of Darwinian theory, thus modifying his earlier insistence that heterogenesis was incompatible with transformism. But he did so without at all embracing the increasingly popular Darwinian theory—indeed, he declared his own continued belief in successive creations. At the same time, he reiterated his belief in spontaneous generation. By this point, if not long before, a politically motivated Pouchet almost certainly would either have openly endorsed evolutionism (if a radical) or have abandoned the presumably dangerous doctrine of spontaneous generation (if a conservative).

This is not to insist that Pouchet's version of spontaneous generation was entirely free of socio-political influences—indeed, his very eagerness to emphasize the orthodoxy of his views suggests otherwise. Nonetheless, he resisted to the end the temptation to modify the essential content of his scientific ideas themselves. Ultimately, one is most impressed by the tenacity with which Pouchet adhered to his belief in spontaneous generation in spite of its alleged threat to orthodox religious and political views which he seems fully to have shared. From this perspective, the picture of Pouchet that emerges is one more traditionally associated with Pasteur. T. S. Hall writes of Pasteur that he:

> did not permit his religious convictions to influence his scientific conclusions. His canon, as a scientist, he firmly stated, was to stand aloof from religion, philosophy. atheism, materialism and spiritualism. . . . It was Pasteur's point that the material

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[71] Darius C. Rossi, *Le Darwinisme et les generations spontanees* (Paris, 1870), p. vi.

> conditions of life had momentous implications for religion, but that these implications must not interfere with the objective interpretation of experimental results.[72]

Here we might equally well substitute the name of Pouchet. The question now is whether Pasteur's own response to the issue of spontaneous generation can also be described in these terms.

Pasteur's Religious and Political Beliefs

If Pasteur entered the debate with Pouchet curiously divided in his own private scientific position on the issue of spontaneous generation, he also entered it secure in his political and religious convictions. In the latter areas, Pasteur's views conformed perfectly to the reigning orthodoxies of the Second Empire. From his father, who had served with distinction in the Napoleonic armies, Pasteur learned to hope for a day when France would regain the glory, internal stability and external prestige it had lost since the fall of the First Empire. As a symbol of that hope, Louis Napoleon enjoyed Pasteur's support from the beginning and held it to the end. Immediately after the coup d'état of 2 December 1851, Pasteur declared himself a "partisan" of the new leader. During the 1860s, his support gained new force from personal acquaintance with the emperor, who encouraged his research—especially the more practical aspects of his studies on wine and on silkworm diseases—and who helped him to secure the facilities and financial resources needed to pursue it. In 1865, Pasteur enjoyed a lavish week as a personal guest of the emperor at one of his most elegant residences, the Palais de Compiègne. Three years later the emperor promoted Pasteur to Commander of the Imperial Order of the Legion of Honour—having already named him Chevalier in 1853—and intervened to secure 60,000 francs toward the construction of a large new laboratory for him at the Ecole Normale Supérieure. The emperor's abdication in 1870 profoundly saddened Pasteur and nullified an imperial decree of 27 July 1870, by which he was to have been made a senator and awarded a national pension.[73]

In return for these and other expressions of Imperial favor, Pasteur dedicated his 1866 book on wines to the emperor and his 1870 book on silkworm diseases to Empress Eugénie. In 1867 he denounced and sought to expel from the Ecole Normale two students who had referred ironically and with seeming approval to an assassination attempt against the emperor while leading a student agitation in support of free speech. Although such organized political activity clearly violated the by-laws of the Ecole Normale, Pasteur's rigid, authoritarian handling of this and other disciplinary matters precipitated further student protest, culminating in the closing of the Ecole and in Pasteur's removal from the administrative posts he had held there.[74] As a matter of fact, this episode can serve to illustrate Pasteur's general preference for order and stability over free speech, civil liberty or even democracy, whose potential for anarchy and mediocrity he feared.

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[72] T.S. Hall, *Ideas of Life and Matter* (Chicago: Univ. of Chicago Press, 1969), Vol. 2, p. 294.

[73] See, *inter alia,* Louis Pasteur, *Correspondance* (n. 53), I, pp. 228, 230; II, pp. 216-236, 345-351, 368, 375, 385, 484-485, 489, 567-568.

Despite his public insistence that he was apolitical, Pasteur readily acceded to the wishes of friends from Arbois that he run for the Senate in the election of 1876. He ran as a conservative, presenting himself as the champion of science and patriotism and making his central political pledge "never to enter into any combination whose goal is to upset the established order of things."[75] But lest that be taken as a commitment to the new republican form of government, he reminded the electorate that the Republic was by law a temporary experiment whose continued existence should depend upon its success at restoring domestic order and international prestige. Unable effectively to deny his close ties to the Second Empire and suspicions of his continued Bonapartist inclinations, Pasteur was overwhelmed in the election by two republican candidates.[76] He then withdrew entirely from the political arena, taking refuge in his scientific research so long as his strength for it remained. In the 1880s he described politics as sterile and ephemeral compared to science and at least twice during that decade declined appeals to try again for the Senate.[77] On a return visit to his native Arbois in 1888, Pasteur was jeered and in fact physically assaulted because of his unpopular political conservatism.[78] In short, Pasteur found the political climate of the Third Republic notably less congenial than that of the Second Empire.

Not surprisingly, Pasteur's religious and philosophical positions were of a piece with his political instincts. Although not quite so "devout" a Catholic as is sometimes supposed, he had been reared in the teachings of the Church and died with the benefits of its last rites. He never doubted the existence of God or the immortality of the soul. Insisting on a sharp distinction between matters of knowledge and matters of faith, he described his own "philosophy" as one "entirely of the heart." [79] Openly contemptuous of materialism, atheism and "free thought," he also criticized positivism for its failure to take account of that "most important of positive motions, that of the Infinite," one form of which is the idea of God.[80] In fact, of the traditional philosophical schools, only spiritualism (in the sense of anti-materialism) seems ever to have held any significant appeal for him. In his inaugural discourse of 1882 at the *Académie Française*, he somewhat cryptically suggested that his scientific work had perhaps contributed to the spiritualist cause, "so neglected elsewhere, but certain at least to find a glorious refuge in your ranks." [81] The conformity of Pasteur's political and religious views to those of the Second Empire scarcely requires further elaboration. More importantly, there exists explicit evidence that he consciously perceived spontaneous generation as a threat to those views.

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[74] See *ibid.*, II, pp. 136-142, 332-339; and Pasteur Vallery-Radot, *Pasteur inconnu* (Paris: Flammarion, 1954), pp. 36-58.

[75] Pasteur, *Correspondance* (n. 53), II, p. 612.

[76] On this campaign and election, see *ibid.*, II, pp. 611-630; and Vallery-Radot, *Pasteur inconnu* (n. 76), pp. 203-215.

[77] Pasteur, *Correspondance* (n. 53), II, pp. 424; IV, pp. 34, 268-270.

[78] See E. Ledoux, *Pasteur et la Franche-Comté; Dole, Arbois, Besançon* (Besançon: Chaffanjon, 1941), pp. 55ff.

Pasteur's Sorbonne Lecture of 1864

On the evening of 7 April 1864, in a striking departure from his usual *modus operandi*, Pasteur gave a wide-ranging public lecture at the Sorbonne on spontaneous generation and its religio-philosophical implications.[82] His lecture was strategically timed, coming two months after the second commission on spontaneous generation had been named but two months before it actually met. The large audience consisted of the Parisian social, scientific and political elite, and there can be little doubt what they hoped and expected to hear. Pasteur did not disappoint them. He opened the lecture with a list of the great problems then agitating and dominating all minds: "the unity or multiplicity of human races; the creation of man several thousand years or several thousand centuries ago; the fixity of species or the slow and progressive transformation of one species into another; the reputed eternity of matter . . .; and the notion of a useless God." In addition to these questions—indeed transcending them all, since it impinged directly or indirectly on each of the others and since it alone could be subjected to experimental inquiry—was the question of spontaneous generation: "Can matter organize itself? In other words, can organisms come into the world without parents, without ancestors? There's the question to be resolved."

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[79] Pasteur, *Correspondance* (n. 53), II, pp. 213-214.

[80] See *O.P.*, VII, p. 338.

[81] *Ibid.*, pp. 326-339, quote on p. 326.

[82] For the text of this lecture, which the next several pages analyze in considerable detail, see *ibid.*, II, pp. 328-346.

After a brief historical sketch of the controversy—in which his aim was to show that the doctrine of spontaneous generation "has followed the developmental pattern of all false ideas"—Pasteur struck to the heart of the matter:

> Very animated controversies arose between scientists, then as now—controversies the more lively and passionate because they have their counterpart in public opinion, divided always, as you know, between two great intellectual currents, as old as the world, which in our day are called *materialism* and *spiritualism*. What a triumph, gentlemen, it would be for materialism if it could affirm that it rests on the established fact of matter organizing itself, taking on life of itself; matter which already has in it all known forces! . . . . Ah! If we could add to it this other force which is called life . . . what would be more natural than to deify such matter? Of what good would it then be to have recourse to the idea of a primordial creation, before which mystery it is necessary to bow? To what good then would be the idea of a Creator—God? . . .
>
> Thus, gentlemen, admit the doctrine of spontaneous generation, and the history of creation and the origin of the organic world is no more difficult than this. Take a drop of sea water . . . which contains some nitrogenous material, some sea mucus, some "fertile jelly" as it is called, and in the midst of this inanimate matter, the first beings of creation take birth spontaneously, then little by little are transformed and climb from rank to rank—for example, to insects in 10,000 years and no doubt to monkeys and man at the end of 100,000 years.
>
> Do you now understand the link which exists between the question of spontaneous generations and those great problems I listed at the outset?

But if Pasteur thus explicitly recognized the religio-philosophical implications of the spontaneous generation controversy, he hastened to deny that his scientific work had been motivated or influenced by such concerns:

> But, gentlemen, in such a question, enough of poetry . . ., enough of fantasy and instinctive solutions. It is time that science, the true method, reclaims its rights and exercises them.
>
> Neither religion, nor philosophy, nor atheism, nor materialism, nor spiritualism belongs here. I may even add. as a scientist, it does not much concern me. It is a question of fact. I have approached it without preconceived idea, equally ready to declare—if experiment had imposed the view on me—that spontaneous generations exist as I am now persuaded that those who affirm them have a blindfold over their eyes.

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Pasteur then reviewed what he considered to be "the most important experiments" on both sides of the issue. Crucial among these on the side of the heterogenesists were Pouchet's initial experiments on boiled hay infusions exposed only to artificial air or oxygen under mercury. Pasteur described these experiments as irreproachable in every regard except one. Pouchet had failed to recognize that ordinary laboratory mercury is itself filled with germs, as could be demonstrated by dropping a globule of this mercury into a fermentable medium in an atmosphere of calcined air. Within two days, the medium swarmed with a variety of microorganisms. But if the same experiments were conducted with precalcined mercury, not a single microbe appeared.

Having thus disposed of what he claimed to be the most persuasive evidence in favor of heterogenesis, Pasteur rehearsed his own famous experiments in which yeast-water was prevented from alteration by denying the access of any atmospheric dust. By depriving the medium of germs from the air, wrote Pasteur, "I have removed from it the only thing that it has not been given to man to produce . . . I have removed life, for life is the germ, and the germ is life." Indeed, the simple unqualified conclusion to be drawn from his experiments in the French Alps was that "spontaneous generation does not exist." Thus far Pasteur had merely repeated the substance of his prize-winning memoir of 1861. Toward the end of his lecture, however, he informed his Sorbonne audience of yet another crucial blow he had more recently delivered against the doctrine of spontaneous generation. Until 1863 Pasteur had relied solely upon experiments in which organic infusions had been vigorously heated, leaving him vulnerable to the charge that such high temperatures might modify the media in such a way as to destroy any vital force it contained and thereby render it incapable of generating life.[83] But in March 1863, he had finally succeeded in preserving two highly alterable natural liquids—blood and urine—without heating them at all, but rather merely by collecting them directly and hermetically from the veins or bladder of healthy animals and then exposing them only to germ-free air.[84] Here, then, was another powerful proof that "spontaneous generation is a chimera."

Pasteur and the "Experimental Method"

By this impressive experimental critique of heterogenesis, Pasteur sought to justify his claim that the question was one of fact, which he had approached "without preconceived idea." If so, one can only marvel at some oddly inconsistent aspects of his "scientific" behavior and some surprising lapses in his supposedly pure commitment to the precepts of the "experimental method." At one point in his prize-winning memoir of 1861, Pasteur admitted that his own repeated attempts to prevent the appearance of microbial life in infusions under mercury succeeded only rarely, perhaps less than 10% of the time. But rather than draw the seemingly obvious conclusion that this microbial life had originated spontaneously, Pasteur refused to accept the experimental evidence at face value and pressed relentlessly toward an alternative explanation. "I did not publish these experiments," Pasteur wrote, "for the consequences it was necessary to draw from them were too grave for me not to suspect some hidden cause of error in spite of the care I had taken to make them irreproachable."[85] Although Pasteur failed to specify what "grave consequences" he feared, it seems obvious that the very existence of spontaneous generation must have been chief among them. As a matter of fact, throughout the spontaneous generation controversy, Pasteur virtually *defined* as "unsuccessful" any experiments—including his own—in which life mysteriously appeared and as "successful" any experiments which gave an opposite result. Happily for him, he managed to indict contaminated mercury as the source of microbial life which appeared in the many "unsuccessful" experiments conducted with the mercury trough.

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[83] Cf. Pasteur, *Correspondance* (n. 75), II, p. 134.

[84] *O.P.*, II, pp. 345-346.

If this achievement seems to justify Pasteur's approach—if indeed it might even be used to emphasize his allegiance to the precept that one should suspend judgment until "all the facts are in"—no such interpretation can be applied to other aspects of his scientific behavior. Most remarkable among these is his apparent failure to repeat Pouchet's disputed experiments in the Pyrenees. The most striking feature of these experiments was the *absence* of mercury from Pouchet's flasks of boiled hay infusions. In his 1864 Sorbonne lecture, Pasteur entirely ignored this problem, choosing to discuss only Pouchet's earlier experiments with mercury. Only once did Pasteur attempt directly to challenge Pouchet's experiments in the Pyrenees. In a note of November 1863, he criticized Pouchet and his collaborators for limiting their flasks to so small a number as eight (thereby introducing the possibility that their results were due to mere "chance") and on the really quite desperate ground that they had broken their sealed flasks with a heated file rather than a pair of pincers as Pasteur had.[86] Not even before the commission appointed to adjudicate their dispute did Pasteur repeat or directly refute Pouchet's experiments. Instead, he chose merely to repeat his own secure experiments with *yeast* infusions, in spite of which the commission praised his exactitude in a report which scarcely veiled its contempt for the opposite side.[87]

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[85] *Ibid.*, II, p. 236.

That Pasteur should thus have violated one of the presumably fundamental precepts of the "experimental method"—namely, the duty to "falsify" his opponents' experiments—seems no less remarkable than the failure of any member of the commission to perceive the violation. In the case of spontaneous generation, moreover, this violation was particularly serious since a single uncontroverted experiment in support of the doctrine automatically carried greater weight than any number of experiments against it. Advocates of spontaneous generation did not need to show that they could produce life artificially under a variety of circumstances, nor even that they could do so consistently. They needed only to show that the feat was possible—a situation Pasteur himself later recognized by emphasizing how difficult his task was compared to the heterogenesists and by noting that "in the observational sciences, unlike mathematics, the absolutely rigorous demonstration of a negation [namely, that spontaneous generation does not exist] is impossible."[88]

And in fact, as Duclaux emphasized long ago,[89] the Pasteur-Pouchet debate might have ended quite differently had Pasteur carefully repeated Pouchet's experiments, or had Pouchet and his collaborators maintained their nerve in the face of Pasteur's self-assurance and the commission's contempt. Thanks chiefly to continued experimental work outside of France, where scientists were relatively isolated from the presumed social dangers of spontaneous generation and from judiciary commissions of the Parisian *Académie des Sciences,* it became clear by the early 1870s that microbial life often appeared in boiled hay infusions even in experiments conducted with irreproachable exactitude and Pasteur-perfect technique. In 1876, the German botanist Ferdinand Cohn and the English physicist John Tyndall were able to offer an explanation for such cases of seeming spontaneous generation. They showed that the life cycle of the hay bacillus (Cohn's *Bacillus subtilis*) included a highly resistant endospore phase which could survive boiling and develop into the usual form of the bacillus upon the introduction of oxygen.[90] For this reason, Pouchet's flasks of boiled hay infusions might well have produced life even in Pasteur's hands upon exposure to the atmosphere, and might therefore have lent crucial support to spontaneous generation during the 1860s.

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[86] *Ibid*, II, pp 321-323.

[87] See *ibid.*, II, pp. 637-647.

[88] See *ibid.*, II, p. 459; VI, 25fn. 1, 41, 54, quote on p. 54.

[89] See Duclaux (n. 1), pp. 108-111.

Curiously enough, Pasteur himself had argued as early as 1861 that the heat-resistance of certain microbes increased in alkaline media[91] (hay infusions are alkaline), and in his Sorbonne lecture of 1864 briefly raised the possibility that Pouchet's hay infusions might contain some unknown heat-resistant microorganism.[92] But he mentioned this possibility only in passing and seemed fully satisfied that Pouchet's precautions were sufficient to preclude it. For Pouchet's early experiments, this posed no serious problems for Pasteur, since he was able to indict contaminated mercury as the cause of the supposedly "spontaneous" appearance of microbial life in Pouchet's flasks. But this explanation could not be applied to the mercury-free Pyrenees experiments of Pouchet and his collaborators. Perhaps because he remained satisfied with Pouchet's preliminary precautions, Pasteur did not now even mention the possibility that his opponents' media may have contained some unknown heat-resistant microorganism from the outset. Once again, but now on highly dubious grounds, he preferred instead to accuse them of having contaminated their flasks through sloppy technique.[93] If Pasteur ever did repeat Pouchet's experiments without mercury, he kept the results private.

The Influence of External Factors on Pasteur

In the absence of corroborating evidence, one might be reluctant to draw the conclusion that Pasteur's experimental work was influenced—consciously or unconsciously—by socio-political factors. But another body of evidence reinforces this conclusion. For throughout the 1860s—the politically most sensitive phase of the spontaneous generation debate—Pasteur gave no public indication of the scientific dilemma he faced in his own mind. While privately convinced that asymmetric forces might hold the key to the artificial production of life, he gave no public voice to this conviction, emphasizing instead the experimental errors of all who affirmed spontaneous generation. Never during this period did he refer in public to his own experimental attempts of the 1850s to "imitate nature" by creating asymmetry or life. Perhaps something besides his initial lack of success and Biot's discouraging advice led Pasteur temporarily to abandon such experiments and to focus his energies elsewhere throughout the most sensitive phase of the debates in France over Darwinism and spontaneous generation.

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[90] See Glenn Vandervliet, *Microbiology and the Spontaneous Generation Debate during the 1870's* (Lawrence, Kan., Coronado Press, 1971), pp. 43-54.

[91] *O.P.*, II, pp. 253-259.

[92] See *ibid.*, II, p. 337.

[93] *Ibid.*, II, pp. 321-323.

In any case, Pasteur's public posture underwent a gradual and subtle change after the collapse of the Second Empire. Perhaps in some sense foreshadowing this public shift was his return, in the very year of Louis Napoleon's abdication, to those private experiments on asymmetry and life which he had abandoned nearly twenty years before. In manuscript notes written at Arbois in autumn 1870, while Paris was embroiled in the Commune, Pasteur speculated about the origin of life and projected a new series of experiments designed to create or modify life by means of magnets or other asymmetric influences.[94] If these experiments were ever carried out, their results never became public property. Soon after projecting these new attempts to create life, Pasteur began to modify his public position on spontaneous generation, but this shift took place so gradually and so subtly as to be virtually imperceptible. For the most part, in fact, he continued his uncompromising campaign against heterogenesis, and in 1872 even discovered a new political mode of discrediting his opponents. Now describing spontaneous generation as a "German" theory, he impugned the patriotism of those Frenchmen who dared to defend it in the wake of the recent Franco-Prussian War.[95] By the mid-1870s, however, the tone of Pasteur's opposition seems rather less decisive. To his earlier bald assertions that spontaneous generation does not exist or is a chimera, he began to add such qualifying phrases as "in the current state of science" and now sometimes confessed that it was impossible to disprove spontaneous generation experimentally.[96] In one discussion of February 1875, he even described as a "capital discovery which perhaps contains the solution of spontaneous generation" Albert Bergeron's discovery of microorganisms in human abcesses which had never been exposed to the external air.[97] Even though that remark appears in a context which reveals Pasteur's doubt that Bergeron had in fact observed spontaneous generation, one may reasonably wonder whether he would even have uttered it during the 1860s.

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[94] See *ibid.*, VII, pp. 21-29

[95] *Ibid.*, II, pp. 379, 396-397.

[96] See *ibid.*, II, p. 459; VI, pp. 25fn. 1, 41, 54, 57. But cf. *ibid.*, VI, pp. 59-60.

Rather more positive evidence of Pasteur's public shift on spontaneous generation emerges from his discussions of asymmetry and life in the 1870s and 1880s. For he began at last to develop and articulate his previously tentative notion of asymmetric forces—forces about which he had been utterly silent since introducing them in his famous lecture of February 1860. He now made it clear, as he had not before, that these asymmetric forces should be considered within the bounds of experimental inquiry and as the means by which the barrier between asymmetric (living) and symmetric (dead) nature might actually be crossed. Such an achievement, he wrote in 1874, "would give admittance to a new world of substances and reactions and probably also of organic transformations." "It is there . . .," he continued, "that it is necessary to place the problem not only of the transformation of species but also of the creation of new ones."[98] In a note of July 1875, he restated this position in essentially identical terms.[99]

By 1883, when Pasteur admitted for the first time in public that he had attempted thirty years before to "imitate nature" and to produce "the immediate, essential principles of life," he had apparently begun to look rather more favorably upon the Third Republic. In June 1880, writing to a friend of a governmental increase of 50,000 francs in his annual research budget, Pasteur declared, "Finally, I have dealt with an amiable and confident Republic."[100] In his lecture of 1883, delivered to the Chemical Society of Paris, Pasteur gave a brief description of his early experiments with magnetism and light at Strasbourg and Lille and suggested that to create life, "it is necessary to manufacture some asymmetric forces, to resort to the actions of a solenoid, of magnetism, of the asymmetric movements of light. . . ."

> The line of demarcation of which we speak is not a question of pure chemistry or of the obtaining of such or such products. It is a question of forces. Life is dominated by asymmetric forces which present themselves to us in their enveloping and cosmic existence. I would even urge that all living species are primordially, in their struc-

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[97] *Ibid.*, VI, p. 20.

[98] *Ibid.*, I, p. 362

[99] *Ibid.*, I, pp. 364-365.

[100] See Pasteur, *Correspondance* (n. 75), III, p. 140.

> ture, in their external form, functions of cosmic asymmetry. Life is the germ and the germ life. Now who can say what the *destiny* of germs would be if one could replace the immediate principles of those germs—albumin, cellulose, etc., etc.—by their inverse asymmetric principles? The solution would consist in part in the discovery of spontaneous generation, if such is within our power; on the other hand, in the formation of asymmetric products with the aid of the elements carbon, hydrogen, nitrogen, sulphur, phosphorus, if in their movements these simple bodies may be dominated at the moment of their combination by asymmetric forces. Were I to try some asymmetric combinations from simple bodies, I would make them react under the influence of magnets, solenoids, elliptically polarized light—finally, under the influence of everything which I could imagine to be asymmetric actions.[101]

How different, how almost "materialistic," is Pasteur's tone here compared to his Sorbonne lecture of 1864! In fact, after this account of Pasteur's shifting and inconsistent views on spontaneous generation, does he any longer conform to that standard picture of the man who, in the words of T. S. Hall, "did not permit his religious convictions to influence his scientific conclusions," and who did not allow external factors "to interfere with the objective interpretation of experimental results"? Let it be clear from the outset that the answer to this question is subtle and complex. On the one hand, Pasteur's religious and political views obviously did not prevent him from conceiving of the possibility of spontaneous generation. As a matter of fact, and in spite of its tortured quality, his divided position on spontaneous generation can be seen as having its genesis entirely in his scientific research—for his work in crystallography led him to believe in the possibility of abiogenesis, while his study of fermentation led him on equally scientific grounds to discount the possibility of heterogenesis. On the other hand, a highly persuasive—if not absolutely decisive—case can be made that Pasteur allowed "external" factors to influence the timing and even the content of his public pronouncements on the spontaneous generation issue and, more remarkably, to affect his experimental work itself. It is hard otherwise to explain his total silence about asymmetric forces during the 1860s; it is hard otherwise to understand how during that same period he could have led a frontal assault against a doctrine in which privately he partly believed; it is hard otherwise to explain why he limited his published experimental work to those areas and to those media which he had good reason to believe would discredit that doctrine; and it is hard otherwise to imagine that so justly celebrated an experimentalist would have minimized the significance of Pouchet's experiments in the Pyrenees and failed in his duty to refute them.

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[101] *Ibid.*, I, pp. 377-378

Conclusion

Remarkably enough, we are led to a conclusion precisely the opposite of that usually attached to the Pasteur-Pouchet debate. For we are persuaded that external factors influenced Pasteur's research and scientific judgment more powerfully than they did the defeated Pouchet. Having formulated his version of spontaneous generation prior to the politically significant Darwinian controversy in France, Pouchet maintained his views with striking consistency in spite of their presumed threat to orthodox religious and political beliefs which he fully shared. By contrast, Pasteur's public posture on the issue seems to reveal a quite high degree of sensitivity to reigning socio-political orthodoxies. This is not necessarily to claim that Pasteur behaved this way deliberately or consciously. Nor is it to claim that unfavorable social circumstances robbed Pouchet of a victory that rightly belonged to him. For the fact remains that Pasteur was a more ingenious and more skillful experimentalist as well as a more persuasive advocate for his point of view. In drawing his analogy between the Cuvier-Geoffroy and Pasteur-Pouchet debates, Richard Owen himself admitted that the similarities extended beyond the fact that Cuvier and Pasteur "had the advantage of subserving the prepossessions of the 'party of order' and the needs of theology." For Pasteur, like Cuvier, also had "the superiority in fact and argument," and "the justice of . . . awarding to [Pasteur] the palm of superior care and skill both in devising and performing the experiments, and exposing the inferiority of [Pouchet] in polemical ability and coolness of argumentation, cannot be denied." [102]

Nonetheless, because Pasteur is so generally considered a leading exponent of the true "experimental method," and because microbiology is so widely assumed to have attained maturity under his influence, the impact of socio-political factors on his scientific work deserves special emphasis. Indeed, by focusing so much on Pasteur we hope to have communicated a fairly clear sense of the specific manner in which external factors affected the actual experimental work and theoretical positions of one whose allegiance to "the experimental method" has rarely if ever been challenged.

[102] Owen (n. 15), p. 814.

In writing this paper, we have quite naturally considered the possible influence of external factors on our own interpretation of the spontaneous generation debate, and it seems only proper to examine this issue in closing. Especially because we find distasteful many of Pasteur's religious, political and personal attitudes, our interpretation may well differ from that of historians and scientists of more conservative persuasions. In the end, the judgment one applies to an issue as subtle and complex as this depends on nuances and matters of emphasis in the interpretation of all the available evidence. In such matters there is no *experimentum crucis*. Nonetheless, we have subjected our personal beliefs to as critical a scrutiny as possible and we emerge convinced that the balance of evidence suggests that the denigrated Pouchet was at least as "objective," if not more so, than the triumphant Pasteur. If we have championed Pouchet at the expense of Pasteur, it must be seen chiefly as a response to earlier distortions of the historical record so extreme as to approach caricature.

ORIGINS OF THE RECEPTOR THEORY OF DRUG ACTION

JOHN PARASCANDOLA AND RONALD JASENSKY *

In modern molecular pharmacology, the effects of drugs on biological systems are explained in many cases in terms of the interaction between the drug molecule and specific molecules or complex macromolecular structures in the cell which are termed "receptors." Although it was suggested by several investigators in the nineteenth century that the action of drugs might be due to a reaction between the drug and some constituent of the cell, the concept of drug receptors was first clearly developed at the beginning of the twentieth century by John Newport Langley (1852-1925) in England and by Paul Ehrlich (1854-1915) in Germany. Beginning with different problems, each elaborated a theory which he was able to use to explain certain pharmacological phenomena. The purpose of this paper is to examine the origins of the receptor theory in the work of these two men.

Nineteenth-Century Background

By the second half of the nineteenth century, some investigators were attempting to study the relationship between the pharmacological action of a drug and its chemical structure and properties. Even before the middle of the century, in the 1840s, James Blake demonstrated that elements which were isomorphous generally had very similar pharmacological properties. The studies of Benjamin Ward Richardson and especially of Crum Brown and Fraser in the 1860s were really responsible for opening up the field of structure-activity relationships.[1] In this period of emerging interest in the application of chemistry to pharmacology, it is not surprising that interest in the nature of the reaction between

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[1] For a discussion of these early structure-activity studies, see John Parascandola, "Structure-activity relationships: the early mirage," *Pharmacy in History*, 1971, *13:* 3-10; also William F. Bynum, "Chemical structure and pharmacological action: a chapter in the history of 19th century molecular pharmacology," *Bull. Hist. Med.*, 1970, *44:* 518-538.

the drug and the body was stimulated. To pharmacologists such as Thomas Fraser and T. Lauder Brunton, who were themselves involved in structure-activity studies, it seemed likely that the physiological action of drugs is usually due to a chemical reaction between the drug and some constituent of the cell or tissue. Oscar Loew postulated in 1893 that certain poisons exert their actions by combining with aldehyde or amino groups of protoplasm. At the turn of the twentieth century, Sigmund Fränkel argued that the selective action of drugs can only be understood by assuming that certain groups in the drug molecule enter into a chemical union with the cell substance of a particular tissue. Once fixed in the cell in this manner, the drug can then exert its pharmacological action.[2]

Some pharmacological evidence for the view that certain drugs exert their influence by forming compounds with substances in the cells was provided by the experimental work of John Newport Langley. In 1874, while still a student of physiology at Cambridge University, he began to investigate the action of pilocarpine on the heart at the suggestion of the noted physiologist Michael Foster.[3] He also studied its effects on secretion, especially on the secretion of saliva by the sub-maxillary gland. During the course of his work, he observed that atropine and pilocarpine were mutually antagonistic in their actions on the gland. In a paper published in 1878, he explained this antagonism by assuming that these drugs compete for a specific substance in the body. He commented:

> . . . we may, I think, without much rashness, assume that there is a substance or substances in the nerve endings or gland cells with which both atropin and pilocarpin are

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[2] Fraser felt that "physiological action is often, if not always, the result of a chemical reaction between the foreign body and certain of the constituents of the vital structures whose action is modified by it." Thomas Fraser, "Lectures on the connection between the chemical properties and the physiological action of active substances," *Brit. Med. J.*, 1872, *2:* 403. Brunton suggested that certain chemical compounds, e.g., curare, enter into a reversible combination with the cell, thus altering its physical character and functional properties. T. Lauder Brunton, "Lectures on the experimental investigation of the action of medicines," *Brit. Med. J.*, 1871, *1:* 414. For Loew's views, see Oscar Loew, *Ein natürliches System der Gift-wirkungen* (Munich: E. Wolff and H. Lüneburg, 1893), especially pp. 38-62. The theory mentioned above applied only to what Loew called "substituting poisons" (his elaborate classification scheme for poisons is beyond the scope of this paper) The concept of "substituting poisons" could not explain the selective action of drugs, since presumably the protoplasm of all cells contains aldehyde and amino groups. Sigmund Fränkel, in his *Die Arzneimittel-Synthese auf Grundlage der Beziehungen zwischen chemischem Aufbau und Wirkung* (Berlin: Julius Springer, 1901), especially pp. 13-41, offered perhaps the clearest expression of the view that drugs combine with specific substances in the cell before the elaboration of the receptor theory by Ehrlich and Langley.

[3] W. M. Fletcher, "John Newport Langley. In memoriam," *J. Physiol.*, 1926, *61:* 2-3.

capable of forming compounds. On this assumption then the atropin or pilocarpin compounds are formed according to some law of which their relative mass and chemical affinity for the substance are factors.[4]

This statement contains the germ of the receptor theory, but Langley did not follow it up for another quarter of a century.

The Side Chain Theory of Paul Ehrlich

In the same year that Langley published the above-mentioned paper, 1878, Paul Ehrlich received his medical degree from the University of Leipzig. His thesis dealt with histological staining. This interest in dyes was to influence much of his later work, and it is necessary to note here the views expressed in this dissertation concerning the mechanism of binding of dyes to the tissues. Ehrlich suggested that the process was more than merely physical adhesion, but he saw it as involving a chemical interaction similar to that involved in the formation of so-called "double salts" (i.e., the linking together of two apparently saturated compounds, such as silver chloride and mercurous chloride). The reaction might be non-stoichiometric, but nevertheless it was chemical in nature.[5] He also recognized that the chemical group on the dye molecule which was responsible for its color was generally not the same as the group responsible for the molecule's affinity for the tissues.[6]

In his "Habilitationsschrift" for his appointment as "Privatdocent" in the University of Berlin (1885), Ehrlich investigated the ability of different tissues to reduce certain dyestuffs. This study is relevant to our present concern because it contains the first indication of his famous "side chain" theory.[7] Ehrlich pointed out that recent studies on the connection between chemical constitution and function, e.g., in dyes, showed that particular properties depend upon the presence of specific atom groups. This concept can also be applied to protoplasm. He adopted Eduard Pflüger's view that protoplasm can be envisioned as a giant molecule consisting of a chemical nucleus of special structure which is responsible for the specific functions of a particular cell (e.g., a liver cell or a kidney cell), with attached chemical side chains. These side chains are more involved in the vital processes common to all cells than in the specific functions of a particular type of cell. It was likely, he felt, that the side chains were the site of cellular oxidation. Some side chains have the ability to fix oxygen (or, more accurately, to give up hydrogen to oxygen and thus become oxidizing agents themselves).[8] Other side chains are concerned with nutrition. Ehrlich imagined that there were probably certain sites on the chemical nucleus which had the capacity to attract and fix "combustible molecule-groups," thus building up nutritive side chains. These nutritive chains are consumed during the process of oxidation and then regenerated.

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[4] J. N. Langley, "On the physiology of the salivary secretion Part II. On the mutual antagonism of atropin and pilocarpin, having especial reference to their relations in the sub-maxillary gland of the cat," *J. Physiol.*, 1878, *1:* 367.

[5] Paul Erlich, "Contributions to the theory and practice of histological staining," in *The Collected Papers of Paul Ehrlich,* edited by F Himmelweit (London: Pergamon Press, 1956-1960), vol. 1, p. 68-73 (previously unpublished Inaugural Dissertation, University of Leipzig, 1878) All references to Ehrlich's published works will be cited from the *Collected Papers* (as it will be referred to hereafter) except in the case of works which were not included in this collection. If an English version of the publication appears in the *Collected Papers,* the reference will be to this version. For bibliographical purposes, the place and date of the first published version of all works will be given in parentheses after each citation (e.g., in this note the original version, in this case an unpublished dissertation, of Ehrlich's paper on staining is cited).

[6] *Ibid.*, pp. 73-74.

[7] Paul Ehrlich, "The requirement of the organism for oxygen: an analytical study with the aid of dyes," *Collected Papers,* vol. 1, pp. 435-438 (Berlin: Hirschwald, 1885). For a discussion of the concept of protoplasm as a "giant molecule" ("biogen"), see Thomas

It was this side chain concept which Ehrlich later used to explain the neutralization of bacterial toxins by antibodies. In 1890, he joined the staff of Robert Koch's newly founded Institute for Infectious Diseases, and became actively involved in the problems of immunotherapy. In 1897, he published the first account of his influential side chain theory of immunity.[9] His experiments with toxins such as ricin and diptheria toxin indicated that the interaction between a toxin and its antitoxin is chemical in nature. Perhaps influenced by his earlier work on dyes, he suggested that the neutralization of a toxin by an antitoxin or antibody may involve the formation of a double salt, although the evidence seemed to indicate that the reaction was stoichiometric (a molecule of toxin combined with a definite quantity of antibody in accordance with the proportions of a simple equivalence). He further noted:

> It must be assumed that this ability to combine with antitoxin is attributable to the presence in the toxin complex of a specific group of atoms with a maximum specific

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Hall, *Ideas of Life and Matter* (Chicago: University of Chicago Press, 1969), vol. 2, pp. 351-353

[8] Ehrlich, "Requirement" (n. 7 above), pp. 482-485.

[9] Paul Ehrlich, "The assay of the activity of diphtheria-curative serum and its theoretical basis," *Collected Papers*, vol. 2, pp. 113-115 (*Klin. Jb.*, 1897).

affinity to another group of atoms in the antitoxin complex, the first fitting the second easily, as a key does a lock, to quote Emil Fischer's well-known simile.[10]

Ehrlich was referring, of course, to Fischer's "lock and key theory" of enzyme action, which was first published in 1894.[11] In attempting to explain the structural specificity exhibited by certain enzymes towards their substrates, Fischer utilized the analogy of a lock and key to emphasize the close relationship which must exist between the configuration of the enzyme and the configuration of the substrate.

The organism has the ability to neutralize a multiplicity of different toxins, yet Ehrlich found it hard to believe that the body had the "inventive activity" to create as needed antitoxins with the appropriate chemical structure to combine with specific toxins. This idea probably seemed to him to border on vitalism, and he dismissed it as a return to the concepts current in the days of "Naturphilosophie."[12] It seemed more likely that the enhancement of a normal cell function was involved, and that physiological analogs of the antibodies must exist beforehand in the organism.

To explain the process of immunization, he called upon the side chain theory which he had earlier used to explain cellular oxidation (Fig. I). Assume, he postulated, that one of the side chains possesses an atom group with a specific combining property for a particular toxin, such as tetanus toxin (No. 1). This side chain is normally involved in ordinary physiological processes, and it is merely coincidental that it has the ability to combine with tetanus toxin. Later,[13] he tended to identify this normal physiological function as a nutritive one, the taking up of foodstuffs (which thus become assimilated into protoplasm). He noted that toxins are complex, protein-like substances, hence it would not be surprising if they possessed a combining group corresponding to that of a foodstuff. Combination with the toxin, however, renders the side chain

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[10] *Ibid.*, p. 114.

[11] Emil Fischer, "Einfluss der Configuration auf die Wirkung der Enzyme." *Ber. dtsch. chem. Ges.*, 1894, *27:* 2992.

[12] In the original German version of the paper cited in note 9 above, Ehrlich uses the term "Zeiten der Naturphilosophie." See Paul Ehrlich, "Die Wertbemessung des Diphtherieheilserums und deren theoretische Grundlagen." *Collected Papers,* vol 2, p. 94. This phrase is translated in the English version (n. 9 above, p 114) as the "days of [an obsolete] natural philosophy." It appears to us, however, that Ehrlich is probably referring specifically to the German "Naturphilosophie" of Oken, Goethe and others in the early nineteenth century.

[13] Paul Ehrlich, "On immunity with special reference to cell life." *Collected Papers,* vol 2, pp. 184-185 (*Proc. Roy. Soc.,* 1900).

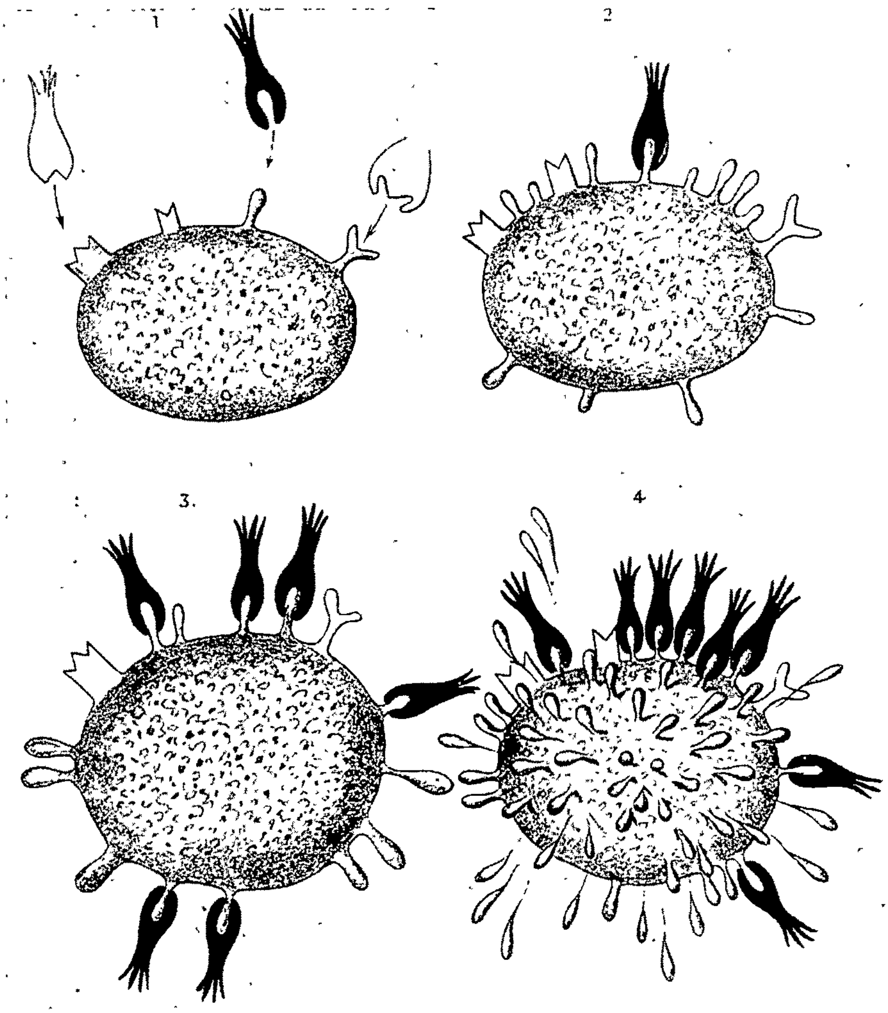

Fig. I

Diagrammatic Representation of Ehrlich's Side Chain Theory
Ehrlich emphasized that these diagrams were merely a pictorial method of presenting his views and were not supposed to represent morphological structures. (*Proc. Roy. Soc.*, 1900. With the permission of the Royal Society.)

incapable of performing its physiological function (No. 2). The cell then produces more of these side chains to make up for the deficiency, but it overcompensates (No. 3). Excess side chains are produced, and these break away from the cell and are released into the bloodstream (No. 4). These excess side chains in the blood are what we call antibodies or antitoxins, and they neutralize the toxin by combining with it. If the toxin cannot be fixed to the cell by combining with a side chain attached to the chemical nucleus, it cannot exert its poisonous effects.[14] Ehrlich later distinguished clearly between the "toxophore" and the "haptophore" groups of the toxin. The haptophore group is the atom group involved in binding the toxin to the side chain. Once the toxin is thus anchored to the cell, the cell comes under the influence of the toxophore group, which is responsible for the poisonous properties of the toxin.[15] This concept may have derived from an analogy with dyes, where the chemical grouping responsible for color is different from that responsible for fixing the dye to the fabric or tissue.

Ehrlich's theory became more elaborate as he struggled to explain various immunological phenomena, but it is not necessary for us to discuss these modifications here.[16] This paper is concerned with his views about the mechanism of action of drugs, and his work on immunity has been considered here, in brief outline, only because of its influence on his thinking about drugs.

Ehrlich's Reluctance to Apply the Side Chain Concept to Drugs

Accounts of Ehrlich's work generally stress the similarity of his view of "chemoreceptors" for drugs to his side chain theory of immunity. The obvious relationship between these two concepts should not mislead us, as it has some authors, into assuming that the former followed directly and immediately from the latter. For when we examine Ehrlich's work, we find that it took him about ten years to apply his side chain theory to the problem of drug action. Indeed, he at first specifically denied that drugs were bound to the cell in a manner similar to the binding of toxins, a fact generally overlooked in the historical literature.[17]

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[14] Ehrlich, "Assay" (n. 9 above), p. 115.

[15] Ehrlich, "Immunity" (n. 13 above), pp. 181-182, 185-186.

[16] For further information on Ehrlich's elaboration of his theory of immunity, see his *Collected Studies in Immunity*, translated by Charles Bolduan (New York: John Wiley, 1906); also Ernest Witebsky, "Ehrlich's side-chain theory in the light of present immunology," *Ann. N.Y. Acad. Sci.*, 1954, *59:* 168-181.

[17] T. S. Work, "The work of Paul Ehrlich and his position in the history of medical research," *Int. Arch. Allergy*, 1954, *5:* 103 claims that Ehrlich's "Habilitationsschrift" (1885) contains the idea that drugs are fixed to receptors because they are chemically similar to the foodstuffs normally fixed by these receptors. Adrien Albert, "The relationship between structure and biological activity: some fundamental aspects," *Ergeb. Physiol.*, 1957, *49:* 426 discusses Ehrlich's theory of immunology and then adds that "Ehrlich soon applied the concept of receptors to explain the action of drugs." We provide evidence in

In an address delivered in 1898, for example, he stated that it is not likely that a firm combination is formed between a drug and the cell, such as in the case of toxins. He pointed out that many drugs can easily be extracted from tissues by solvents, thus they cannot be firmly bound. In addition, the action of many drugs is of a transitory character. Barring a few exceptions, chemically defined drugs and poisons, as opposed to foodstuffs and toxins, are not incorporated into the protoplasmic molecule by a chemical union. They do not possess haptophore groups and are not capable of evoking the production of antibodies.[18]

In his Croonian Lecture of 1900, Ehrlich stated:

> If alkaloids, aromatic amines, antipyretics, or aniline dyes be introduced into the animal body it is a very easy matter, by means of water, alcohol, or acetone, according to the nature of the substance, to remove all these things quickly and easily from the tissues . . . . We are therefore obliged to conclude that none of the foreign bodies just mentioned enter synthetically into the cell complex; but are merely contained in the cells in their free state. The combinations into which they enter with the cells, and notably with the not really living parts of them . . . are very unstable, and usually correspond only to the conditions obtaining in solid solutions, while in other cases only a feeble salt-like formation takes place. . . .
>
> Hence with regard to the pharmacologically active bodies in general, it was not allowable to assume that they possessed definite atom groups, which entered into combination with corresponding groups of the protoplasm.[19]

Yet Ehrlich was well aware of the fact that drugs exhibit a selectivity for certain tissues. In fact, he argued that the distribution of a substance in the organism plays an essential part in determining pharmacological activity. His own experimental work had demonstrated to him the affinities of various dyes for specific tissues, such as nerve tissue or fat tissue, and he was convinced that drugs act in a similar manner.[20] This

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this paper, however, which contradicts these statements and demonstrates that Ehrlich did not apply the receptor concept to drugs until much later. Even when such claims are not specifically made, accounts of Ehrlich's work generally seem to overlook the fact that he deliberately refused to apply the side chain theory to drugs for some ten years. See, for example, Hans Loewe, *Paul Ehrlich: Schöpfer der Chemotherapie* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1950), especially pp. 182-187, and Claude Dolman, "Paul Ehrlich," *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons, 1971), vol. 4, pp. 295-305.

[18] Paul Ehrlich, "The relations existing between chemical constitution, distribution and pharmacological action," *Collected Papers*, vol. 1, pp. 608-611, 613-614 (*v. Leyden Festschrift*, Berlin: Hirschwald, 1902). In this connection, Ehrlich criticized Lowe's theory of the action of "substituting poisons" (*ibid*, pp. 610-611).

[19] Ehrlich, "Immunity" (n. 13 above), p. 184.

[20] See, for example, his "Constitution, distribution and pharmacological action" (n. 18 above), pp. 602-608.

specificity had to be explained, and here his work on dyes once again influenced his thinking.

The fixing of dyes, he noted, is not merely a mechanical or physical process involving surface attraction and adsorption. He discussed two current theories of the mechanism of dyeing or staining, each of which might be correct, he felt, in certain cases. The first of these involved the combination of the dye with a constituent of the fabric to form an insoluble, salt-like compound called a "lake." A similar process might occur, he felt, in the tissues of the organism and might be involved in fixing drugs. Cells which possessed constituents capable of forming a "lake" with a particular drug could thus localize the drug. Although this process involves chemical combination, it is quite different from his side chain or receptor concept. The drug is not thought to combine with the cellular protoplasm (note Ehrlich's reference in the above quotation from his Croonian Lecture to the "not really living parts" of the cell). Instead, it is believed to react with a free substance in the cell, such as an acid, to form an insoluble complex which precipitates out of solution and is thus fixed in the cell.

The second theory of dyeing discussed by Ehrlich involved the formation of solid solutions, where the dye forms a homogeneous mixture with the substance of the fabric. He suggested that certain drugs might be fixed in cells through a similar process, which he associated with the lipid portions of the cell. We must assume, he stated, "that certain fat-like substances of the nervous system, as well as the fat of fat cells, possess a high solvent power, by means of which these soluble substances are anchored or accumulated in the tissue, just as the alkaloids are taken up by the ether in the Stas-Otto procedure." Again, this mechanism (based upon the physicochemical property of solubility) is quite different from the side chain theory, and does not involve chemical combination between an atom group of the drug and an atom group of a receptor. Hence while Ehrlich felt that the interaction between a drug and the cell involved "chemical affinities in the widest meaning of the term" (i.e., the formation of "lakes" and solid solutions), he did not believe that drugs (barring a few exceptions) entered into a "chemical union" with the "protoplasmic molecule." [21]

In the case of drugs, Ehrlich believed, there was no single definite group, no haptophore group, which was responsible for the distribution of the substance in the organism (i.e., responsible for fixing it in specific cells). Instead, the distribution of a drug depended upon "the combined action of the separate components," and hence upon the "entire constitution" of the molecule. On the other hand, he felt that drugs did possess toxophore or "therapeutically active" groups which were responsible for their pharmacological action once they were fixed in the cell (although he did not offer any suggestions as to the mechanism involved).[22]

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[21] *Ibid.*, pp. 608, 614-616. Ehrlich noted that of the hundreds of different compounds which he investigated, he only found convincing evidence, in his view, for the formation of a chemical synthetic union with protoplasm in one case (*ibid.*, p. 611).

In defending his views of the essential difference between the mode of action of toxins and that of drugs, Ehrlich argued in 1903 that whereas the haptophore groups of toxins exhibit a specific combining power towards certain atom groups of protoplasm, the ordinary functional groups of organic chemistry possess affinities for a large number of other groups. For example, aldehyde groups can unite with amino groups, methylene groups. etc. Their combining power is not specifically limited, and hence the lock and key analogy (and the receptor concept) does not apply to drugs.[23]

Ehrlich's doubts about the probability of a "true chemical combination" taking place between the drug and the cell were shared by many of his contemporaries. The fact that so little was known about the nature of chemical bonding must certainly have played an important role in creating such doubts. Chemical combination was generally thought of essentially in terms of covalent bonds, which are not easily broken. Such concepts as hydrogen bonds and Van der Waals bonds had not yet been developed, nor was the electronic nature of bonding recognized. It was thus hard to reconcile phenomena such as the ease with which many drugs could be washed out of tissues by solvents with the contemporary concepts of chemical bonding.

By the end of the nineteenth century, some of the early enthusiasm about studying the relationship between pharmacological activity and chemical structure had begun to wane. The task had proved far more difficult than some investigators had imagined, and no general laws had emerged from such studies.[24] There was also a growing awareness of the importance of such physical properties as solubility in determining the action of drugs, and the advances made in physical chemistry during this period no doubt stimulated interest in such factors. Some pharmacologists preferred to attribute most drug actions to general physicochemical properties of the molecule rather than to the presence of specific structural features which would enable the molecule to combine with the cell. While it was recognized that physical properties depended ultimately on chemical structure, such questions were in the realm of the chemist and the physicist rather than the pharmacologist. As Arthur Cushny noted in 1903, if the pharmacological effects of drugs are due to the formation of chemical compounds in the tissues, then the pharmacologist must be concerned with the molecular structure of drugs. If, on the other hand, the action is due more to the physical properties of the drugs as "uncombined foreign bodies," then structure is of less immediate interest to the pharmacologist.[25] He himself felt that:

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[22] *Ibid.*, pp. 617-618.

[23] Paul Ehrlich, "Toxin and antitoxin: a reply to the latest attack of Gruber," in his *Collected Studies on Immunity* (n. 16 above), pp. 531-533 (this paper was originally published in *Münch. med Wochensch.*, 1903; the German version was reprinted in Ehrlich's *Collected Papers*, vol. 2, pp. 391-394).

[24] See Parascandola, "Structure-activity" (n. 1 above), p. 9.

> . . . many facts . . . can be explained more satisfactorily by assuming that structure is only the ultimate fact, and that the physical factors depending on structure are the immediate determinants of the action in the organism.[26]

By the end of the nineteenth century, it had become obvious that chemicals of widely different structures can sometimes produce the same pharmacological effect. The best known example of this phenomenon involved narcosis, which can be produced by such diverse compounds as ether, chloroform, pentane, etc. (which are generally rather inert chemically). At the turn of the twentieth century, Meyer and Overton independently pointed out that the depressant activities of these narcotic agents was directly proportional to their partition coefficients between a lipid and water, i.e., dependent upon a physical property only indirectly related to structure.[27] The role played by chemical combination in determining pharmacological activity was thus further called into question.

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[25] Arthur Cushny, "The pharmacologic action of drugs. Is it determined by chemical structure or by physical characters?" *J. Amer. Med. Assoc.*, 1903, *41:* 1253.

[26] *Ibid.*, p. 1255. For a discussion of this controversy over the nature of drug action, see John Parascandola, "The controversy over structure-activity relationships in the early 20th century," *Pharmacy in History*, 1974, *16:* 54-63.

[27] Hans Meyer, "Zur Theorie der Alkoholnarkose. Erste Mittheilung. Welche Eigenschaft der Anästhetica bedingt ihre narkotische Wirkung?" *Arch. exp. Path. Pharmakol.*, 1899, *42:* 109-137; E. Overton, *Studien über die Narkose, zugleich ein Beitrag zur allgemeinen Pharmakologie* (Jena: Gustav Fischer, 1901). Adrien Albert has pointed out that the Overton-Meyer rule was very disturbing to those investigating the relationship between

Ehrlich Changes His Mind

As late as September of 1906, in an address delivered at the dedication of the Georg-Speyer-Haus, Ehrlich discussed the question of the affinity of drugs for various tissues or various types of organisms without utilizing the receptor concept. He stressed the fact that drugs must be "fixed" in the cell before they can act, and he suggested that the action of drugs depends upon the presence of two structural features, a selective group which governs distribution and a pharmacophore group which is responsible for pharmacological activity (the same type of dualism which appears in his thinking about dyes and toxins).[28] Ehrlich had thus moved a step closer to accepting the side chain theory for drugs by adopting the view that a specific chemical group (the "selective group"), rather than the entire structure of the molecule, is responsible for its distribution in the body (i.e., for its being fixed in particular cells). There is no mention, however, of a specific chemical combination between the drug and a receptor or side chain in the cell. Yet by June of 1907, when he delivered his Harben Lectures in London, he was able to state:

> I have now formed the opinion that some of the chemically defined substances are attached to the cell by atom groupings that are analogous to toxin-receptors; these atom groupings I will distinguish from the toxin-receptors by the name of "chemoreceptors." [29]

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structure and activity. See his "Relations between molecular structure and biological activity: stages in the evolution of current concepts," *Ann. Rev. Pharmacol*, 1971, *11:* 15-16. Ehrlich, incidentally, felt that he had been the first to point out the pharmacological importance of the distribution of a substance between tissues in his discussion of neurotropic, lipotropic, etc. properties, and to emphasize the importance of lipid solubility. He complained that Meyer had overlooked his work. See Paul Ehrlich, "Address delivered at the dedication of the Georg-Speyer-Haus," *Collected Papers,* vol. 3, pp. 56-57 (previously unpublished).

[28] Ehrlich, "Georg-Speyer-Haus" (n. 27 above), pp. 54-58.

[29] Paul Ehrlich, "Experimental researches on specific therapy: chemotherapeutic studies on trypanosomes. Third Harben Lecture," *Collected Papers,* vol. 3, p. 132 (*The Harben Lectures for 1907 of the Royal Institute of Public Health,* London: Lewis, 1908). Martha Marquardt, Ehrlich's secretary, recounts that when Professor Erich Hoffmann of Bonn came to visit him in the autumn of 1906, Ehrlich mentioned that his experiments had convinced him that chemical substances such as atoxyl are fixed by receptors in the cell. See Martha Marquardt, *Paul Ehrlich* (London: Heinemann, 1949), pp. 151-152. Her recollection of this meeting, written many years later, may not be accurate in every detail, but this story is not incompatible with the views presented in this paper. It would appear that Ehrlich became firmly convinced of the receptor theory of drug action somewhere between September 6, 1906 (when he delivered the Speyer-Haus speech) and early June of 1907 (when he delivered his Harben Lectures).

What had happened to change Ehrlich's mind about the mechanism of action of drugs? Two important factors appear to have played a role in altering Ehrlich's attitude, the work of J. N. Langley and Ehrlich's own studies on drug resistance. In 1913, Ehrlich, looking back at this period, said:

> For many reasons I had hesitated to apply these ideas about receptors to chemical substances in general, and in this connexion it was, in particular, the brilliant investigations by Langley, on the effects of alkaloids, which caused my doubts to disappear and made the existence of chemoreceptors seem probable to me.[30]

We must thus turn back to Langley and consider these investigations to which Ehrlich refers.

Langley and Receptive Substances

Since the late 1880s, Langley had been involved in a detailed study of the autonomic nervous system, the work for which he is best known. During the course of this research, he found that the drug nicotine was a useful tool in the investigation of the structures and functions of the autonomic system. Some of his early experimental work with Dickinson suggested to him that the paralysis of sympathetic nerves caused by nicotine was due to a direct action of the drug on the nerve cells of the superior cervical ganglia, rather than on the peripheral endings of the sympathetic nerves.[31] In 1901, he was able to demonstrate more convincingly that the stimulation of sympathetic ganglia by nicotine, which preceded paralysis of the nerves, was due to a direct action on the nerve cells. Langley severed the pre-ganglionic fibers and allowed their nerve endings to degenerate, yet nicotine still had its usual stimulating effect, thus indicating that it did not cause stimulation by acting on the nerve endings.[32]

In the same year, he reported on a study of the effects of supra-renal extract, which contained adrenalin as its active ingredient.[33] His interest in this substance was probably motivated by the fact that it was known to produce certain effects similar to those resulting from stimulation of the sympathetic nerves, such as contraction of the unstriated muscle of the eye.[34] Supra-renal extract thus seemed like another potential tool in his investigation of the autonomic nerves. Langley was able to demonstrate other effects of the extract, such as the constriction of the blood vessels of the sub-maxillary gland, which were also similar to the effects produced upon stimulation of certain sympathetic nerves. The evidence thus suggested that the extract exerted its action through stimulation of the sympathetic nerve endings which terminated in the various glands or muscles affected. On the other hand, Langley confirmed and extended the earlier observation of Lewandowsky[35] that supra-renal extract continued to exert its effects after degeneration of the post-ganglionic sympathetic nerves had taken place. To Langley, this was strong evidence that the action must be directly on the tissues affected, such as unstriated muscle, and not on the nerve endings.[36]

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[30] Paul Ehrlich, "Chemotherapy," *Collected Papers,* vol. 3, p. 507 (*Proc. 17th Int. Congr. Med.,* 1913, published 1914).

[31] J. N. Langley and W. Lee Dickinson, "On the local paralysis of peripheral ganglia, and on the connexion of different classes of nerve fibres with them," *Proc. Roy. Soc.,* 1889, *46:* 423-431; J. N. Langley and W. Lee Dickinson, "On the progressive paralysis of the different classes of nerve cells in the superior cervical ganglion," *ibid.,* 1890, *47:* 379-390.

[32] J. N. Langley, "On the stimulation and paralysis of nerve-cells and of nerve-endings. Part I," *J. Physiol.,* 1901, *27:* 228-229.

[33] J. N. Langley, "Observations on the physiological action of extracts of the supra-renal bodies," *J. Physiol.,* 1901, *27:* 237-256.

Langley's studies generated a certain amount of controversy, for since Claude Bernard's work on curare there had been a tendency to view the nerve endings as being especially sensitive to the action of various drugs. Brodie and Dixon challenged Langley's conclusions about the action of supra-renal extract (adrenalin), and offered evidence that it acted on the nerve endings or on the "connection link" between nerve and muscle.[37] Elliott elaborated upon this latter suggestion and developed a theory of the action of adrenalin on the "myo-neural junction."[38] But Langley was convinced that there was more or less satisfactory evidence that a number of drugs and poisons act directly on tissues such as muscle and gland, rather than through the nerve endings or through a hypothetical myo-neural junction.[39] It was in an attempt to provide further evidence for this point of view that he was led to develop the receptor theory, which was first described in a paper published in 1905 and in his Croonian Lecture of the following year.

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[34] For example, Langley (*ibid.*, pp. 244-245) called attention to the studies of Lewandowsky on supra-renal extract. See M. Lewandowsky, "Ueber die Wirkung des Nebennierenextractes auf die glatten Muskeln, im Besonderen des Auges," *Arch. Physiol.*, 1899, 360-366.

[35] Lewandowsky, *ibid.*

[36] Langley, "Observations" (n. 33 above), p. 256.

[37] T. G. Brodie and W. E. Dixon, "Contributions to the physiology of the lungs. Part II. On the innervation of the pulmonary blood vessels; and some observations on the action of suprarenal extract," *J. Physiol.*, 1904, *30:* 491-501.

[38] T. R. Elliott, "The action of adrenalin," *J. Physiol.*, 1905, *32:* 427-441

[39] J. N. Langley, "On the reaction of cells and of nerve-endings to certain poisons, chiefly as regards the reaction of striated muscle to nicotine and to curari," *J. Physiol.*, 1905, *33:* 379-380.

Langley decided to test his hypothesis on another tissue besides unstriated muscle or gland tissue, and he chose striated muscle for his experiments.[40] In 1903, in the course of investigating the sympathetic nervous system of birds, he had devoted some attention to the muscular contraction which was known to be produced in fowl by injection of nicotine. At that time, this contraction appeared to him to be largely due to a stimulation of the nerve endings.[41] In 1905, however, he decided to examine this phenomenon in greater detail in connection with the developing controversy about the site of action of drugs such as nicotine and adrenalin. The injection of nicotine into the fowl caused certain muscles to contract, in fact to pass into a state of tonic rigidity. Nicotine, of course, also exerts a paralyzing action, when administered in sufficient quantities, by preventing stimulation of the motor nerve from having any effect upon the muscle. Yet Langley found that even after the nervous-muscular connection had been blocked by relatively large doses of nicotine, further administration of the drug still continued to cause muscular contraction.[42] Could it be that nicotine acts upon two different parts of the neuro-muscular mechanism, paralyzing the nerve endings (thus preventing nervous stimulation of muscle) while at the same time directly stimulating the muscle? Since the paralyzing effect of curare, like that of nicotine, was believed to be due to an action on the nerve endings, Langley reasoned that if the contraction caused by nicotine in fowls was really due to a direct action on the muscle cell it should not be antagonized by curare. He found to the contrary, however, that curare had a marked antagonizing effect; a sufficient dose of curare completely annulled the contraction produced by a small amount of nicotine and diminished that caused by a large amount. Further injection of nicotine once again resulted in contraction.[43]

Langley's next step was to investigate the effects of these drugs when no nerve endings were present, i.e. after cutting the nerves to the muscle and allowing them to degenerate. He found that nicotine still produced contraction after section of the nerves, and, in fact, that curare continued to antagonize this contracting action. He could only conclude that both substances were capable of a direct action on the muscle cell.[44]

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[40] *Ibid.*, p. 380.

[41] J. N. Langley, "On the sympathetic system of birds, and on the muscles which move the feathers," *J. Physiol.*, 1903, *30:* 239.

[42] Langley, "On the reaction" (n. 39 above), pp. 380-385; J. N. Langley, "On nerve endings and on special excitable substances in cells," *Proc. Roy. Soc.*, B, 1906, *78:* 174-176.

[43] Langley "On the reaction" (n. 39 above), pp. 387-393; Langley, "On nerve endings" (n. 42 above), pp. 176-178.

To explain the observed antagonism between nicotine and curare, he returned to the suggestion he had made in 1878 when discussing the antagonism between atropine and pilocarpine. The two drugs must act on the same protoplasmic substance or substances in the muscle, and presumably this involved a combination of the alkaloid with protoplasm. In the presence of both nicotine and curare, which muscle-alkaloid compound is formed depends upon the amount of each alkaloid present and on their relative chemical affinities for the muscle substance.[45]

Since the contracting effect of nicotine and the antagonistic effect of curare were apparently due to an action on the muscle, Langley questioned whether there was any justification for the accepted view that the paralyzing action of these compounds occurred at the nerve endings. He saw no need to postulate two different sites of action. Since the curare decreased the irritability of the muscle to the nicotine stimulus, he felt that it was not unreasonable to assume that it also decreased the irritability of the muscle to nervous stimuli, inducing paralysis in this fashion. Nicotine prevented stimulation of the nerves from causing contraction in the same manner, but it also had the ability of itself stimulating the contraction of muscle (hence explaining its double action of stimulation and paralysis).[46]

After a muscle had been paralyzed by curare or nicotine and it no longer reacted to nervous stimulation, the muscle could still be directly stimulated to contract, e.g., by an electrical current. The muscle substance with which these drugs combine, Langley reasoned, could thus not be the substance which actually contracts. Instead, nicotine and curare must combine with some other constituent of the muscle cell, and he called this unknown constituent "receptive substance." He postulated that the normal function of this receptive substance is to receive the stimulus from the nerve and transmit it to the contractile substance. By combining with the receptive substance, curare and nicotine might diminish its excitability or irritability, hence preventing nervous stimuli from having any effect.[47] In 1906, he suggested alternatively that it was also possible that the transmission of an impulse from nerve to muscle does not take place via an electrical discharge, but involves the secretion of a special chemical substance at the end of the nerve. The receptive substance in the muscle cell combines with the chemical transmitter, forming a compound which stimulates the contractible substance. If curare or nicotine combine with the receptive substance, they prevent it from uniting with the transmitter substance and thus block the passage of nervous impulses.[48] Since the contractile substance itself is not altered by these drugs, however, it can still be directly stimulated. The nicotine-receptive substance compound, although blocking nervous transmission, must have a certain ability to stimulate the contractile substance and cause contraction.

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[44] Langley, "On the reaction" (n. 39 above), pp. 393-397; Langley, "On nerve endings" (n 42 above), pp. 178-180.

[45] Langley, "On the reaction" (n. 39 above), pp. 392-393 (on p. 393, Langley specifically refers to his earlier work on atropine and pilocarpine); Langley, "On nerve endings" (n. 42 above), pp. 181-182.

[46] *Ibid.*

Langley felt that there was evidence to suggest that many other drugs and poisons, such as adrenalin, acted by combining with specific constituents of the cell. He generalized:

> I conclude then that in all cells two constituents at least must be distinguished, (1) substances concerned with carrying out the chief functions of the cells, such as contraction, secretion, the formation of special metabolic products and (2) receptive substances especially liable to change and capable of setting the chief substance in action. Further, that nicotine, curari, atropine, pilocarpine, strychnine, and most other alkaloids, as well as the effective material of internal secretions produce their effects by combining with the receptive substance, and not by action on axon-endings if these are present, nor by a direct action on the chief substance.[49]

The fact that drugs produce different effects in different cells indicates that all cells do not possess the same receptive substance or substances. One would expect, however, that the receptive substances of the cells within a given class would be similar. Consequently, Langley argued, the fact that the effects of adrenalin on different tissues are always like those produced by stimulation of the sympathetic nerves does not mean that it acts by stimulating these nerves. The relationship of adrenalin to the sympathetic system could be explained by assuming that the presence of sympathetic nerves during the development of certain tissues created a certain chemical environment which led to the formation in these tissues of similar receptive substances, substances which had the ability to combine with adrenalin.[50]

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[47] Langley, "On the reaction" (n. 39 above), pp. 393, 399, Langley, "On nerve endings" (n. 42 above), pp. 181-183.

[48] Langley, "On nerve endings" (n. 42 above), p. 183.

[49] Langley, "On the reaction" (n. 39 above), pp. 400-401.

Langley also suggested that the receptive substances and the other substances responsible for carrying out the function of the cell need not actually be separate compounds. It is possible, he commented, that the receptive substance is a side chain on the contractile molecule.[51] In his Croonian Lecture of 1906, he leaned towards the more general idea that all of these different substances (receptive substance, contractile substance, etc.) are radicles or side chains of the protoplasmic molecule.[52] He recognized[53] and specifically mentioned the similarity of such views to Ehrlich's side chain theory of immunity, and we must now return to Ehrlich and his adoption of the concept of chemoreceptors for drugs.

Ehrlich's Receptor Theory of Drug Action

Ehrlich himself has indicated, as previously mentioned, that Langley's work was instrumental in changing his mind about drug receptors. An entry in one of his notebooks indicates that he was aware of Langley's concept of receptive substances by February of 1906, although his failure to mention this theory in his address at the dedication of the Speyer-Haus in September of that year suggests that he was not yet convinced of its validity.[54] It is questionable, however, whether Ehrlich would have been receptive to Langley's theory if the studies being carried out in his own laboratory at about that time on drug resistance had not provided him with another reason for altering his ideas. It was in dealing with the phenomenon of drug resistance that he first applied the receptor concept to drugs.

About 1903, Ehrlich and his coworkers had begun in earnest their work in chemotherapy with a study of the effects of various chemicals in treating infections caused by trypanosomes, the microorganisms responsible for sleeping sickness. Over the next few years, their work and that of other investigators established that there were three different classes of compounds which could attack trypanosomes:

1) arsenical compounds, such as atoxyl;

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[50] *Ibid.*, pp. 402-404.

[51] *Ibid.*, p. 399.

[52] Langley, "On nerve endings" (n. 42 above), p. 194

[53] Langley, "On the reaction" (n. 39 above), p. 399.

[54] Paul Ehrlich's Copybooks, MS, Series IV, No. III, p. 127, February 20, 1906, Wellcome Institute of the History of Medicine Library, London, England.

2) azo dyes, such as trypan red; and
3) basic triphenylmethane dyes, such as fuchsin.[55]

Experiments carried out in Ehrlich's laboratory by Franke, Röhl and Browning in the period 1905-1907 led to the first clear recognition of the phenomenon of drug resistance. The results of these studies were published by Ehrlich and by Browning in 1907.[56]

During the treatment of trypanosome-infected animals by drugs, in some cases the trypanosomes would disappear from the blood for a time and then reappear again at a later date. When the animal was treated again with the same drug, a larger dose was necessary to banish the microorganism from the blood stream for a second time. The trypanosomes might then reappear again, and a still greater quantity of the drug would be required to remove them. Eventually, the drug was virtually ineffective and the animal succumbed to the disease. Was this diminishing effect of the drug due to an alteration of the trypanosome strain or to some change occurring in the host animal? Franke and Röhl were able to demonstrate that in such cases the recurrence was due to the fact that the trypanosome strain had acquired a resistance to the drug. They treated mice infected with the nagana strain of trypanosomes with parafuchsin, and produced a strain in certain animals which no longer responded to treatment by the drug. When the parasites were isolated from these mice and inoculated into a normal animal, the resulting infection did not respond in the least to parafuchsin. Hence the resistance resided in the microorganisms themselves rather than in the original host animal. Once acquired, this resistance persisted from generation to generation as the trypanosome strain was passed through normal untreated animals.

Browning then developed a strain of trypanosomes resistant to atoxyl, as well as strains resistant to trypan red and trypan blue. Resistance was found not to be limited to the compound used to develop it, but to extend also to other compounds within the same chemical class. For example, the atoxyl-resistant strain exhibited a significant degree of resistance towards other arsenical compounds, such as acetyl-atoxyl, and the trypan-red resistant strain exhibited some resistance towards other azo dyes, such as trypan blue and trypan violet. The development of resistance towards one group of compounds (e.g., the arsenicals), however, did not increase resistance towards compounds of the other classes (i.e., azo dyes and basic triphenylmethane dyes). A strain with triple resistance, i.e., resistance against all three of the classes of drugs which attacked trypanosomes, was produced by Browning, but each type of resistance had to be developed separately.

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[55] For a discussion of these studies, see Paul Ehrlich, "Chemotherapeutische Trypanosomen-Studien," *Collected Papers,* vol. 3, pp. 81-93 (*Berlin klin. Wochensch.,* 1907). See also Carl Browning, "Chemotherapy in trypanosome-infections: an experimental study," *J. Path Bact.,* 1908, *12:* 169-176.

[56] Ehrlich, "Trypanosomen-Studien" (n. 55 above), pp. 98-103, Carl Browning, "Experimental chemotherapy in trypanosome infections," *Brit. Med. J.* 1907, *2:* 1405-1409. Our discussion of the drug resistance studies is based upon these two sources.

In his first published report of these results in 1907, Ehrlich offered a brief and somewhat vague explanation for this specificity, but one which would seem to suggest an acceptance of the receptor concept for drugs. He argued that one must imagine that the protoplasm of trypanosomes (and, in general, of all cells) must possess different "places of attack" ("Angriffsstellen") for which specific types of drugs have particular affinities.[57]

Ehrlich's first clear exposition of his concept of "chemoreceptors" was in his previously mentioned Harben Lectures in London in June of 1907. As we have already noted, he pointed out in these lectures that he had come to believe that at least some drugs are bound to the protoplasm by certain atom groupings in a manner somewhat analogous to the binding of toxins. Yet he stressed the fact that there are fundamental differences between the binding of drugs and the binding of toxins. The chemoreceptors for drugs, he felt, must be assumed to possess a simpler structure than toxin receptors, and they do not show the same independence (i.e., they are not separated from the cell and released into the blood as antibodies).[58] In order for an organism to be acted upon by a given poison, it must possess chemoreceptors which combine with the poison. He further explained (using arsenicals as an example):

> If now a mouse infected with trypanosomes is injected with the arsenical medicament, this substance will be distributed between the parasites and the organism of the mouse. If the receptivity of the parasites is the stronger, they will be killed in the organism; in the alternative case, they will not be destroyed. Consequently the curative result obtained when experimenting under normal circumstances represents also the differential between two avidities; i.e., if a mouse infected with the normal strain of trypanosomes is injected with the arsenical preparation, the "trypanotropic" force

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[57] Ehrlich, "Trypanosomen-Studien" (n. 55 above), p. 103.

[58] Ehrlich, "Third Harben Lecture" (n. 29 above), p. 132.

> of the drug is stronger than the "organotropic" force; we therefore obtain a curative effect with doses which are not injurious to the organism of the mouse.[59]

The studies on drug resistance, Ehrlich argued, supported the concept of chemoreceptors. The atoxyl-resistant strain, for example, was resistant to a number of arsenic compounds which otherwise possessed significant differences in their chemical characteristics. The arsenic acid radicle, however, represents a common point of attack in this series. It is bound to the cell by chemoreceptors. The trypanosome cell possesses other chemoreceptors which represent the points of attack for other poisons.[60] In a lecture to the German Chemical Society and in his Nobel Prize Lecture, both delivered in 1908, Ehrlich emphasized further the indirect evidence which the studies on drug resistance provided for the receptor theory. The specificity of resistance made it clear, he felt, that one was dealing with three separate functions, i.e., there are different receptors for each of the three different known classes of trypanocidal drugs.[61]

The development of resistance, he noted in his Harben Lectures, could be readily explained in terms of the receptor concept. The chemoreceptors of resistant trypanosome strains have somehow developed a reduced affinity for the drug, so that the distribution of the drug between the microorganisms and the host animal is shifted in the direction of the latter (the cells of the host animal, of course, also possess chemoreceptors).[62] Or, in other words, the "organotropic" force of the drug becomes greater than its "trypanotropic" force.

Thus Ehrlich had come to accept the receptor theory of drug action. In 1908, at a Congress of the German Dermatological Society, he admitted that he had earlier believed that drugs, as opposed to toxins, were not bound to specific receptors in the cell, but that the study of trypanosomes had caused him to change his mind. He now found himself, he added, in agreement with the views of Langley.[63]

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[59] Paul Ehrlich, "Experimental researches on specific therapy: on athreptic functions. Second Harben Lecture," *Collected Papers,* vol. 3, p. 121 (*The Harben Lectures for 1907 of the Royal Institute of Public Health,* London: Lewis, 1908).

[60] Ehrlich, "Third Harben Lecture" (n. 29 above), p. 132. Browning, in his 1907 paper (n. 56 above), p. 1408, stated that the results of the resistance studies "can only be explained on the basis of Ehrlich's theory, according to which toxic action depends upon a direct chemical relationship between the toxic agent and particular atom groups of the protoplasm, the chemoreceptors." He added that Langley had recently developed a similar concept.

[61] Paul Ehrlich, "Über den jetzigen Stand der Chemotherapie," *Collected Papers,* vol. 3, pp. 159-161 (*Ber. dtsch. chem. Ges.,* 1909); Paul Ehrlich, "On partial functions of the cell," *Collected Papers,* vol. 3, pp. 190-191 (*Les Prix Nobel,* 1909).

[62] Ehrlich, "Second Harben Lecture" (n. 59 above), p. 121.

Conclusion

The concept of drug receptors was first clearly stated by J. N. Langley in 1905 and by Paul Ehrlich in 1907. Langley's concept developed out of his investigations on the actions of nicotine and adrenaline on the body, which were occasioned by his interest in the sympathetic nervous system. Ehrlich's theory developed out of his studies on drug resistance, which were a result of his interest in the chemotherapy of trypanosomes, and ultimately out of his side chain concept of cellular function. While their ideas were developed separately and from different sources, their thoughts did interact. Langley recognized the similarity of his concept of receptive side chains of protoplasm (an idea which he developed further in his later work) to Ehrlich's side chain theory of immunity. Ehrlich admitted to being influenced by Langley's work in his decision to apply the side chain or receptor concept to drugs.

Both Ehrlich and Langley further developed the concept of drug receptors in the period following the origin of the theory. For Langley, the receptor idea played an important part in his understanding of the interaction of nerves and other cells such as muscles. For Ehrlich, the theory formed the theoretical basis for his work in chemotherapy.

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[63] Paul Ehrlich, "Über moderne Chemotherapie," *Collected Papers,* vol. 3, p. 146 (*Verh. 10. Kongr. dtsch. derm. Ges.,* 1908). In a statement written in one of his notebooks in October, 1907, Ehrlich also clearly stated that he had earlier accepted the "lipoid theory," especially in view of the fact that dyestuffs can easily be removed from cells by alcohol. He indicated, however, that on the basis of the work on drug-resistant strains of trypanosomes, he had come to accept the idea of chemoreceptors. See Paul Ehrlich's Copybooks, MS, Series IV, No. IV, p. 132, October 15, 1907, Wellcome Institute of the History of Medicine Library, London, England. By the "lipoid theory" Ehrlich meant the view that substances are fixed in the cell through their solubility in lipids (discussed above).

MEASURING AND PLUGGING: THE MAGICAL CONTAINMENT AND TRANSFER OF DISEASE *

WAYLAND D. HAND

Measuring a patient in various ways as a means of either diagnosing his ailment or ridding him of the disease is a folk medical practice encountered on both sides of the Atlantic. In previous papers on the subject I have discussed measurement by means of string and other kinds of cordage and fibre and also the somewhat rarer method of outlining the ailing part of the body on the trunk of a tree, in the ground, or in the turf.[1] In this ritual of scoring, once the limits of the disease have been demarcated, it can be removed symbolically by hollowing out the contaminated part in the new host, be it wood or earth, and then treating the affected area in various ways. Implicit in this ritual, in addition to the measurement, of course, is the notion of the transference of the disease to the tree or to the earth by the simple process of contagious magic.

In the present paper I treat a kind of measurement that is more common than either of those just mentioned, particularly in the United States and Canada, namely, measuring with a stick or other lineal measuring devices by cutting and notching and then by nailing and plugging in trees and other vertical stationary objects. Common to these forms, as well as to certain other kinds of magical measurement for disease, is the notion that the unit of measure, the length marked, or the area circumscribed somehow prevents the ravage of the disease beyond the confines measured. Perhaps in a more philosophical and symbolic sense, measurement, as Grabner has observed, constitutes a mysterious extension of oneself.[2]

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[1] Wayland D. Hand, "Measuring with string, thread and fibre: a practice in folk medical magic," in Walter Escher, Theo Gantner, and Hans Trümpy, eds., *Festschrift fur Robert Wildhaber zum 70. Geburtstag am 3. August 1972* (Basel. Verlag G. Krebs, 1973), pp. 240-251; Wayland D. Hand, "Magical treatment of disease by outlining the ailing part," *Bull. N.Y. Acad. Med.*, 1972, *48:* 951-954. (The first item is hereinafter cited: Hand, *Measuring*.)

[2] Elfriede Grabner, "Verlorenes Mass und heilkräftiges Messen: Krankheitserforschung und Heilhandlung in der Volksmedizin," in Elfriede Grabner, ed , *Volksmedizin: Probleme*

First to be considered is the cutting of a stick, or occasionally a twig of some sort, to the exact length of the patient. The rationale behind this measurement is the belief that the ailment will be cast off when the child or young person outgrows the mark. Oddly enough, the stick, once measured, is never again put to the sufferer's back to see if the measuring device has really been outgrown. Actually, as things turn out, care is often taken that the stick never be found or that it never even be seen again. These taboos are a part of the ritualistic disposal of objects that have come into contact with the sick person and we shall consider this aspect of measuring later on.

In various parts of the South it was customary, for example, to cut a sourwood stick in a measuring ritual for the cure of asthma and phthisic, which are often regarded by common people of the area as the same ailment.[3] The stick was cut to the exact height of the sufferer. If a child or a young person was involved, he was measured in a standing position so that the stick, if bent at all, could be inclined to the patient's back.[4] In the case of babies, the measurement was undertaken in any way necessary to get the exact length of the infant.[5] In Kentucky when a small child was measured for asthma, it was believed that when he outgrew the sourwood stick the asthma would be gone,[6] but in the same state, and elsewhere, riddance of the disease often depended on what happened to the measuring stick itself.

I am not sure that other writers on measuring as a means of magical riddance have been willing to think of the unit of measure itself as a sort of intermediate agent, or *Zwischenträger,* to which the disease is communicated in the process of measurement. Since customs and rituals connected with measurement do conform in many ways to the patterns of storage, safe-keeping, burial, casting off, floating away, and the like—

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und Forschungsgeschichte (Wege der Forschung, vol. 63, Darmstadt: Wissenschaftliche Buchgemeinschaft, 1967), p. 549.

[3] Wayland D. Hand, ed., *Popular Beliefs and Superstitions from North Carolina* (Durham, N.C.: Duke University Press, 1961-1964, constituting vols. VI-VII of the *Frank C. Brown Collection of North Carolina Folklore,* 7 vols., 1952-1964), VI, 120, no. 829 (hereinafter cited: *Brown Coll.*); Jerry S. Parr, "Folk cures of Middle Tennessee," *Tenn. Folklore Soc. Bull.,* 1962, *28:* 9.

[4] Joseph H. Hall, *Smoky Mountain Folks and Their Lore* (Asheville, N.C.; Cataloochee Press, 1973), p. 50.

[5] Mary E. Miller, "A folklore survey of Dickson County, Tennessee," *Tenn. Folklore Soc. Bull.,* 1958, *24:* 64.

[6] Leonard W. Roberts, "Floyd County folklore," *Kentucky Folklore Record,* 1956, *2:* 54; William E. Barton, *Pine Knot* (New York, 1900), p. 154 (as cited in the *Kentucky Folklore Record,* 1956, *2:* 133).

processes so very common in magical disposal—I feel that we must consider the problem in this broader light. The full range of magical riddance by *Zwischenträger*, of course, cannot be treated in a paper of such restricted scope as this one, but we shall see enough examples to warrant the consideration of the unit of measure as a possible disposal agent of the disease as well as a mere marking device. Perhaps some of the means employed will become more apparent in other parts of this paper, but a few typical kinds of disposal are to be seen in connection with measuring sticks cut to length. In Kentucky, for example, the stick—kinds of wood not mentioned—was placed somewhere out of sight, such as in the loft or attic.[7] In Tennessee, if the sourwood limb were hidden so that no one could ever find it, it was thought that the baby would be cured.[8] Elsewhere in the Southern Mountains, just after the turn of the century, care was taken to hide the stick so that the child would never see it. If he ever did chance to see the stick, it was believed that the charm would be broken.[9] In Tennessee in the late 1930s the sourwood stick was cut to the length of the child and then laid in a dry place. The child then outgrew the phthisic just as it outgrew the stick.[10] Almost twenty-five years later, in the same state, one could still find the prescription about keeping the stick dry, but in this cure for asthma the taboo against seeing the measuring stick apparently no longer existed:

> Asthma. To cure the "tizzey," measure yourself, cut a dry stick of the same length, keep dry and in one corner of the sick person's room.[11]

Such developments over a period of time are, of course, common in folklore, but in an active tradition where enlargement, contraction, and substitution are common, even such reversals as the one we have just seen are not unexpected.

Disposal of the measuring stick by throwing it away is noted for both asthma and "tyzic" in Tennessee and North Carolina.[12] Croup in children was measured in the same way as asthma and phthisic. In the Great

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[7] Gordon Wilson, *Folklore of the Mammoth Cave Region* (Kentucky Folklore Series, no. 4, 1968), p. 61.

[8] Miller, *op. cit.* (n. 5 above), p. 64.

[9] Henry M. Wiltse, "In the field of southern folk-lore," *J. Amer. Folklore*, 1901, *14:* 207. Cf. Joseph D. Clark, "North Carolina popular beliefs and superstitions," *North Carolina Folklore*, 1970, *18:* 7, no. 36.

[10] T. J. Farr, "Tennessee folk beliefs concerning children," *J. Amer. Folklore*, 1939, *52:* 114, no. 49.

[11] Parr, *op. cit.* (n. 3 above), p. 9

[12] Cleo McGlasson, "Superstitions and beliefs of Overton County," *Tenn. Folklore Soc. Bull.*, 1941, *7:* 18, no. 13; *Brown Coll.*, VI, 120, no. 829

Smoky Mountains sourwood was used for this purpose, and the stick was hidden in the woods so that it could never be found.[13] In the Pennsylvania German country the elderberry wand, with which a child suffering from croup was measured, was placed where neither the sun nor moon would shine on it.[14] In North Carolina an ordinary stick was employed, and it was placed in the closet, not necessarily hidden, until the child grew past the mark.[15] Broomsticks, with which children suffering from asthma and phthisic were measured, were carried upstairs where they would never be seen again,[16] but in Illinois the broomstick was buried beneath the front doorstep, where it remained until the child had outgrown the asthma.[17]

The aspect of decay, which is often associated with the riddance of warts, applies to pieces of wood as well as to thread, beanstraw, and various other plant fibres. Usually the wart is circumscribed by these pliable fibres, and the measure is recorded by circumference. Measuring for warts by diameter is exceptional; however, in North Carolina and Kentucky we see a small stick utilized to measure the diameter of the wart.[18] In all of these entries we have the classical pattern of magical disposal. In the North Carolina example the stick is either thrown away or it is buried, and there is the additional prescription of measuring the wart without the sufferer's knowing it. Rotting of the stick in the first Kentucky entry is noted as a condition of the disappearance of the wart. The Fentress item involves burial at a secret place as well as the decaying of the measuring stick.

An unusual cure, one for hay fever, was practiced in parts of Alabama during the 1950s. A willow was cut to one's length, and then it was thrown into the loft of a barn to dry out. When it had dried out, it was believed that the hay fever would be gone.[19] This is strongly reminiscent of cures for rheumatism in which a small potato or a horse chestnut is carried in the pocket of the sufferer who finds relief when the potato or chestnut dries out and withers,[20] absorbing the disease, as it were, in the process.

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[13] Hall, *op. cit.* (n. 4 above), p. 50.

[14] Edwin Miller Fogel, *Beliefs and Superstitions of the Pennsylvania Germans* (Americana Germanica, vol. 18, Philadelphia, 1915), p. 330, no. 1756.

[15] *Brown Coll.*, VI, 51, no. 302.

[16] Daniel Lindsey Thomas and Lucy Blayney Thomas, *Kentucky Superstitions* (Princeton, N.J.· Princeton University Press, 1920), p. 110, no. 1285; *Brown Coll.*, VI, 248, no. 1924; Lawrence S. Thompson, "The broom in the Ohio Valley," *Kentucky Folklore Record*, 1963, *9:* 92.

[17] Harry Middleton Hyatt, *Folk-Lore from Adams County, Illinois*, 2nd ed. (New York: Alma Egan Hyatt Foundation, 1965), p. 253, no. 5642 (hereinafter cited: Hyatt, 2nd ed.).

[18] *Brown Coll.*, VI, 339, no. 2615; Thomas and Thomas, *op. cit.* (n. 16 above), pp. 128-129, no. 1518; Elza E. Fentress, "Superstitions of Grayson County, Kentucky." M.A. thesis, Western State Teachers College, Bowling Green, Kentucky, 1934 (unpub.), p. 64, no. 61.

Two unusual ways to measure a patient for the riddance of a disease are found in Illinois, an area far removed from the main part of the country where the custom of measuring is well known. The first one of these involves measurement in a more transitory way. An asthmatic child is taken to a tree and made to stand under a limb that just touches the top of its head. The limb is then cut off at the trunk and buried. When the child grows above the mark (which is to be seen by the cutting marks where the limb was), he will no longer suffer from asthma.[21] Far more unusual is another entry from Hyatt's standard collection:

> A baby was born with a rupture. Several years later a friend told his father to cut an elm tree down to the height of his son and split the tree down the center, and place the boy over this and say *In the Name of the Father, Son, and Holy Ghost* over him, and he should do this three times. The father did this and the boy was cured of rupture.[22]

Measurement against a tree, whether by a common marking of some sort, or by notching or nailing, almost invariably involved placing the mark at the exact height of the sufferer, or a trifle above, so that outgrowing the ailment could logically follow. Transference of the disease to the tree, if intended, is never mentioned. In any event, translocation is not as clearly indicated as in the case of plugging, where a sort of implantation takes place, occasionally followed by the withering or death of the tree. In cases of simple marking, notching, or nailing, interest centers on the kinds of trees sought for the purpose and on the details of the measuring ritual. In Indiana, for example, it was a Negro custom in Marion County to find a tree struck by lightning and to take the child's measure on it.[23] Sourwood trees were favored in Kentucky and were notched in the usual way, at the exact height of the asthma sufferer.

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[19] Ray B. Browne, *Popular Beliefs and Practices from Alabama* (Folklore Studies, vol. 9, Berkeley and Los Angeles: University of California Press, 1958), p. 70, no. 1148.

[20] *Brown Coll.*, VI, 262-263, nos. 2022-2023; W J. Wintemberg and Katherine H. Wintemberg, "Folk-Lore from Grey County, Ontario," *J. Amer Folklore*, 1918, *31:* 93, no. 150.

[21] Hyatt, 2nd ed., p. 254, no. 5660.

[22] *Ibid.*, pp. 333-334, no. 7184.

[23] W. Edson Richmond and Elva Van Winkle, "Is there a doctor in the house?" *Indiana History Bull.*, 1958, *35:* 122, no. 66.

It was believed that the disease would leave as the child outgrew the mark, or, according to some beliefs, two years afterward.[24] In the Vermillion parish of Louisiana, the French selected a smooth young growing tree and pricked a little hole in it with a quill. This was attended by the usual prognosis concerning the cure.[25] In places as remote from each other as North Carolina, Tennessee, and Texas, no special kinds of trees were mentioned for the affixing of the mark, whether by simple cutting at the proper height, or a more visible kind of notching.[26] Nailing represented a more permanent kind of marking. In Illinois a nail was driven just a bit above the head, so that the croup would leave when the child had grown as high as the nail.[27] The cure of a child suffering from croup, in parts of Texas, involved driving the nail at the exact height of the child, and in some cases on the north side of the tree.[28] As the bark grew over the nail, it was believed that the asthma would disappear. An oak tree was selected for this purpose in Illinois, and three nails were driven into the tree.[29] Croup was treated the same way in this state, except that the disease was not overcome until the child could "chin the nail," which apparently meant that the child would have to grow to a point where the chin reached the nail that had been driven into the tree earlier, level with the top of his head.[30] Placing the nail mark four inches above the head of a child suffering from rheumatism, draping some of the child's hair around the nail and nailing it fast, and then having the child return a year later when it grew up to the mark was a cure employed by an old Indian squaw in New York State in the 1940s. Returning the hair to the girl was supposed to bring about a remission of the disease.[31]

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[24] Wilson, *op. cit.* (n. 7 above), p. 62, no. 5; Jesse Stuart, "The yarb doctor," *Kentucky Folk-Lore and Poetry Mag.*, 1931, *6*, no. 1: 10

[25] Elizabeth Brandon, *Les Moeurs de la Paroisse de Vermillon en Louisiane.* Ph. D. diss., Université Laval, Quebec, 1955, p. 270.

[26] W. Adelbert Redfield, "Superstitions and folk beliefs," *Tenn. Folklore Soc. Bull.*, 1937, *3:* 15, no. 40; Ruth W. O'Dell, "Signs and superstitions," *Tenn. Folklore Soc. Bull.*, 1944, *10*, no. 4: 3; John Q. Anderson, "Magical transference of disease in Texas folk medicine," *Western Folklore,* 1968, *27:* 198; Joseph D Clark, "North Carolina superstitions," *North Carolina Folklore,* 1966, *14:* 8, no. 168.

[27] Harry Middleton Hyatt, *Folk-Lore from Adams County, Illinois,* 1st ed. (New York: Alma Egan Hyatt Foundation, 1935), p. 210, no. 4383 (hereinafter cited: Hyatt, 1st ed.).

[28] Tressa Turner, "The human comedy in folk superstitions," *Pub. Texas Folklore Soc.*, 1937, *13:* 169-170; Anderson, *op. cit.* (n. 26 above), p. 198.

[29] Hyatt, 2nd ed., p. 254, no. 5661.

[30] *Ibid.*, p. 279, no. 6139.

[31] *New York Folklore Q.*, 1955, *11:* 264.

Measuring of children against doors and walls, either by marking, notching, or nailing, is encountered not only in the treatment of the common childhood diseases of asthma, croup, and phthisis, but also to combat convulsions and stuttering. As late as the 1930s in Indiana, a child suffering from phthisis was measured against three inner doors of a house. If he did not return to the house until after he had outgrown the marks, it was believed that the phthisis would be gone.[32] Measuring against the door sill—by which term the door jamb was most likely meant—was practiced in the Blue Ridge Mountain country for the cure of croup, and it was believed that the child would get well when he grew taller than the notch.[33] In Lane County, Oregon, far removed from parts of the country where ritual measuring for the cure of disease was common, people were treated for asthma and for stuttering by measurement against a wall.[34] This was before the turn of the century. Nailing into masonry was also practiced, as for example in Illinois, where a child was made to stand in the chimney corner and then measured for asthma.[35] In cases treated thus far where nailing is involved, I have purposely not discussed the deeper implications of nailing.

"Plugging," a well known folk medical rite for the magical transference of disease, is also pressed into service in measuring rituals. When I mentioned this conjuncture of basic folklore ideas in an article on plugging, nailing, and kindred folk medical customs in 1966,[36] I had not yet made a formal study of measurement in connection with the magical riddance of disease. The present study has not only helped to clear up some of the questions and ambiguities encountered at that time, but has suggested new and fruitful lines of inquiry. In order to get the matter before us in a full folk medical text, it is well to let Brendle and Unger speak of this ritual as it was understood in the Pennsylvania German country, wherein not only measurement is taken up, but also magical arrest of the disease, and transference itself.

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[32] Paul G. Brewster, "Folk cures and preventives from Southern Indiana," *Southern Folklore Q.*, 1939, *3:* 37, no 3.

[33] Roxie Martin, "Old remedies collected in the Blue Ridge Mountains," *J. Amer. Folklore*, 1947, *60:* 185, no. 25.

[34] Donald M. Hines, "Superstitions from Oregon," *Western Folklore,* 1965, *24:* 11, no. 70.

[35] Hyatt, 1st ed., p. 215, no. 4478.

[36] Wayland D Hand, "Plugging, nailing, wedging and kindred folk medical practices," in Bruce Jackson, ed., *Folklore and Society:* Essays in Honor of Benj. A. Botkin (Hatboro, Pa.: Folklore Associates, 1966), p. 66: "From a survey of the literature it is not clear whether measurement and circumscription was the original idea, or whether magical transference through notching, or plugging, constituted the underlying *raison d'être.*"

> Children are supposed to free themselves from chronic afflictions by outgrowing them. To outgrow means to overgrow. A child suffering from convulsions was placed against the inside wall of the house and a nail was driven into the wall at the exact height of the child. When the child grew beyond the mark, it freed itself from the affliction. This, of course, is a sympathetic cure. The "evil thing" was nailed fast in the wall and was drawn away from the child by being prevented from growing with the child.[37]

This ritual, as pregnant as it is concise, nevertheless does not get at the most basic notions of contagious magic, except in an indirect way. In subsequent parts of this paper we shall see numerous examples of the communication of human disease to a tree or to other hosts by means of plugging bits of hair and occasionally nail parings and the like in a hole measured at the sufferer's height and bored into a tree, a doorjamb or other vertical surfaces. Similarly, hair is held fast in the bark of a tree under the head of a nail driven at head level.

Transference of diseases to trees, as to animals, and even to humans, as I have shown in a study on the magical transference of disease,[38] often involves signs of disease and corruption in the new host. Although there are copious references in the literature to the withering and death of trees following the symbolic implantation of disease by means of *Zwischenträger,* these consequences must be considered somewhat exceptional as they relate to plugging as a part of the ritual of measuring for disease. In the Negro tradition of Maryland in 1913, however, one encountered the following cure for chills:

> Ef youse goes to a oak-tree on de sunny-side, en boe a hole in hit toward de north side dez bout to de h'art, en blow yo' bref in hit, en stop hit up tight, den de tree'll die, end yu's won't hab no mo' chills. Dat's sarta—in en sho, dat is, fo' I's dun been dun hit myse'f.[39]

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[37] Thomas R. Brendle and Claude W. Unger, *Folk Medicine of the Pennsylvania Germans. The Non-Occult Cures* (Proceedings of the Pennsylvania German Society, vol. 45, Norristown, Pa., 1935), p. 163.

[38] Wayland D. Hand, "The magical transference of disease," *North Carolina Folklore,* 1965, *13:* 83-109. 1965. In order to demonstrate clearly that an actual transference of disease had taken place, as well as a symbolic one, I chose in that paper only examples that drew attention to the effect of the translocation of disease on the new host: "These symbolic rituals are widely known and easily understood. To claim our attention here the disease must be communicated to second parties or things, and it must be expressly stated, or clearly implied, that the disease is received by the new victim, or a new receiving agent which is itself affected by the transfer. Neither does communication to an intermediate agent, a *Zwischenträger,* suffice for this discussion. Ideally for the argument of this study, it should be shown that when the disease is communicated to other persons, or to animals, it will continue its ravages or induce disease or death in the new victim" (pp. 83-84).

[39] *J. Amer. Folklore,* 1913, *26:* 191.

Harm to the host tree after the implanting of asthma was still encountered in the folk medical traditions of New York state as late as the mid-forties, when a lock of the patient's hair was nailed to a beech tree.[40] According to the account, the patient was cured, but the tree died. Generally, however, the health of the tree is in no way affected. As a matter of fact, it is occasionally stated that the cure does not take effect until the bark has grown over the plugged hole, notably in cures for asthma.[41] Only a thrifty tree, of course, could experience such growth.

The classical forms of "plugging" involve boring a hole into a tree, stuffing the hole with hair, nail parings, and the like, and then plugging up the hole with a peg of some sort. In this treatise we are concerned mainly with the aspect of measuring the height of the sufferer by standing him or her against a tree, and then boring the hole at the level of the head. Asthma is the disease most often involved in these measuring and plugging cures, and the treatment for this malady alone can be traced in an unbroken chain of provinces, states, and regions between Nova Scotia and Missouri, Nebraska, and Texas.[42] Various species of oak trees, for example, are sought in Kentucky, Missouri, and Tennessee.[43] Although measurement is prescribed in the usual way in the last named state, emphasis seems to fall more on the element of plugging, where it is stressed that the hole must be bored to the heart of the tree, and that, after the hair has been placed in it, the hole must be securely plugged so that no air can penetrate the place of safe-keeping. The tree's growing over the plug is also made a condition of the cure. Cherry trees are specified in New York, the honey locust in Illinois, the sweet gum in Texas, and simply a green tree in Indiana.[44] Additional magical elements are seen in the Illinois entry, where the measurement takes place on the west side of the honey locust, and in the Texas ritual where the sufferer is forbidden to look back as he leaves the woods.

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[40] *N.Y. Folklore Q.*, 1945, *1:* 180.

[41] Vance Randolph, *Ozark Superstitions* (New York: Columbia University Press, 1947), pp. 134-135; Grace Partridge Smith, "Folklore from Egypt," *Hoosier Folklore*, 1946, *5:* 69-70, no. 24; Wilson, *op. cit* (n. 7 above), pp. 61-62.

[42] Helen Creighton, *Bluenose Magic: Popular Beliefs and Superstitions in Nova Scotia* (Toronto: The Ryerson Press, 1968), p 194, no. 7; Ruth Ann Musick, "Superstitions," *West Virginia Folklore*, 1964, *14:* 48, no. 5, Richmond and Van Winkle, *op. cit.* (n. 23 above), p. 122, no. 67; Pauline Monette Black, *Nebraska Folk Cures* (University of Nebraska Studies in Language, Literature, and Criticism, no. 15, Lincoln, 1935), p. 34, no. 2.

[43] *Kentucky Folklore Record*, 1960, *6:* 63; Ida Mae McKinney, "Superstitions of the Missouri Ozarks," *Tenn. Folklore Soc. Bull.*, 1952, *18:* 107; *Tenn. Folklore Soc. Bull.*, 1950, *16:* 10.

[44] Janice C. Neal, "Grandad—pioneer medicine man," *N.Y. Folklore Q.*, 1955, *11:* 283; Hyatt, 2nd ed., p. 255, no. 5669; George D. Hendricks, *Mirrors, Mice & Mustaches: A Sampling of Superstitions and Popular Beliefs in Texas* (Austin: Texas Folklore Society, 1966), p. 32; Richmond and Van Winkle, *op. cit.* (n. 23 above), p. 122, no. 68.

Implantation, in addition to measurement, is important in the plugging ritual as practiced in Nova Scotia. Here the fingernails and toenails, as well as the hair, of the sufferer from asthma are cut off before the trip is undertaken into a secluded spot in the woods "where they won't be often going." A tree is found, a hole is bored in it even with the child's head, and the cuttings are placed within. The hole is then stopped up with a pine plug and the time awaited until the child outgrows that point on the tree at which the plug was inserted.[45] In Georgia the asthmatic child coughs into the hole bored into a tree, symbolically leaving the malady deposited there. This part of the ritual outweighs, apparently, the measuring of the child before the hole is drilled.[46]

Measurement by plugging in a tree is ritually undertaken for the cure of such other diseases as croup,[47] phthisis,[48] fits,[49] tetter,[50] and nosebleed.[51] Short growth, so-called, most often measured with string by comparing the span of the outstretched arms with the height from the crown of the head to the soles of the feet,[52] is also occasionally measured by plugging, as entries from Maryland and Indiana attest.[53]

Before concluding the discussion of plugging as it relates to trees, notice should be given of variations on the notion of outgrowing the disease. We have already seen an example in Illinois of a child's outgrowing the mark of a nail driven in a tree by a far greater measure than was ordinarily envisioned. Croup would leave the child, it was claimed, only when he had "chinned the nail," i.e., had grown to a point where the chin reached the nail originally driven into the tree at the child's height.[54] Comparable to this is a curative ritual known in Pennsylvania and Indiana between 1870 and 1920, where a child suffering from "tissic" (asthma) was measured against a tree and a lock of his hair plugged into a hole bored at the height of his head. The delayed cure took effect when the child later grew to a point where his breastbone passed the plug in the tree.[55]

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[45] Arthur Huff Fauset, *Folklore from Nova Scotia* (Mem. Amer. Folklore Soc., vol. 24, New York, 1931), p. 197, no. 336

[46] *Foxfire,* 1968, *2,* no. 1: 10, no. 3

[47] Wiltse, *op. cit.* (n. 9 above), p. 207; Emma Gertrude White, "Folk-medicine among the Pennsylvania Germans," *J. Amer. Folklore,* 1897, *10,* 79.

[48] *Brown Coll.,* VI, 55, no. 328: Brewster, *op. cit.* (n. 32 above), p. 37, no. 2 (notching, and replacing the chip rather than plugging *per se*).

[49] Brewster, *op. cit.,* p. 36, no. 2.

[50] Hyatt, 2nd ed., p. 269, no. 5895.

[51] Black, *op cit.* (n. 42 above), p. 30, no. 20 (mulberry tree); Hyatt, 2nd ed., p. 211, no. 4830.

[52] Hand, *Measuring,* pp. 240-251, esp. pp. 243 ff.

[53] Annie Weston Whitney and Caroline Canfield Bullock, *Folk-Lore from Maryland* (Mem. Amer. Folklore Soc., vol 18, New York, 1925), p. 89, no. 1810.

Nailing a mark on a tree as a means of indicating the height of a sufferer was discussed above. Now we can look at those aspects of nailing which involve plugging as well. In the Pennsylvania German country a child suffering from croup was placed against an oak tree and a nail driven into the tree at the level of the head in such a manner that some of the sufferer's hair was held fast under the head of the nail.[56] Even clearer notions of the close connection between nailing and plugging are seen in examples from Texas and Illinois. Nosebleed, for instance, is stanched in Texas by standing the sufferer up against a mulberry tree, measuring his height, and then boring a hole in the tree. A lock of hair is then cut and nailed fast into the hole with a rusty nail.[57] Even more unique is the ritual cure for asthma in Illinois:

> If a child has asthma—when the moon is first turning to dark, take the child and stand him up against a tree; then take a ten-penny nail and hammer that nail in the tree even with the child's top of the head, then pull that nail out, then cut a lock of the child's hair and put in that hole, then drive the nail back into the hole up to the head. When the child grows above the head on the nail, it will be cured. You must say *In the Name of the Father, Son, and Holy Ghost* when driving the nail in that hole.[58]

Measurement by plugging was also carried out in seasoned wood of various kinds, as well as in trees. Posts were used for this purpose in Kentucky and Iowa, for example, and the plugging of hair into the hole was managed in the usual way.[59] Measurement by plugging into doors and doorjambs was relatively common, only slightly less favored, as a matter of fact, than in trees themselves. This tradition extends from French Canada and Nova Scotia through the Pennsylvania German country, into the Appalachians, into Ohio, and as far west as Illinois.[60] Almost all procedures involve the implanting of hair after the measurement was made, and the kinds of diseases treated range from cough and bronchitis to asthma, phthisic, undergrowth, hernia, and nosebleed. In the Adirondacks an ingenious kind of measurement was employed, whereby the child's hair was wedged into a window frame above the child's head so that he or she would lose the asthma when the mark was outgrown.[61] When measurement was made against walls, it was generally inside the house and in walls of wood and plaster, rather than those of masonry. Records of this practice are found from New York State to Alabama and Louisiana and as far west as Kansas.[62]

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[54] Hyatt, 2nd ed., p. 279, no. 6139.

[55] *Pennsylvania Folklife,* Oct. 1964, *14:* 37.

[56] Fogel, *op. cit.* (n. 14 above), p. 330, no. 1759; Brendle and Unger, *op. cit.* (n. 37 above), p. 136.

[57] Frost Woodhull, *Ranch Remedios: Man Bird, Beast* (Pub. Texas Folklore Soc., vol. 8, Austin, 1930), p. 61.

[58] Hyatt, 2nd ed., pp. 254-255, no. 5667.

[59] Thomas and Thomas, *op. cit.* (n. 16 above), p. 100, No. 1143; Earl J. Stout, *Folklore from Iowa* (Mem. Amer. Folklore Soc., vol. 29, New York, 1936), pp. 189-190, No. 1035.

Measurement of patients in a recumbent position usually involves children too young to stand. In the Pennsylvania German country, for example, a liver-grown child was laid on a doorsill or threshold. Though this was manifestly not a vertical measurement, healing was predicated upon the child's outgrowing the measurement, in accordance with the general stipulations of "outgrowing." [63] Unique in horizontal measurement, and very rare, is the method of measuring an asthmatic child in Illinois. Here stakes were driven at the head and at the foot of the child lying on the ground under the eaves of the house, and it was believed that, as the child outgrew the distance between the stakes, he would outgrow his asthma.[64]

As the work of Grabner and others show, measuring is a form of curative magic that goes back to the Middle Ages in Europe, and perhaps back to Old Testament times.[65] Little is known historically, however, of the specific kinds of measurement treated in this paper or of the prevalence of these kinds of practices outside of Europe and America. It is my hope, therefore, that the examples offered here may serve as leads for others to follow in establishing larger patterns of belief, and perhaps in opening up speculations of the sort included in my opening remarks as to the symbolic extension of self. Finally, I hope that enough was said in the early paragraphs of the present paper to induce others to explore further the unit of measure itself as a possible intermediate agent in the magical transfer of disease. Most fruitful, it seems to me, would be the study, not only of sticks and similar kinds of rigid and portable measuring units, but also of string and other kinds of lineal measure that are either symbolically disposed of or are rendered inert by storage or made permanently harmless by sequestering or by consignment to the realm of forgetfulness and oblivion. These are questions of considerable interest to scholars in the field of folk medicine where much basic work still remains to be done in philosophic and conceptual matters having to do with measuring, plugging, and the magical transfer of disease.

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[60] William Rensick Riddell, "Some old Canadian folk medicine," *The Canada Lancet and Practitioner*, Aug. 1934, *83:* 42; Creighton, *op. cit.* (n. 42 above), pp. 194-195, nos. 6, 9; Brendle and Unger, *op. cit.* (n. 37 above), p. 140; *Brown Coll.*, VI, 32, no. 176; VI, 55, no. 327; *West Virginia Folklore*, 1964, *14:* 60; *Folk-Lore*, 1897, *8:* 187; Hyatt, 2nd ed., p. 253, nos. 5643-5646.

[61] Edith E. Cutting, *Lore of an Adirondack County* (Ithaca, N.Y.: Cornell University Press, 1944), p. 41.

[62] Catherine M. Relihan, "Folk remedies," *N.Y. Folklore Q.*, 1947: *3:* 81; Brendle and Unger, *op. cit.* (n. 37 above), p. 136; Browne, *op. cit.* (n. 19 above), p. 24, no. 318; Hilda Roberts, "Louisiana superstitions," *J Amer. Folklore*, 1927: *40:*, 166, no. 385; Gertrude C. Davenport, "Folk-cures from Kansas," *J. Amer. Folklore*, 1898, *11:* 132.

[63] Fogel, *op. cit.* (n. 14 above), p. 45, no. 98.

[64] Hyatt, 2nd ed., p. 254, no. 5656.

[65] Grabner, *op. cit.* (n. 2 above), pp. 539-540, *passim.*

THE DISEASE OF MASTURBATION: VALUES AND THE CONCEPT OF DISEASE *

H. TRISTRAM ENGELHARDT, JR.

Masturbation in the 18th and especially in the 19th century was widely believed to produce a spectrum of serious signs and symptoms, and was held to be a dangerous disease entity. Explanation of this phenomenon entails a basic reexamination of the concept of disease. It presupposes that one think of disease neither as an objective entity in the world nor as a concept that admits of a single universal definition: there is not, nor need there be, one concept of disease.[1] Rather, one chooses concepts for certain purposes, depending on values and hopes concerning the world.[2] The disease of masturbation is an eloquent example of the value-laden nature of science in general and of medicine in particular. In explaining the world, one judges what is to be significant or insignificant. For example, mathematical formulae are chosen in terms of elegance and simplicity, though elegance and simplicity are not attributes to be found in the world as such. The problem is even more involved in the case of medicine which judges what the human organism should be (i.e., what counts as "health") and is thus involved in the entire range of human values. This paper will sketch the nature of the model of the disease of masturbation in the 19th century, particularly in America, and indicate the scope of this "disease entity" and the therapies it evoked. The goal will be to outline some of the interrelations between evaluation and explanation.

The moral offense of masturbation was transformed into a disease with somatic not just psychological dimensions. Though sexual overindulgence generally was considered debilitating since at least the time of Hippocrates,[3] masturbation was not widely accepted as a disease until a

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[1] Alvan R. Feinstein, "Taxonomy and logic in clinical data," *Ann. N. Y. Acad. Sci.*, 1969, *161:* 450-459.

[2] Horacio Fabrega, Jr., "Concepts of disease: logical features and social implications," *Perspect. Biol. Med.*, 1972, *15:* 583-616.

[3] For example, Hippocrates correlated gout with sexual intercourse, *Aphorisms*, VI, 30. Numerous passages in the *Corpus* recommend the avoidance of overindulgence especially during certain illnesses.

book by the title *Onania* appeared anonymously in Holland in 1700 and met with great success.[4] This success was reinforced by the appearance of S. A. Tissot's book on onanism.[5] Tissot held that all sexual activity was potentially debilitating and that the debilitation was merely more exaggerated in the case of masturbation. The primary basis for the debilitation was, according to Tissot, loss of seminal fluid, one ounce being equivalent to the loss of forty ounces of blood.[6] When this loss of fluid took place in an other than recumbent position (which Tissot held often to be the case with masturbation), this exaggerated the ill effects.[7] In attempting to document his contention, Tissot provided a comprehensive monograph on masturbation, synthesizing and appropriating the views of classical authors who had been suspicious of the effects of sexual overindulgence. He focused these suspicions clearly on masturbation. In this he was very successful, for Tissot's book appears to have widely established the medical opinion that masturbation was associated with serious physical and mental maladies.[8]

There appears to have been some disagreement whether the effect of frequent intercourse was in any respect different from that of masturbation. The presupposition that masturbation was not in accordance with the dictates of nature suggested that it would tend to be more subversive of the constitution than excessive sexual intercourse. Accounts of this difference in terms of the differential effect of the excitation involved are for the most part obscure. It was, though, advanced that "during sexual intercourse the expenditure of nerve force is compensated by the magnetism of the partner."[9] Tissot suggested that a beautiful sexual partner was of particular benefit or was at least less exhausting.[10] In any event, masturbation was held to be potentially more deleterious since it was unnatural, and, therefore, less satisfying and more likely to lead to a disturbance or disordering of nerve tone.

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[4] René A. Spitz, "Authority and masturbation. Some remarks on a bibliographical investigation," *Yb. Psychoanal.*, 1953, *9:* 116. Also, Robert H. MacDonald, "The frightful consequences of onanism: notes on the history of a delusion," *J. Hist. Ideas,* 1967, *28:* 423-431.

[5] Simon-André Tissot, *Tentamen de Morbis ex Manustrupatione* (Lausannae: M. M. Bousquet, 1758). An anonymous American translation appeared in the early 19th century: *Onanism* (New York: Collins & Hannay, 1832). Interestingly, the copy of Tissot's book held by the New York Academy of Medicine was given by Austin Flint. Austin Flint in turn was quoted as an authority on the effects of masturbation; see Joseph W. Howe's *Excessive Venery, Masturbation and Continence* (New York: Bermingham, 1884), p. 97. Also the American edition of Tissot's book, to show its concurrence with an American authority, added in a footnote a reference to Benjamin Rush's opinion concerning the pernicious consequences of masturbation. See Tissot, *Onanism,* p. 19, and Benjamin Rush's *Medical Inquiries and Observations Upon the Diseases of the Mind* (Philadelphia: Kimber and Richardson, 1812), pp. 348-349; also Tissot, *Onanism,* p. 21.

[6] Simon-André Tissot, *Onanism* (New York: Collins & Hannay, 1832), p. 5.

[7] *Ibid.,* p. 50.

[8] E. H. Hare, "Masturbatory insanity: the history of an idea," *J. Mental Sci.,* 1962, *108:* 2-3. It is worth noting that Tissot, as others, at times appears to have grouped together female masturbation and female homosexuality. See Vern L. Bullough and Martha Voght, "Homosexuality and its confusion with the 'secret sin' in pre-Freudian America," *J. Hist. Med. All. Sci.,* 1973, *28:* 143-155.

At first, the wide range of illnesses attributed to masturbation is striking. Masturbation was held to be the cause of dyspepsia,[11] constrictions of the urethra,[12] epilepsy,[13] blindness,[14] vertigo, loss of hearing,[15] headache, impotency, loss of memory, "irregular action of the heart," general loss of health and strength,[16] rickets,[17] leucorrhea in women,[18] and chronic catarrhal conjunctivitis.[19] Nymphomania was found to arise from masturbation, occurring more commonly in blonds than in brunettes.[20] Further, changes in the external genitalia were attributed to masturbation: elongation of the clitoris, reddening and congestion of the labia majora, elongation of the labia minora,[21] and a thinning and decrease in size of the penis.[22] Chronic masturbation was held to lead to the development of a particular type, including enlargement of the superficial veins of the hands and feet, moist and clammy hands, stooped shoulders, pale sallow face with heavy dark circles around the eyes, a "draggy" gait, and acne.[23] Careful case studies were published establishing masturbation as a cause of insanity,[24] and evidence indicated that it was a cause of hereditary insanity as well.[25] Masturbation was held also to cause an hereditary predisposition to consumption.[26] Finally, masturbation was believed to lead to general debility. "From health and vigor, and intelligence and loveliness of character, they became thin and pale and cadaverous; their amiability and loveliness departed, and in their stead irritability, moroseness and anger were prominent characteristics. . . . The child loses its flesh and becomes pale and weak." [27] The natural history was one of progressive loss of vigor, both physical and mental.

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[9] Howe, *op. cit.* (n. 5 above), pp. 76-77.

[10] Tissot, *op. cit.* (n. 6 above), p. 51.

[11] J. A. Mayes, "Spermatorrhoea, treated by the lately invented rings," *Charleston Med. J. & Rev.*, 1854, *9:* 352.

[12] Allen W. Hagenbach, "Masturbation as a cause of insanity," *J. Ner. Ment. Dis.*, 1879, *6:* 609.

[13] Baker Brown, *On the Curability of Certain Forms of Insanity, Epilepsy, Catalepsy, and Hysteria in Females* (London: Hardwicke, 1866) Brown phrased the cause discreetly in terms of "peripheral irritation, arising originally in some branches of the pudic nerve, more particularly the incident nerve supplying the clitoris. . . ." (p. 7)

[14] F. A. Burdem, "Self pollution in children," *Mass. Med. J.*, 1896, *16:* 340.

[15] Weber Liel, "The influence of sexual irritation upon the diseases of the ear," *New Orleans Med. & Surg. J.*, 1884, *11:* 786-788.

[16] Joseph Jones, "Diseases of the nervous system," *Trans. La. Med. Soc* (New Orleans: L. Graham & Son, 1889), p. 170.

[17] Howe, *op. cit.* (n. 9 above), p. 93.

[18] J. Castellanos, "Influence of sewing machines upon the health and morality of the females using them," *South. J. Med. Sci.*, 1866-1867, *1:* 495-496.

[19] Comment, "Masturbation and ophthalmia," *New Orleans Med. & Surg. J*, 1881-1882, *9:* 67.

[20] Howe, *op. cit.* (n. 5 above), pp. 108-111.

[21] *Ibid.*, pp. 41, 72.

[22] *Ibid.*, p. 68.

In short, a broad and heterogeneous class of signs and symptoms were recognized in the 19th century as a part of what was tantamount to a syndrome, if not a disease: masturbation. If one thinks of a syndrome as the concurrence or running together of signs and symptoms into a recognizable pattern, surely masturbation was such a pattern. It was more, though, in that a cause was attributed to the syndrome providing an etiological framework for a disease entity. That is, if one views the development of disease concepts as the progression from the mere collection of signs and symptoms to their interrelation in terms of a recognized causal mechanism, the disease of masturbation was fairly well evolved. A strikingly heterogeneous set of signs and symptoms was unified and comprehended under one causal mechanism. One could thus move from mere observation and description to explanation.

Since the signs and symptoms brought within the concept of masturbation were of a serious order associated with marked debility, it is not unexpected that there would be occasional deaths. The annual reports of the Charity Hospital of Louisiana in New Orleans which show hospitalizations for masturbation over an eighty-six year period indicate that, indeed, two masturbators were recorded as having died in the hospital. In 1872, the reports show that there were two masturbators hospitalized, one of whom was discharged, the other one having died.[28] The records of 1887 show that of the five masturbators hospitalized that year two improved, two were unimproved, and one died.[29] The records of the hospital give no evidence concerning the patient who died in 1872. The records for 1887, however, name the patient, showing him to have been hospitalized on Tuesday, January 6, 1887, for masturbation. A forty-five year old native of Indiana, a resident of New Orleans for the previous thirty-five years, single, and a laborer, he died in the hospital on April 8, 1887.[30] There is no indication of the course of the illness. It is interesting to note, though, that in 1888 there was a death from anemia associated with masturbation, the cause of death being recorded under anemia. The records indicate that the patient was hospitalized on August 17, 1887, and died on February 11, 1888, was a lifelong resident of New Orleans, and was likewise a laborer and single.[31] His case suggests something concerning the two deaths recorded under masturbation: that they, too, suffered from a debilitating disease whose signs and symptoms were referred to masturbation as the underlying cause. In short, the concept of masturbation as a disease probably acted as a schema for organizing various signs and symptoms which we would now gather under different nosological categories.

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[23] *Ibid.*, p. 73.
[24] Hagenbach, *op. cit.* (n. 12 above), pp. 603-612.
[25] Jones, *op. cit.* (n. 16 above), p. 170
[26] Howe, *op. cit.* (n. 5 above), p. 95.
[27] Burdem, *op. cit.* (n. 14 above), pp. 339, 341.

As with all diseases, there was a struggle to develop a workable nosology. This is reflected in the reports of the Charity Hospital of Louisiana (in New Orleans) where over the years the disease was placed under various categories and numerous nomenclatures were employed. In 1848, for example, the first entry was given as "masturbation," in 1853 "onanism" was substituted, and in 1857 this was changed to "onanysmus." [32] Later, as the records began to classify the diseases under general headings, a place had to be found for masturbation. Initially in 1874, the disease "masturbation" was placed under the heading "Male Diseases of Generative Organs." In 1877 this was changed to "Diseases of the Nervous System," and finally in 1884 the disease of "onanism" was classified as a "Cerebral-Spinal Disease." In 1890 it was reclassified under the heading "Diseases of the Nervous System," and remained classified as such until 1906 when it was placed as "masturbation" under the title of "Genito-Urinary System, Diseases of (Functional Disturbances of Male Sexual Organs)." It remained classified as a functional disturbance until the last entry in 1933. The vacillation in the use of headings probably indicates hesitation on the part of the recorders as to the nature of the disease. On the one hand, it is understandable why a disease, which was held to have such grossly physical components, would itself be considered to have some variety of physical basis. On the other hand, the recorders appear to have been drawn by the obviously psychological aspects of the phenomenon of masturbation to classify it in the end as a functional disturbance.

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[28] *Report of the Board of Administrators of the Charity Hospital to the General Assembly of Louisiana* [for 1872] (New Orleans: The Republican Office, 1873), p. 30.

[29] *Report of the Board of Administrators of the Charity Hospital to the General Assembly of Louisiana* [for 1887] (New Orleans: A. W. Hyatt, 1888), p. 53.

[30] Record Archives of the Charity Hospital of Louisiana [in New Orleans] M S, "Admission Book #41 from December 1, 1885 to March 31, 1888 Charity Hospital," p. 198. I am indebted to Mrs. Eddie Cooksy for access to the record archives.

[31] *Ibid.*, p. 287.

[32] This and the following information concerning entries is taken from a review of the *Report of the Board of Administrators of the Charity Hospital*, New Orleans, Louisiana, from 1848 to 1933. The reports were not available for the years 1850-1851, 1854-1855, 1862-1863, and 1865.

As mentioned, the concept of the disease of masturbation developed on the basis of a general suspicion that sexual activity was debilitating.[33] This development is not really unexpected: if one examines the world with a tacit presupposition of a parallelism between what is good for one's soul and what is good for one's health, then one would expect to find disease correlates for immoral sexual behavior.[34] Also, this was influenced by a concurrent inclination to translate a moral issue into medical terms and relieve it of the associated moral opprobrium in a fashion similar to the translation of alcoholism from a moral into a medical problem.[35] Further, disease as a departure from a state of stability due to excess or under excitation offered the skeleton of a psychosomatic theory of the somatic alterations attributed to the excitation associated with masturbation.[36] The categories of over and under excitation suggest cogent, basic categories of medical explanation: over and under excitation, each examples of excess, imply deleterious influences on the stability of the organism. Jonathan Hutchinson succinctly described the etiological mechanism in this fashion, holding that "the habit in question is very injurious to the nerve-tone, and that it frequently originates and keeps up maladies which but for it might have been avoided or cured." [37] This schema of causality presents the signs and symptoms attendant to masturbation as due to "the nerve-shock attending the substitute for the venereal act, or the act itself, which, either in onanism or copulation frequently indulged, breaks men down." [38] "The excitement incident to the habitual and frequent indulgence of the unnatural practice of masturbation leads to the most serious constitutional effects. . . ." [39] The effects were held to be magni-

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[33] Even Boerhaave remarked that "an excessive discharge of semen causes fatigue, weakness, decrease in activity, convulsions, emaciation, dehydration, heat and pains in the membranes of the brain, a loss in the acuity of the senses, particularly of vision, *tabes dorsalis*, simplemindedness, and various similar disorders." My translation of Hermanno Boerhaave's *Institutiones Medicae* (Viennae: J. T. Trattner, 1775), p. 315, paragraph 776.

[34] "We have seen that masturbation is more pernicious than excessive intercourse with females. Those who believe in a special providence, account for it by a special ordinance of the Deity to punish this crime." Tissot, *op. cit.* (n. 6 above), p. 45.

[35] ". . . the best remedy was not to tell the poor children that they were damning their souls, but to tell them that they might seriously hurt their bodies, and to explain to them the nature and purport of the functions they were abusing." Lawson Tait, "Masturbation. A clinical lecture," *Med. News*, 1888, *53:* 2.

[36] Though it has not been possible to trace a direct influence by John Brown's system

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of medicine upon the development of accounts of the disease of masturbation, yet a connection is suggestive. Brown had left a mark on the minds of many in the 18th and 19th centuries, and given greater currency to the use of concepts of over and under excitation in the explanation of the etiology of disease. Guenter B. Risse, "The quest for certainty in medicine: John Brown's system of medicine in France," *Bull. Hist. Med.*, 1971, *45:* 1-12.

[37] Jonathan Hutchinson, "On circumcision as preventive of masturbation," *Arch. Surg.*, 1890-1891, *2:* 268.

[38] Theophilus Parvin, "The hygiene of the sexual functions," *New Orleans Med. & Surg. J.*, 1884, *11:* 606.

[39] Jones, *op. cit.* (n. 16 above), p. 170. It is interesting to note that documentation for the constitutional effects of masturbation was sought even from post-mortem examination. A report from Birmingham, England, concerning an autopsy on a dead masturbator, concluded that masturbation ". . . seems to have acted upon the cord in the same manner as repeated small haemorrhages affect the brain, slowly sapping its energies, until it succumbed soon after the last application of the exhausting influence, probably through the instrumentality of an atrophic process previously induced, as evidenced by the diseased state of the minute vessels" ([James] Russell, "Cases illustrating the influence of exhaustion of the spinal cord in inducing paraplegia," *Med. Times & Gaz., Lond.*, 1863, *2:* 456). The examination included microscopic inspection of material to demonstrate pathological changes. Again, the explanation of the phenomena turned on the supposed intense excitement attendant to masturbation. "In this fatal vice the venereal passion is carried at each indulgence to the state of highest tension by the aid of the mind, and on each occasion the cord is subjected to the strongest excitement which sensation and imagination in combination can produce, for we cannot regard the mere secretion of the seminal fluid as constituting the chief drain upon the energies of the cord, but rather as being the exponent of the nervous stimulation by which it has been ejaculated" (*Ibid.*, p. 456). The model was one of mental tension and excitement "exhausting" the nervous system by "excessive functional activity" leading to consequent "weakening" of the nervous system. Baker Brown listed eight stages in the progress of the disease in females· hysteria, spinal irritation, hysterical epilepsy, cataleptic fits, epileptic fits, idiocy, mania and finally death; Brown, *op. cit.* (n. 13 above), p. 7.

fied during youth when such "shocks" undermined normal development.[40]

Similarly, Freud remarks in a draft of a paper to Wilhelm Fliess dated February 8, 1893, that "Sexual exhaustion can by itself alone provoke neurasthenia. If it fails to achieve this by itself, it has such an effect on the disposition of the nervous system that physical illness, depressive affects and overwork (toxic influences) can no longer be tolerated without [leading to] neurasthenia. . . . *neurasthenia in males* is acquired at puberty and becomes manifest in the patient's twenties. Its source is masturbation, the frequency of which runs completely parallel with the frequency of male neurasthenia."[41] And Freud later stated, "It is the prolonged and intense action of this pernicious sexual satisfaction which is enough on its own account to provoke a neurasthenic neurosis. . . ."[42] Again, it is a model of excessive stimulation of a certain quality leading to specific disabilities. This position of the theoreticians of masturbation in the 19th century is not dissimilar to positions currently held concerning other diseases. For example, the first Diagnostic and Statistical Manual of the American Psychiatric Association says with regard to "psychophysiologic autonomic and visceral disorders" that "The symptoms are due to a chronic and exaggerated state of the normal physiological expression of emotion, with the feeling, or subjective part, repressed. Such long continued visceral states may eventually lead to structural changes."[43] This theoretical formulation is one that would have been compatible with theories concerning masturbation in the 19th century.

Other models of etiology were employed besides those based upon excess stimulation. They, for the most part, accounted for the signs and symptoms on the basis of the guilt associated with the act of masturbation. These more liberal positions developed during a time of reaction against the more drastic therapies such as Baker Brown's use of clitoridectomy.[44] These alternative models can be distinguished according to whether the guilt was held to be essential or adventitious. Those who held that masturbation was an unnatural act were likely to hold that the associated guilt feelings and anxiety were natural, unavoidable consequences of performing an unnatural act. Though not phrased in the more ethically neutral terms of excess stimulation, still the explanation was in terms of a pathophysiological state involving a departure from biological norms. "The masturbator feels that his act degrades his manhood, while the man who indulges in legitimate intercourse is satisfied that he has fulfilled one of his principal natural functions. There is a healthy instinctive expression of passion in one case, an illegitimate perversion of function in the other." [45] The operative assumption was that when sexual activity failed to produce an "exhilaration of spirits and clearness of intellect" and when associated with anxiety or guilt it would lead to deleterious effects.[46] This analysis suggested that it was guilt, not excitation, which led to the phenomena associated with masturbation. "Now it happens in a large number of cases, that these young masturbators sooner or later become alarmed at their practices, in consequence of some information they receive. Often this latter is of a most mischievous character. Occasionally too, the religious element is predominant, and the mental condition of these young men becomes truly pitiable. . . . The facts are nearly these: Masturbation is not a crime nor a sin, but a vice." [47] Others appreciated the evil and guilt primarily in terms of the solitary and egoistic nature of the act.[48]

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[40] "Any shock to this growth and development, and especially that of masturbation, must for a time suspend the process of nutrition; and a succession of such shocks will blast both body and mind, and terminate in perpetual vacuity." Burdem, *op. cit.* (n. 14 above), p. 339. In this regard, not only adolescent but childhood masturbation was the concern of 19th century practitioners; e.g., Russell, *op. cit* (n. 39 above), p. 456.

[41] Sigmund Freud, *The Standard Edition of the Complete Psychological Works of Sigmund Freud,* I (London: The Hogarth Press, 1971), p. 180.

[42] *Ibid.,* III, "Heredity and the Aetiology of the Neuroses," p. 150.

[43] *Diagnostic and Statistical Manual: Mental Disorders* (Washington, D. C.: American Psychiatric Association, 1952), p. 29.

Such positions concerning etiology graded over into models in which masturbation's untoward signs and symptoms were viewed as merely the result of guilt and anxiety felt because of particular cultural norms, which norms had no essential basis in biology. "Whatever may be abnormal, there is nothing unnatural." [49] In short, there was also a model of interpretation which saw the phenomena associated with masturbation as mere adventitious, as due to a particular culture's condemnation of the act. This last interpretation implied that no more was required than to realize that there was nothing essentially wrong with masturbation. "Our wisest course is to recognize the inevitableness of the vice of masturbation under the perpetual restraints of civilized life, and, while avoiding any attitude of indifference, to avoid also an attitude of excessive horror, for that would only lead to the facts being effectually veiled from our sight, and serve to manufacture artificially a greater evil than that which we seek to combat." [50] This last point of view appears to have gained prominence in the development of thought concerning masturbation as reflected in the shift from the employment of mechanical and surgical therapy in the late 19th century to the use of more progressive means (i.e., including education that guilt and anxiety were merely relative to certain cultural norms) by the end of the century and the first half of the 20th century.[51]

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[44] "Mr. Baker Brown was not a very accurate observer, nor a logical reasoner. He found that a number of semi-demented epileptics were habitual masturbators, and that the masturbation was, in women, chiefly effected by excitement of the mucous membrane on and around the clitoris. Jumping over two grave omissions in the syllogism, and putting the cart altogether before the horse, he arrived at the conclusion that removal of the clitoris would stop the pernicious habit, and therefore cure the epilepsy." Tait, *op. cit.* (n. 35 above), p. 2.

[45] Howe, *op. cit.* (n. 5 above), p. 77.

[46] *Ibid.*, p. 77.

[47] James Nevins Hyde, "On the masturbation, spermatorrhoea and impotence of adolescence," *Chicago Med. J. & Exam.*, 1879, *38:* 451-452.

[48] "There can be no doubt that the habit is, temporarily at least, morally degrading; but if we bear in mind the selfish, solitary nature of the act, the entire absence in it of aught akin to love or sympathy, the innate repulsiveness of intense selfishness or egoism of any kind, we may see how it may be morally degrading, while its effect on the physical and mental organism is practically nil." A. C. McClanahan, "An investigation into the effects of masturbation," *N. Y. Med. J.*, 1897, *66:* 502.

To recapitulate, 19th-century reflection on the etiology of masturbation led to the development of an authentic disease of masturbation: excessive sexual stimulation was seen to produce particular and discrete pathophysiological changes.[52] First, there were strict approaches in terms of disordered nerve-tone due to excess and/or unnatural sexual excitation. Over-excitation was seen to lead to significant and serious physical alterations in the patient, and in this vein a somewhat refined causal model of the disease was developed. Second, there were those who saw the signs and symptoms as arising from the unavoidable guilt and anxiety associated with the performance of an unnatural act. Third, there were a few who appreciated masturbation's sequelae as merely the response of a person in a culture which condemned the activity.

Those who held the disease of masturbation to be more than a culturally dependent phenomenon often employed somewhat drastic therapies. Restraining devices were devised,[53] infibulation or placing a ring in the prepuce was used to make masturbation painful,[54] and no one less than Jonathan Hutchinson held that circumcision acted as a preventive.[55] Acid burns or thermoelectrocautery [56] were utilized to make masturbation painful and, therefore, to discourage it. The alleged seriousness of this disease in females led, as Professor John Duffy has shown, to the employment of the rather radical treatment of clitoridectomy.[57] The classic monograph recommending clitoridectomy, written by the British surgeon Baker Brown, advocated the procedure to terminate the "long continued peripheral excitement, causing frequent and increasing losses of nerve force, . . ." [58] Brown recommended that "the patient having been placed completely under the influence of chloroform, the clitoris [be] freely excised either by scissors or knife—I always prefer the scissors." [59] The supposed sequelae of female masturbation, such as sterility, paresis, hysteria, dysmenorrhea, idiocy, and insanity, were also held to be remedied by the operation.

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[49] *Ibid.*, p. 500.

[50] Augustin J. Himel, "Some minor studies in psychology, with special reference to masturbation," *New Orleans Med. & Surg. J.*, 1907, *60:* 452.

[51] Spitz, *op. cit.* (n. 4 above), esp. p. 119.

[52] That is, masturbation as a disease was more than a mere collection of signs and symptoms usually "running together" in a syndrome. It became a legitimate disease entity, a causally related set of signs and symptoms.

Male masturbation was likewise treated by means of surgical procedures. Some recommended vasectomy [60] while others found this procedure ineffective and employed castration.[61] One illustrative case involved the castration of a physician who had been confined as insane for seven years and who subsequently was able to return to practice.[62] Another case involved the castration of a twenty-two year old epileptic "at the request of the county judge, and with the consent of his father . . . the father saying he would be perfectly satisfied if the masturbation could be stopped, so that he could take him home, without having his family continually humiliated and disgusted by his loathsome habit."[63] The patient was described as facing the operation morosely, "like a coon in a hollow."[64] Following the operation, masturbation ceased and the frequency of fits decreased. An editor of the *Texas Medical Practitioner,* J. B. Shelmire, added a remark to the article: "Were this procedure oftener adopted for the cure of these desperate cases, many, who are sent to insane asylums, soon to succumb to the effects of this habit, would live to become useful citizens."[65] Though such approaches met with ridicule from some quarters,[66] still various novel treatments were devised in order to remedy the alleged sequelae of chronic masturbation such as spermatorrhea and impotency. These included acupuncture of the prostate in which "needles from two to three inches in length are passed through the perineum into the prostate gland and the neck of the bladder. . . . Some surgeons recommend the introduction of needles into the testicles and spermatic cord for the same purpose."[67] Insertion of electrodes into the bladder and rectum and cauterization of the prostatic urethra were also utilized.[68] Thus, a wide range of rather heroic methods were devised to treat masturbation and a near fascination developed on the part of some for the employment of mechanical and electrical means of restoring "health."

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[53] C. D. W. Colby, "Mechanical restraint of masturbation in a young girl," *Med. Record in N. Y.*, 1897, *52:* 206.

[54] Louis Bauer, "Infibulation as a remedy for epilepsy and seminal losses," *St. Louis Clin. Reçord,* 1879, *6:* 163-165. See also Gerhart S. Schwarz, "Infibulation, population control, and the medical profession," *Bull. N. Y. Acad. Med.*, 1970, *46:* 979, 990.

[55] Hutchinson, *op. cit.* (n. 37 above), pp. 267-269.

[56] William J. Robinson, "Masturbation and its treatment," *Am. J. Clin. Med.*, 1907, *14:* 349.

[57] John Duffy, "Masturbation and clitoridectomy A nineteenth-century view," *J.A.M.A.*, 1963, *186:* 246-248.

[58] Brown, *op. cit.* (n. 13 above), p. 11.

[59] *Ibid.*, p. 17.

[60] Timothy Haynes, "Surgical treatment of hopeless cases of masturbation and nocturnal emissions," *Boston Med. & Surg. J.*, 1883, *109:* 130.

[61] J. H. Marshall, "Insanity cured by castration," *Med. & Surg. Reptr.*, 1865, *13:* 363-364.

[62] "The patient soon evinced marked evidences of being a changed man, becoming quiet, kind, and docile." *Ibid.*, p. 363.

There were, though, more tolerant approaches, ranging from hard work and simple diet[69] to suggestions that "If the masturbator is totally continent, sexual intercourse is advisable."[70] This latter approach to therapy led some physicians to recommend that masturbators cure their disease by frequenting houses of prostitution,[71] or acquiring a mistress.[72] Though these treatments would appear ad hoc, more theoretically sound proposals were made by many physicians in terms of the model of excitability. They suggested that the disease and its sequelae could be adequately controlled by treating the excitation and debility consequent upon masturbation. Towards this end, "active tonics" and the use of cold baths at night just before bedtime were suggested.[73] Much more in a "Brownian" mode was the proposal that treatment with opium would be effective. An initial treatment with 1/12 of a grain of morphine sulfate daily by injection was followed after ten days by a dose of 1/16 of a grain. This dose was continued for three weeks and gradually diminished to 1/30 of a grain a day. At the end of a month the patient was dismissed from treatment "the picture of health, having fattened very much, and lost every trace of anaemia and mental imbecility."[74] The author, after his researches with opium and masturbation, concluded, *"We may find in opium a new and important aid in the treatment of the victims of the habit of masturbation by means of which their moral and physical forces may be so increased that they may be enabled to enter the true physiological path."*[75] This last example eloquently collects the elements of the concept of the disease of masturbation as a pathophysiological entity: excitation leads to physical debilitation requiring a physical remedy. Masturbation as a pathophysiological entity was thus incorporated within an acceptable medical model of diagnosis and therapy.

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[63] R. D. Potts, "Castration for masturbation, with report of a case," *Texas Med. Practitioner,* 1898, *11:* 8.

[64] *Ibid.,* p. 8.

[65] *Ibid.,* p. 9.

[66] Editorial. "Castration for the relief of epilepsy," *Boston Med. & Surg. J.,* 1859, *60:* 163.

[67] Howe, *op. cit.* (n. 5 above), p. 260.

[68] *Ibid.,* pp. 254-255, 258-260.

[69] Editorial, "Review of European legislation for the control of prostitution," *New Orleans Med. & Surg. J.,* 1854-1855, *11:* 704.

[70] Robinson, *op. cit.* (n. 56 above), p. 350

[71] Parvin, *op. cit.* (n. 38 above), p. 606.

[72] Mayes, *op. cit.* (n. 11 above), p. 352.

In summary, in the 19th century, biomedical scientists attempted to correlate a vast number of signs and symptoms with a disapproved activity found in many patients afflicted with various maladies. Given an inviting theoretical framework, it was very conducive to think of this range of signs and symptoms as having one cause. The theoretical framework, though, as has been indicated, was not value free but structured by the values and expectations of the times. In the 19th century, one was pleased to think that not "one bride in a hundred, of delicate, educated, sensitive women, accepts matrimony from any desire of sexual gratification: when she thinks of this at all, it is with shrinking, or even with horror, rather than with desire."[76] In contrast, in the 20th century, articles are published for the instruction of women in the use of masturbation to overcome the disease of frigidity or orgasmic dysfunction.[77] In both cases, expectations concerning what should be significant structure the appreciation of reality by medicine. The variations are not due to mere fallacies of scientific method,[78] but involve a basic dependence of the logic of scientific discovery and explanation upon prior evaluations of reality.[79] A sought-for coincidence of morality and nature gives goals to explanation and therapy.[80] Values influence the purpose and direction of investigations and treatment. Moreover, the disease of masturbation has other analogues. In the 19th century, there were such diseases in the South as "Drapetomania, the disease causing slaves to run away," and the disease "Dysaesthesia Aethiopis or hebetude of mind and obtuse sensibility of body—a disease peculiar to negroes—called by overseers 'rascality'." [81] In Europe, there was the disease of *morbus democritus*.[82] Some would hold that current analogues exist in diseases such as alcoholism and drug abuse.[83] In short, the disease of masturbation indicates that evaluations play a role in the development of explanatory models and that this may not be an isolated phenomenon.

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[73] Haynes, *op. cit.* (n. 60 above), p. 130.

[74] B. A. Pope, "Opium as a tonic and alternative; with remarks upon the hypodermic use of the sulfate of morphia, and its use in the debility and amorosis consequent upon onanism," *New Orleans Med. & Surg. J.*, 1879, *6:* 725.

[75] *Ibid.*, p. 727.

[76] Parvin, *op. cit.* (n. 38 above), p. 607.

This analysis, then, suggests the following conclusion: although vice and virtue are not equivalent to disease and health, they bear a direct relation to these concepts. Insofar as a vice is taken to be a deviation from an ideal of human perfection, or "well-being," it can be translated into disease language. In shifting to disease language, one no longer speaks in moralistic terms (e.g., "You are evil"), but one speaks in terms of a deviation from a norm which implies a degree of imperfection (e.g., "You are a deviant"). The shift is from an explicitly ethical language to a language of natural teleology. To be ill is to fail to realize the perfection of an ideal type; to be sick is to be defective rather than to be evil. The concern is no longer with what is naturally, morally good, but what is naturally beautiful. Medicine turns to what has been judged to be naturally ugly or deviant, and then develops etiological accounts in order to explain and treat in a coherent fashion a manifold of displeasing signs and symptoms. The notion of the "deviant" structures the concept of disease providing a purpose and direction for explanation and for action, that is, for diagnosis and prognosis, and for therapy. A "disease entity" operates as a conceptual form organizing phenomena in a fashion deemed useful for certain goals. The goals, though, involve choice by man and are not objective facts, data "given" by nature. They are ideals imputed to nature. The disease of masturbation is an eloquent example of the role of evaluation in explanation and the structure values give to our picture of reality.

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[77] Joseph LoPiccolo and W. Charles Lobitz, "The role of masturbation in the treatment of orgasmic dysfunction," *Arch. Sexual Behavior*, 1972, *2:* 163-171.

[78] E. Hare, *op. cit.* (n. 8 above), pp. 15-19.

[79] Norwood Hanson, *Patterns of Discovery* (London: Cambridge University Press, 1965).

[80] Tissot, *op. cit.* (n. 6 above), p. 45. As Immanuel Kant, a contemporary of S.-A. Tissot remarked, "Also, in all probability, it was through this moral interest [in the moral law governing the world] that attentiveness to beauty and the ends of nature was first aroused." (*Kants Werke,* Vol 5, *Kritik der Urtheilskraft* [Berlin: Walter de Gruyter & Co., 1968], p. 459, A 439. My translation.) That is, moral values influence the search for goals in nature, and direct attention to what will be considered natural, normal, and non-deviant. This would also imply a relationship between the aesthetic, especially what was judged to be naturally beautiful, and what was held to be the goals of nature.

[81] Samuel A Cartwright, "Report on the diseases and physical peculiarities of the negro race," *New Orleans Med. & Surg. J.*, 1850-1851, *7:* 707-709. An interesting examination of these diseases is given by Thomas S. Szasz, "The sane slave," *Am. J. Psychoth.*, 1971, *25:* 228-239.

[82] Heinz Hartmann, "Towards a concept of mental health," *Brit. J. Med. Psychol.*, 1960, *33:* 248.

[83] Thomas S. Szasz, "Bad habits are not diseases: a refutation of the claim that alcoholism is a disease," *Lancet,* 1972, *2:* 83-84; and Szasz, "The ethics of addiction," *Am. J. Psychiatry,* 1971. *128:* 541-546.

CHEMICALS AND MEAT: THE EMBALMED BEEF SCANDAL OF THE SPANISH-AMERICAN WAR *

EDWARD F. KEUCHEL

In his later life Carl Sandburg reflected on his youthful service in the Spanish-American War and gave the following description of a shipboard meal:

> We were divided into messes of eight men for rations. A tin of "Red Horse" would be handed to one man who opened it. He put it to his nose, smelled of it, wrinkled up his face, and took a spit. The next man did the same and the next till the eight men of the mess had smelled, grimaced, and spit. Then that tin of "Red Horse" was thrown overboard for any of the fishes of the Atlantic Ocean who might like it. Somehow we got along on cold canned beans, occasional salmon, and the reliable hardtack. What we called "Red Horse" soon had all our country scandalized with its new name of "Embalmed Beef." It *was* embalmed. We buried it at sea because it was so duly embalmed with all flavor of life and every suck of nourishment gone from it though having nevertheless a putridity of odor more pungent than ever reaches the nostrils from a properly embalmed cadaver.[1]

This is the poet's description of one of the most sensational aspects of the Spanish-American War—a war that owed much to sensationalism. At issue was the charge that soldiers in the army camps of the South, and in Cuba and Puerto Rico had been fed beef preserved by harmful chemicals, and that this "embalmed beef" was responsible for much of the sickness and death of the war. Indeed, in this "splendid little war," [2] as John Hay called it, the United States lost only 280 men killed in battle, but 2,630 to other causes.[3]

Meat processing had undergone considerable development in the United States during the nineteenth century when refrigerated beef and canned beef became common. Concentration of slaughtering started at Cincinnati in the 1820s when salted pork was packed into barrels and shipped down the Ohio and Mississippi rivers. Chicago became the dominant packing center in the 1860s when cattle were loaded into freight cars and shipped east. More significant was the development of the refrigerator car which allowed dressed carcasses to be distributed to areas of consumption—George Henry Hammond, Gustavus F. Swift, and Philip D. Armour are the names associated with this latter development. From the technical standpoint Swift achieved success when he captured the New York market in 1882. Swift shipped dressed beef from Chicago in refrigerator cars to a refrigerated warehouse in New York where it in turn was distributed to butcher shops. Considerable savings in freight charges were effected as a thousand-pound steer only dressed out to five hundred and fifty pounds. Refrigerated beef met some public opposition as consumers were suspicious of eating meat a week or more after it had been killed in Chicago, yet the price was so much lower than locally slaughtered animals that such prejudices diminished. By the early 1890s most of the suspicions against refrigerated beef had been removed by urban consumers although resentments against the big meat packers—the so-called "Beef Trust"—remained as an important part of the anti-trust movement.[4]

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* Read at the 46th annual meeting of the American Association for the History of Medicine, Cincinnati, Ohio, May 4, 1973.

[1] Carl Sandburg, *Always the Young Strangers* (New York. Harcourt, Brace, 1952), p. 417.

[2] Frank Freidel uses Hay's expression as the title for his history of the war. *The Splendid Little War* (Boston: Little, Brown, 1958).

[3] "Report of the Commission to Investigate the Conduct of the War Department in the War with Spain," 56 Cong., 1 sess., Sen. Doc. No 221, Ser. 3859, 114. These figures are only for the period of actual fighting in Cuba and Puerto Rico. From May 1, 1898, to April 30, 1899, the official figures are 5,438 died of disease and 968 killed in battle or died of wounds, injuries or accident.

Canned beef had been available in the United States since before the Civil War although its market was limited. During the Civil War it was used but was not a part of the standard army ration. It was accepted for use as a travel ration in 1878 and this was its intended purpose during the Spanish-American War. That is, canned beef was to be used while the troops were in transit or in the field and could not readily be supplied with fresh beef. There were constant criticisms from both the military and general public over the quality of canned beef. The designation "canner" was generally recognized by meat packers as the lowest market grade. Although the usual trade name was "canned roast beef" the product was, more properly, boiled beef. The meat was boiled, packed into cans in broth, and placed in steam retorts for the sterilization process. Since poor quality beef was used, undoubtedly much of the resultant product was gristly and stringy.[5]

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[4] J. Ogden Armour, *The Packers, the Private Car Lines and the People* (Philadelphia: H. Altimus, 1906), pp. 17-25; Siegfried Giedion, *Mechanization Takes Command* (New York: Oxford Univ. Press, 1948), pp. 209-240; Herman Hirschauer, *The Dark Side of the Beef Trust* (Jamestown, N.Y.: T. Z. Root, 1905), pp. 12-45; Louis F. Swift, *The Yankee of the Yards* (Chicago: A. W. Shaw, 1927), pp. 8-9, 66-69, 131.

[5] Individuals, usually officers, sometimes purchased canned foods such as beef, tomatoes,

Rumblings that all was not well with the troops started in July, 1898, when the press turned attention to the presence of typhoid and poor sanitation in the training camps of the South, especially Camp Thomas in Chickamauga Park, Georgia, Camp Alger, Virginia, and Camp Cuba Libre at Jacksonville, Florida. Toward the end of the summer additional stories were released that bad meat as well as poor sanitation was standard fare for the American soldier. One distraught Chicago mother complained to President McKinley that she had been reading in the papers of soldiers grousing about bad beef and added: "We are living under a generous Government, with a good, kind man at its head willing to give the Army the best possible, and yet thieving corporations will give the boys the worst." [6]

Such charges were of serious concern to the McKinley administration. An election was upcoming in November and Republican control of Congress could be weakened or lost. The cabinet office with primary responsibility for army contracts was the War Department headed by Russell A. Alger. Alger requested that a commission be appointed to investigate the conduct of the War Department during the war. On September 24, 1898, a group appointed by McKinley convened and started the investigation. This commission was headed by General Grenville M. Dodge and is usually called the Dodge commission.[7]

Between September 24 and December 21, 1898, the commission met in seventeen cities, towns and army camps connected with the war,

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or peaches to supplement the army diet during the Civil War, but it was not until 1867 that the army made official purchases of canned foods. See Thomas Wilson, *Notes on Canned Goods, Prepared Under the Direction of the Commissary General of Subsistence, U.S. Army* (Washington, D.C., 1870), pp. 32-34. *The Chicago Daily Drovers Journal,* March 25, 1905, lists the following market grades. "choice to fancy," "good to choice," "medium to good," "common to fair," and at the bottom "canners and cutters." While choice heifers brought as high as $4.80 per 100 lbs., an inferior to fair canner went for $1.50 per 100 lbs.

[6] A Soldier's Mother to President McKinley, Aug 5, 1898, Exhibit 18, "Food Furnished by Subsistence Department to Troops in the Field," 56 Cong, 1 sess, Sen. Doc. No. 270, Ser. 3872, 1989. This letter simply signed "A Soldier's Mother" was used in evidence by the Wade investigating court. At this time the press was becoming increasingly critical of the big meat packers, the so-called "Beef Trust," and published letters of soldiers complaining about bad beef. The White House sent copies of the letter to Armour, Swift and other packers requesting an explanation, but the matter was not so easily resolved.

[7] Russell A. Alger, *The Spanish-American War* (New York: Harper, 1901), p. 376; H. Wayne Morgan, *William McKinley and his America* (Syracuse: Syracuse University Press, 1963), pp. 425-426. Morgan notes that Dodge, the old Iowa general who helped build the Union Pacific Railroad, was distinguished, but was so well known in high Republican circles that opposition Democrats feared a whitewash.

and took testimony from soldiers, ex-soldiers and civilians. The public had pretty much lost interest in the hearings when, on December 21, Major-General Nelson A. Miles appeared before the commission in Washington and shocked the members by testifying that "337 tons of so-called refrigerated beef, which you might called embalmed beef" had been sent to the troops in Puerto Rico the previous summer. Miles said he did not know "what may have been injected into it," as he understood the process to be "secret."[8]

Miles was a somewhat reluctant witness. Initially he refused to volunteer testimony and only took the stand after receiving personal invitations from General Dodge and the Adjutant-General. Upon appearing he refused to be sworn as other witnesses had been but told the commission he was responsible for what he said. After testifying Miles and his wife took a night train to Cincinnati where they stayed at the mansion of his brother-in-law, Frank Wiborg. In Cincinnati Miles reaffirmed his charges and complained that newspaper coverage of his previous day's remarks was rather meager. He told reporters that he intended to conduct his own investigation of the army beef scandal. He said his suspicions about meat were aroused several months earlier and that he had found bad beef in Florida, Puerto Rico and Cuba. The general would not name his source but told reporters that he had a medical authority to substantiate his charge that chemicals were largely responsible for the sickness in the army.[9]

In a later appearance before the Dodge commission Miles supported his charges by producing a letter from a Pittsburgh physician, Major W. H. Daly, Chief Surgeon, U.S. Volunteers. The letter, dated September 21, 1898, had originally been sent to the Assistant Adjutant-General. Daly stated that he conducted inspections of camps and troopships at Tampa, Jacksonville, Chickamauga and Puerto Rico, and that he found fresh beef apparently preserved with secret chemicals. At Tampa he saw a quarter of beef hanging in the sun above the deck of the transport *Comal.* The meat was part of an experiment and was untainted after sixty hours of exposure. Such a feat, he stated, would have been impossible in that summer climate without the use of deleterious preservatives such as boric and salicylic acid. On the transport *Panama,* sailing from Puerto Rico, Daly said the ship's fresh beef looked well but had an odor similar to an embalmed human body, and after cooking tasted like decomposed boric acid. Some of the soldiers said it "smelt like a bouquet of cesspools." The officers in charge had this meat dumped at sea. Under questioning Daly admitted that he made no chemical tests at the time of his inspections though he did bottle some broth from a kettle on the *Panama* which he later analyzed as containing salicylic and boric acid.[10]

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[8] "Report of the Commission," Ser. 3859, 155-156, Ser. 3865, 3256-3261. Note that Miles made the charge against refrigerated beef: that is, fresh beef shipped and kept under refrigeration He did not include canned beef under this heading, but the public frequently confused the two. Miles complained that canned beef was beef with the extract removed by boiling. In other words it was beef pulp with the nutriment removed. See Miles' interview, *New York Herald,* Feb. 1, 1899.

[9] *New York Journal,* Dec. 23, 1898; "Report of the Commission," Ser. 3859, 80.

The charges were sensational but the furor might have been short-lived. This was not to be the case, however, for Commissary-General Charles Patrick Eagan, outraged by the implication that he had purchased poisoned beef for the army, reacted vigorously and demanded a hearing before the Dodge commission. On January 12, 1899, Eagan offered effective counter-testimony but was most intemperate in his personal denunciation of Miles. Eagan accused Miles of deliberate lies, and because of this abusive language he was court-martialed for conduct unbecoming an officer and suspended from rank and duty for six years.[11]

The Miles-Eagan controversy kept the public's attention during January of 1899. Jacksonville editor George W. Wilson called it "America's Dreyfus case" because it attacked the foundations of the government—the confidence of the American people in the stability and uprightness of their own institutions. At issue, he said, was whether the commissary department in time of war bartered the health of the soldiers for gain; or, if untrue, was there a commanding general who would make false charges in order to eliminate a disagreeable official? In his contemporary account of the war and its aftermath Murat Halstead used the same analogy stating that the embalmed beef sensation rivaled the Dreyfus scandal in France. Secretary of War Alger doubted if the war with Spain would ever be referred to in his generation without the "odious hue and cry of 'rotten beef.' "[12]

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[10] "Report of the Commission," Ser. 3859, 155-158, Ser. 3865, 3258-3259; "Food Furnished by Subsistence Department," Ser. 3871, 1352-1353. Daly gave no particulars as to his testing procedures. Some of the tests at that time were questionable. In 1906 W. D. Bigelow, Chief of the Division of Foods of the Bureau of Chemistry, U.S.D.A., complained that the current tests for determining the presence of salicylic acid were inadequate. Bigelow to B. H. Smith, Aug. 21, 1906, Letterpress Book 193, Letters Sent 1878-1907, Bur. of Chem., Record Group 97, National Archives. Hereafter RG and NA. In testimony before the Wade court Bigelow was suspicious of the charge that boric and salicylic acid had been used on meat. His main objection was that meat so-treated would be as bitter as quinine and could not be disguised. Bigelow said he had seen formaldehyde used in minute quantities in milk, but if applied to meat would easily be detected by its pungent odor as well as by chemical analysis. Ser. 3871, 1640-1644.

[11] "Report of the Commission," Ser. 3859, 545-547; *Tampa Morning Tribune,* Jan. 13, 1899; *The Florida Times-Union and Citizen* (Jacksonville), Jan. 13, 14, 1899; Alger, *Spanish-American War,* pp. 381-383. In 1901 Eagan, who had been an officer for thirty-six years, was reinstated in the service and immediately placed on the retirement list by his own request.

Since the investigation by the Dodge commission was coming to an end President McKinley created a new court of inquiry to investigate the beef question more fully. This court headed by Major-General James F. Wade, U.S. Volunteers, was convened on February 9, 1899, and is commonly called the Wade Beef Board. This court took testimony from February 20 to April 24, 1899, resulting in a 4,700 typewritten page study, but, in essence, little new was added to what the Dodge commission had already covered.[13]

As to the charges made by Miles an analysis can best be made by separating the issue into three areas of investigation: the Powell process experiment, fresh and canned beef used in army rations, and sickness among the troops. The Powell process experiment concerned the beef that Major Daly saw on board the *Comal* in Tampa Bay in the summer of 1898. This was the meat that had been hanging exposed for sixty hours and was still untainted. Theodore Roosevelt had observed this beef before embarking for Cuba with the Rough Riders.[14]

Alexander B. Powell was a New York City meat dealer who specialized in supplying steamships, hotels and restaurants. When the war started Powell wrote to President McKinley and told him that he had learned from H. O. Armour of Armour and Company that the government anticipated shipping fresh beef to Cuba. Powell told the President that he had much experience in shipping fresh beef to warm areas as he regularly supplied Florida hotels including the Ponce de Leon and the Alcazar in St. Augustine, the St. James in Jacksonville, and the Tampa Bay Hotel. He stated that his experiences indicated that meat would not last long without refrigeration in southern climates, but that he had a process which would preserve meat for four to ten days without refrigeration. Powell proposed that he be authorized to process meat purchased for army use in Florida and Cuba. His fee would be one-half cent per pound. McKinley turned the letter over to the War Department and in reply Captain George B. Davis of the commissary department told Powell that the army did not intend to purchase preserved meat.[15]

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[12] *The Florida Times-Union and Citizen,* Jan. 14, 1899; Murat Halstead, *Full Official History of the War with Spain* (Chicago, 1899), p. 647; Alger, *Spanish-American War,* p. 406.

[13] "Food Furnished by Subsistence Department," Ser. 3870, 3. Before the Wade court Miles continued to display the arrogance he had previously shown before the Dodge commission He appeared before the court in fatigue uniform rather than the full dress of the court. When court recorder Colonel Davis asked him if he reported the bad beef to the commissary-general's office at the time, Miles snapped back defiantly, "I am not required, sir, to report to the commissary-general." *Florida Times-Union and Citizen,* Feb. 21, 1899.

[14] "Report of the Commission," Ser. 3859, 155-158; Roosevelt to Nelson Appleton Miles, Jan. 9, 1899, Elting E. Morison, ed., *The Letters of Theodore Roosevelt* (Cambridge: Harvard University Press, 1951) II, pp. 903-904

In testimony before the Wade court Powell related that in May of 1898 he treated six carcasses of beef provided by Armour and Company as an experiment in preservation. Powell stated under oath that his process employed sulfur fumigation and involved no boric or salicylic acid. The meat was treated in Chicago where one carcass remained while the other five were shipped to Tampa. Powell went to Tampa to observe the meat. Four of the carcasses were placed on board the *Comal* where they reportedly remained in good condition for seventy-five to eighty hours. When Major Daly saw a quarter of this beef hanging above the deck of the ship, Colonel J. F. Weston of the commissary department told him that it had been hanging exposed for over sixty hours. Daly said the meat looked fresh and he cut off a piece which he cooked and ate the following day. Daly testified that he became nauseated after eating the meat but could not be sure if it was the cause as the day was hot and he had been thrown three times while horseback riding that day. Powell testified that only the six experimental carcasses were processed this way and that his process was never used by either Armour or Swift. He added that the packers received no contracts for meat preserved this way.[16]

Colonel Weston, Eagan's successor, testified that the commissary department had nothing official to do with the Powell experiment. He stated that Armour and Company informed him that they were shipping some treated beef and were interested in learning how long it would remain untainted on board ship. Weston stated he was merely accommodating the beef people in allowing the meat on the *Comal.* He gave a favorable report on the meat but reaffirmed that, to his knowledge, Armour did not use the process during the war and no contracts were made with Powell.[17]

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[15] Powell to William McKinley, May 20, 1898, Davis to Alexander B. Powell, May 26, 1898, Exhibit 30, "Food Furnished by Subsistence Department," Ser. 3872, 2509-2510 In testimony before the Wade court Powell was reluctant to admit he shipped his processed beef to Florida hotels for fear people would ask the hotels if their meat had been purchased from "the embalmer." Ser. 3871, 1476.

[16] "Food Furnished by Subsistence Department," Ser. 3871, 1351-1352, 1475-1492; *Florida Times-Union and Citizen*, Feb. 23, 1899.

The American soldier in the Spanish-American War was primarily a beef and bacon eater. The standard daily ration for one soldier called for twelve ounces of bacon, pork or canned beef, or twenty ounces of fresh beef. In addition the ration called for eighteen ounces of soft bread or sixteen ounces of hardtack. Cornbread could be used in lieu of wheat bread. To every one hundred rations were allowed the following: fifteen pounds of beans or peas, or ten pounds of rice or hominy, ten pounds of green or eight pounds of roasted coffee, fifteen pounds of sugar, four pounds of salt, four quarts of vinegar, and four ounces of pepper.[18]

During the Civil War fresh beef had been supplied from animals driven on the hoof and slaughtered as required by cooks in the field. In the Spanish-American War it was decided to supply the army with refrigerated beef. That is, the animals would be slaughtered in Kansas City, Chicago or other packing centers and delivered in refrigerated railroad cars and ships. In principle the program was sound as this system had been used to supply domestic and European markets for over two decades. In practice during wartime, however, many problems did arise.[19] Swift and Company received the initial contract to supply refrigerated beef. The contract stipulated that the company would construct refrigerated facilities in Cuba from which the meat could be distributed. Refrigeration apparatus did not reach Santiago until July 27, but even then it was not unloaded by the quartermaster until long after its arrival. Agents of the company complained to General Shafter but the unit was not ready for operation until September 21—after the fighting was over. On the *Panama,* the ship on which Major Daly made his observations that the beef had an odor similar to an embalmed cadaver, Captain R. J. Irvine, the commissary, said that the ice given to the ship was not sufficient for refrigeration and the beef, which was in good condition when loaded, spoiled in transit and was dumped at sea. Dr. Charles R. Blundell, a New Jersey physician stationed at Camp Cuba Libre, said some of the companies did not have ice boxes and could only cover the meat even though summer temperatures were often in the nineties. Describing what must have been a low point in sanitation Colonel H. G. Sharpe told the Wade court that wagons at Camp Thomas had not been cleaned before beef was hauled and they were crawling with maggots hatched in the slime of previous manure and garbage haulings. Although the Wade court found numerous accounts of poor handling of meat within military units, it concluded that the refrigerated beef sold to the army was essentially sound but that there was neglect in arrangements for its local delivery and care prior to consumption.[20]

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[17] *Florida Times-Union and Citizen,* Feb. 24, 1899; *New York Times,* Apr. 7, 9, 1899.

[18] "Report of the Commission," Ser. 3859, 565-567; "Food Furnished by Subsistence Department," Ser. 3872, 1941-1942. During the five months of the war the commissary department purchased 10,330,486 lbs. of fresh beef, 4,896,854 lbs. of canned beef, 2,044,355 lbs. of canned corned beef, and 7,038,389 lbs. of bacon. Only 330,919 lbs. of pork, 2,259 lbs. of mutton, and 52,352 lbs of fish were purchased. The principal vegetables were potatoes (9,197,882 lbs), canned tomatoes (20,837,224 cans), and beans (6,054,059 lbs.). Of the cereal grains 17,898,299 lbs. of flour, 11,512,608 lbs. of hardtack, 1,187,722 lbs. of oatmeal, and 945,276 lbs. of cornmeal were purchased. In smaller quantities were such items as canned and dried fruit, jams and jellies, canned corn, dried peas and other vegetables, coffee, tea and other foods.

[19] There had been problems with supplying beef from animals slaughtered in the field during the Civil War. The commissary department anticipated many difficulties if beef animals had to be landed in the surf, as were the horses and mules, and herded to the

Canned beef was the object of most of the criticism by the troops themselves. Canned beef was not new to the army, having been used as a travel ration since 1878. General Miles had made it quite clear that the term "embalmed" applied to refrigerated not canned beef, although the press and the public came to use the term for all army beef. Most of the soldiers' complaints were directed against canned beef. Some critics condemned canned beef as harmful while others questioned its quality. Of the many soldiers who testified in the Dodge and Wade investigations the most frequent complaint was that canned beef was stringy, gristly, unpalatable, and at times nauseating. Many of the men refused to eat it.[21]

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front areas over rough terrain. Moreover, the flesh of such animals would not be good for food unless the animals rested for several days prior to slaughter. The proposal to supply beef from cold storage was regarded as a way to overcome these problems.

[20] "Food Furnished by Subsistence Department," Ser. 3870, 599-600, Ser. 3871, 1667-1675, 1876-1889; *Florida Times-Union and Citizen,* Jan. 17, March 12, 1899.

[21] "Food Furnished by Subsistence Department," Ser. 3871, 1570-1573, Ser. 3872, 1910-1915; John Bigelow, Jr., *Reminiscences of the Santiago Campaign* (New York. 1899), p. 74;

In actuality the main problem with canned beef was poor quality. Moreover, without suitable condiments and vegetables the canned product probably did lack flavor. Considering the manner in which it was served it is understandable that the soldiers complained. Canned beef was intended as a travel ration which also included canned tomatoes, hardtack and coffee. It was frequently served cold and constituted a monotonous diet when only canned tomatoes and hardtack accompanied it. The Second Wisconsin Volunteers arrived at Camp Thomas after fourteen straight days of cold canned beef, canned tomatoes and hardtack. In the field in Cuba many days passed before the soldiers could obtain supplements to the travel ration. The Sixteenth Infantry lived on it for weeks with only an occasional variation of bacon, onions and potatoes. The Wade court concluded that canned beef would have been acceptable if adequate vegetables and potatoes had been available, but as used the court regarded the purchase of it as a mistake.[22]

During the investigations by the Dodge commission and the Wade board the Department of Agriculture's Bureau of Chemistry, headed by Harvey W. Wiley, was called upon to present evidence on the beef question. Wiley, the apostle of the pure food movement in the United States, was an inveterate foe of chemical preservatives. He and his chief assistant, W. D. Bigelow, were called upon by Secretary of Agriculture James Wilson to conduct analyses of canned beef. Wiley tested samples of canned beef furnished to soldiers during the war as well as samples secured in the open market. He found no traces of chemicals except salt used as seasoning. The products were tested for the presence of borax, boric acid, sulfites, sulfurous acid, salicylic acid and benzoic acid—the preservatives most likely to be employed.[23]

William Camman, ed., *The History of Troop "A" in the Spanish-American War* (New York, 1899), p 103.

Even before the war was formally declared the government placed heavy orders for canned beef. Edward S. Judge, editor of the leading canned food trade journal, estimated that it would take the four big meat canners at least a month to fill the initial order. *The Canning Trade,* XX (April 22, 1898), n.p.n.

[22] "Food Furnished by Subsistence Department," Ser. 3871, 1869-1885, Ser. 3872, 2581-2582.

[23] Wilson to Harvey W. Wiley, Jan. 9, 1899, Wiley Papers, box 40, Manuscripts Division, Library of Congress, hereafter MD and LC; Wiley to Secretary of Agriculture, Jan. 18, 1899, Letterpress Book 73, Letters Sent 1878-1907, Bur. of Chem., RG 97, NA. Wiley told Wilson he was opposed to chemical preservatives because "chemical substances which preserve food from decay without the body interfere with digestion in the body." Wiley later conducted tests with volunteers—his "poison squad"—and found that large doses of borax and boric acid caused headache and constipation. See Preservative Reports, Special

Upon hearing Major Daly's testimony on January 20, 1899, the Dodge commission ordered samples of refrigerated beef from the army camps of Florida, Cuba, and Puerto Rico to be sent to Washington for analysis. Wiley tested these as well as samples he obtained from camps in South Carolina, Georgia and Alabama. The samples were sent to Wiley in mason jars packed in ice. Some were slightly tainted upon arrival, but in no case was there any evidence of chemical preservation.[24]

With regard to nutritive value Wiley found that fresh beef tended to have an average fat content higher than canned beef, and the canned product had a higher average protein content. The bureau chief concluded this was because the packers trimmed off some of the fat going into the canned item. Indeed, Wiley thought this would make the canned product superior in the tropics where fat was not so important.[25]

Wiley was convinced that the whole trouble in connection with army beef had arisen from two causes: the total army diet, and a hot climate conducive to the development of microorganisms. Wiley thought that fresh meat, or canned meat after opening, spoiled quickly and developed ptomaines. He concluded that these ptomaines were responsible for much of the nausea and malaise.[26] He was also insistent that beef should only be a part of an over-all balanced diet. Wiley looked upon fruits as helpful but regarded starches such as potatoes and rice as essential for a balanced diet.[27]

Neither the Dodge nor the Wade investigations produced any con-

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File, 1886-1909, Adulteration, Bur. of Chem., RG 97, NA. On the other hand diarrhea and nausea were the usual complaints of soldiers claiming sickness from the beef—symptoms more closely associated with dysentery.

[24] "Report of the Commission," Ser 3859, 112; Wiley to Secretary of Agriculture, Jan. 25, 30, Feb. 6, 1899, Letterpress Book 73, Letters Sent 1878-1907, Bur. of Chem., RG 97, NA.

[25] Wiley to Secretary of Agriculture, Feb. 16, 1899. Letterpress Book 73, Letters Sent 1878-1907, Bur. of Chem , RG 97, NA.

[26] In 1872 the Italian chemist Francesco Selmi found highly poisonous alkaloids in decomposed meat which he called "ptomaines" after the Greek word *ptoma* for corpse. The ptomaine theory was widely accepted for many years and caused a controversy among physicians and scientists. More recent views hold that the putrefactive poisonous alkaloids called ptomaines are not present in the early stages of decomposition and are only formed after the food has decomposed to such an advanced state that it is repulsive Most non-fatal food poisoning cases are the result of salmonella and staphylococcus. Botulism is frequently fatal but rare in occurrence. Gail M. Dack, *Food Poisoning* 3rd ed. (Chicago: University of Chicago Press, 1956), pp 1-10; Elliot B. Dewberry, *Food Poisoning,* 4th ed. (London: L. Hill, 1959), pp 1-8.

[27] Wiley to Secretary of Agriculture, Apr 20, 1899, Letterpress Book 74, Letters Sent 1878-1907, Bur of Chem., RG 97, NA.

clusive evidence of chemically treated or unwholesome food as a direct cause of death or serious sickness. Miles' charges were based mainly on Daly's investigations and his evidence was inconclusive. It is reasonable to assume that there were cases of food poisoning from tainted meat but Miles' charge that chemically preserved beef caused most of the sickness in the camps was not supported. As Wiley told Representative James R. Mann of Illinois seven years later, it simply was not the practice of packers to use chemicals on army meat.[28]

The major cause of death and sickness was not spoiled food, or food preserved by chemicals, but disease, particularly typhoid fever. (The typhoid bacillus is usually introduced with food or drink.) Investigations of army camps conducted during the war by a group headed by Walter Reed clearly demonstrate that typhoid fever, the result of poor sanitation and improper handling of the sick, was the major scourge of the army. Reed, Victor C. Vaughan and Edward O. Shakespeare made inspections at Chickamauga, Jacksonville, Tampa, Fernandina, Alger and other camps. Sanitation deficiencies were more prevalent in the volunteer units. The following are some examples of what they found.

With the Fifty-Second Iowa Volunteer Infantry at Camp Thomas, thirty-six of the thirty-seven deaths were caused by typhoid fever. Their drinking water was obtained from a nearby spring but was not boiled, although regulations specified boiling before consumption. Fecal pits were not adequately maintained and much fecal matter was simply deposited on the ground. With the Sixty-Ninth New York Volunteer Infantry at Tampa the group found that water—again not boiled—was obtained from a local well. Since latrines dug in the soil quickly filled with water an area of ground was set aside where fecal matter was deposited. To make matters worse the unit physician Captain John Fuchsius noted that the men purchased lemonade from a local huckster who used water from a well which had outhouses within a radius of three feet. Miami was probably the worst camp but fortunately the army quickly abandoned it. Miami provided a sewer system to the camp which was nothing more than a wooden trough. Much fecal matter was hauled out in buckets or the men simply used nearby bushes. The camp at Jacksonville had a high typhoid rate although the city had no serious problem at that time. Local ordinances prohibited the digging of latrines so fecal matter was kept in wooden tubs and removed by local labor. General Fitzhugh Lee, the commander at Jacksonville and nephew of Robert E. Lee, recognized that the camp should have been tied into city sewers, but stated that this was "too expensive." [29]

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[28] Wiley to Hon. James R. Mann, May 3, 1906. Letterpress Book 184, Letters Sent 1878-1907, Bur. of Chem., RG 97, NA. Mann was a leader in the House during the passage of the Pure Food and Drugs Act of 1906. In his *Foods and their Adulteration* (Philadelphia: P. Blakiston, 1907), pp. 22-60, Wiley stated that chemical preservatives were common in preparations such as potted and deviled meats, and especially sausage, but were rarely used in fresh meat or canned fresh meat.

Indeed, Reed called camp pollution the greatest sin committed by the troops in 1898. Typhoid fever infected ninety percent of the regiments within eight weeks after arrival in the camps. Among 107,973 men there were 20,738 reported cases—about twenty percent. Of these cases 1,580 or 7.61 percent resulted in death. The rate of death from typhoid fever for the entire army was 14.63 per 1,000.[30]

All the ingredients for the transmission of typhoid were found in the army camps. Poor sanitation permitted fecal matter to contaminate drinking water. Moreover, as Reed noted, the practice of using healthy men on a rotating basis to care for the sick quickly exposed large numbers to the disease. The percentage of death among typhoid victims in the army, however, was comparable to other typhoid outbreaks during the same period. The 7.61 percent of the army in 1898 compares with 8.5 percent for an outbreak in Hamburg, Germany, in 1886, and 7.5 percent for an outbreak in Maidstone, England, in 1897. The city of Chicago had 2,372 deaths out of approximately 24,000 cases of typhoid in 1891-1892.[31]

In an address before the American Medical Association in 1899, U.S. Surgeon-General George M. Sternberg noted that in actuality there was an unusually small number of deaths among the sick and wounded during the Spanish-American War as compared with the Civil War. Sternberg reported that the disease rate for the first year of the Civil War was 1,971 per 100,000 while it was reduced to 1,237 per 100,000 in the Spanish-American War. Sternberg noted, however, that pure water and proper sanitation would have reduced the figure even lower.[32]

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[29] Walter Reed, Victor C. Vaughan, Edward O Shakespeare, "Report on the Origin and Spread of Typhoid Fever in the U.S. Military Camps during the Spanish War of 1898," 58 Cong., 2 sess., House Doc. No. 757, I, 240-257, 660-666; Major-General Fitzhugh Lee, *Annual Report Seventh Army Corp, Camp Cuba Libre, Jacksonville, Florida* (Jacksonville, 1898), p. 11.

[30] Walter Reed, "Report," I, 674-676. Reed thought the figure should be higher for he thought some physicians attributed death to malaria and other diseases when better diagnosis would have found typhoid as the causative agent. Typhoid fever was early recognized although the typhoid bacillus *salmonella typhosa* was not identified until 1880. The Widal diagnostic test was developed in 1896, therefore, adequate diagnostic tools were available during the war. The U.S. Army started using antityphoid vaccine in 1909, a year in which there were 173 cases and 16 deaths in an army of 57,124 By 1913 there were only two cases and no deaths Harvey Wain, *A History of Preventive Medicine* (Springfield, Ill.: Thomas, 1970), pp. 283-287.

[31] Walter Reed, "Report," I 674; Morris Fishbein, "Transmissible Diseases," *Illustrated Medical and Health Encyclopedia,* VII, 2030-2054

Although the Reed investigation offered the most adequate explanation of sickness during the war—he had presented his evidence before the Dodge commission—still the legend of embalmed beef persisted. In February, 1899, after the Dodge commission had issued its report, Secretary of War Alger was heckled in Boston with jeers of "yah, yah, yah! Beef! Beef!"[33] In 1903 a *New York Times* reporter, covering a meeting of the Association of Manufacturers and Distributors of Food Products, noted concern among food industry men over public distrust of prepared foods. He quoted one official as saying that "since the 'embalmed' beef scandal after the Spanish-American War every one has looked with doubt on any food which was known to contain preservatives."[34] Even though Wiley concluded that army beef had not been chemically treated *The North American* lauded him in these words in 1908 when there was talk of his dismissal: "Every blood-sucking poisoner in America will rejoice; every packer of 'embalmed beef' who got his dirty dollars from the deaths of soldier boys in 1898 will be jubilant."[35] In March, 1917, freshman Congressman Fiorello LaGuardia of New York introduced as his first bill a measure calling for imprisonment during peacetime and death in time of war for anyone selling inferior food, clothing, arms or ammunition to the army or navy. LaGuardia's father had been stationed in Tampa during the war and the family believed his death, which had occurred several years later, was caused by eating embalmed beef in Tampa in 1898.[36]

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[32] George M. Sternberg, "Sanitary Lessons of the War," *J.A.M.A.*, June 10, 1899: *32:* 1287-1294.

[33] *New York Tribune,* Feb. 18, 1899.

[34] *New York Times,* Nov. 1, 1903.

[35] *The North American* (Philadelphia), Dec. 30, 1908. At this time Roosevelt had lost confidence in Wiley during the controversy over the use of benzoate of soda and saccharin in food products See James Harvey Young, "The Science and Morals of Metabolism: Catsup and Benzoate of Soda," *Journal Hist. Med. & Allied Sci.,* 1968, *22:* 86-104. Although Wiley feuded with Roosevelt on the preservative question he did not turn against the Republican Party until 1912 when he resigned as bureau chief. See Oscar E Anderson, Jr., "The Pure Food Issue: A Republican Dilemma, 1906-1912," *Amer. Hist. Rev.,* Apr., 1956, *61:* 550-573.

[36] Howard Zinn, *LaGuardia in Congress* (Ithaca, 1959), pp. 3, 15.

Probably the reason why the charge of embalmed beef was accepted was because it was so believable. The Dodge commission uncovered many deficiences in the handling of the war. The charge was believable, moreover, because there was no adequate inspection program which would have controverted it. The Bureau of Animal Industry of the Department of Agriculture had conducted inspections of cattle, swine, and sheep to be sold in interstate commerce since 1891, but this inspection was limited to an examination of the animal shortly before and after death for evidence of disease or serious injury. The government had no jurisdiction beyond that point as the animal underwent the various stages of processing. Cases of spoiled canned beef reported at Jacksonville were stamped "inspected and passed"; this meant only that the animal was not diseased or seriously damaged at the time of slaughter.[37]

In addition the charge was believable because this was a period of a more general concern over the adulteration of foods. In December, 1897, Representative Marriott Brosius of Pennsylvania introduced a bill to prohibit the interstate commerce of adulterated foods, and drugs, and in March, 1898, the first National Pure Food and Drug congress met in Washington. Coming when it did the embalmed beef scandal directed public attention to the pure food movement in a most dramatic way, and, as O. E. Anderson observes, possibly stimulated the passage of state pure food laws.[38] Federal pure food legislation would not be passed until 1906, but the events of the late nineties had their place in the pure food movement and have been characterized by Anderson as the "beachhead" in the "battle for the law." [39]

In the long run its role in the pure food movement was probably the most significant aspect of the embalmed beef scandal. It had its immediate repercussions during the Dodge and Wade inquiries when Eagan and Miles bombasted each other and Alger stood in public scorn, but in the struggle to improve the nation's food the scandal was more noteworthy. Indeed, even before the passage of the Act of 1906 Wiley could write the director of the Laurel Institute in Cleveland that, because of the scandal, meat packers were "very particular now in regard to the character of preserved meats."[40]

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[37] "Report of the Commission," Ser. 3866, 406; "Food Furnished by the Subsistence Department," Ser. 3871, 1394; U.S D.A., *Report of the Secretary of Agriculture 1891* (Wash., D.C., 1892), pp. 17, 83-85; U.S.D.A , Bureau of Animal Industry, "Rules and Regulations Governing the Operations of the Bureau of Animal Industry," *Bulletin* No. 9 (1895), 42-43.

[38] Oscar E. Anderson, Jr., *The Health of a Nation: Harvey W. Wiley and the Fight for Pure Food* (Chicago: University of Chicago Press, 1958), p. 128.

[39] Anderson, *Health of a Nation,* pp. 120-147. Anderson notes that late in 1899 and early in 1900 Senator William E. Mason of Illinois conducted an investigation into the adulteration of food that continued to keep interest aroused after the beef issue quieted.

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[40] Wiley to Helen Louise Johnson, Nov. 26, 1901, Wiley Papers, box 7, MD, LC.

THE CESAREAN OPERATION IN ALTA CALIFORNIA DURING THE FRANCISCAN MISSION PERIOD (1769-1833)

ROSEMARY KEUPPER VALLE

That cesarean operations were performed by the missionaries during the Franciscan Mission Period in Alta California is frequently mentioned in the literature and Father Vicente Francisco de Sarria's 1830 treatise *Descripción de la Operación Cesaria* has been translated into English.[1] However, up to the present there has been no attempt to determine how often the operation was employed. Therefore, this study undertakes to furnish this information together with a brief review of the historical and religious background for the performance of the cesarean operation, especially as it relates to the Alta California missions, and to present a number of relevant and little known documents not previously available in English.

Hubert H. Bancroft,[2] George D. Lyman,[3] Henry Harris,[4] Patrick I. Nixon[5] and others state that only two cesarean operations were performed in Alta California between 1769 and 1833. Nevertheless, a study of the parochial books[6] of the nineteen, of twenty-one, missions for which these books are still in existence shows that fourteen of these operations were recorded, seven times as many as had previously been reported. One cesarean operation was performed at Missions San Gabriel Arcángel, Santa Cruz, Santa Clara de Asís and San Francisco de Asís, two were done at Mission San Carlos Borromeo, three at Mission San Antonio de Padua and five at Mission San Jose (Table 1).[7] In all instances the performance of the operation was noted in the Burial Register.

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[1] Sherburne F. Cook, "Sarria's treatise on the cesarean section, 1830," *California and Western Medicine,* 1937, *37,* part I, 107-109; part II, 187-189 and part III, 248-250.

[2] Hubert H. Bancroft, *California Pastoral* (San Francisco: The History Co., 1888), p. 277.

[3] George D. Layman, "The scalpel under three flags," *California Historical Quarterly,* 1925, *4:* 149.

[4] Henry Harris, *California's Medical Story* (San Francisco: J.W. Stacey, 1932), p. 32.

[5] Patrick I. Nixon, *The Medical Story of Early Texas, 1528-1853* (Lancaster, Pa.: Molly Bennett Lupe Memorial Fund, 1946), p. 52.

[6] These are uncatalogued mission records and consist of the Libro de Bautismos (Baptismal Register), Libro de Casimientos (Marriage Register), Libro de Confirmaciones (Confirmation Register), Libro de Mortuos or Libro de Difuntos (Burial Register) and Libro de Patentes. This last is not a book but rather a file of all the official correspondence and orders received by the missionaries. All the books for Mission San Luis Rey and the Death Register for Mission Nuestra Señora de la Soledad are missing. For the locations of the existing books see Jacob N. Bowman's "The parochial books of the California missions," *Southern California Historical Society Quarterly, 43:* 303-315 which is correct except for the following: books for Mission San Diego de Alcalá are now at the Chancery of the Diocese of San Diego, San Diego, Ca.; all books for Mission San Juan Bautista, volume II of the Baptismal Register for Mission San Luis Obispo, the Baptismal and Marriage Registers for Mission San Miguel, volume I of the Death Register for Mission San Antonio de Padua and all those listed as being at Fresno are presently at the Chancery of the Diocese of Monterey, Monterey, Ca. All the Books of Patentes are chronological. All the Registers are chronological and consecutively numbered except those of Mission

These operations were not cesarean sections in the modern sense but involved only the extraction of the fetus after the death of the mother. All were on Christian Indians except one, at Mission San Jose, which was on a Spanish woman. There are no detailed descriptions of the operations but it can be assumed that the technique given by Father Josef Manual Rodriguez[8] was used for those performed prior to 1804. After this date it is probable that the procedure in the instructions accompanying the Pragmatic of King Charles IV, which is presented in translation later in this paper, was followed. The two techniques are similar and both sets of instructions make it clear that it is preferable that the operation be carried out by a surgeon, a physician or a midwife, and by a "non-facultivo" (non-professional) only when none of these was available. In Alta California the performance of the operation became the duty of the missionary because during the mission period there was never more than one physician in the province at any one time and for five years, 1824-1829, there was none.[9]

Very little information is given about the infants. The longest survival was two days and eight were found to be doubtfully alive on operation. They were baptized positively if alive and conditionally if there

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San Buenaventura (see Bowman). As listed in Bowman, the mission books for Mission San Gabriel Arcángel are in the rectory of the mission but since these are restricted the microfilm record (Mss. 528·18, Reels 1-4) at Huntington Library, San Marino, Ca. was used for this study.

[7] See Death Registers for Mission San Gabriel, vol. 1, Burial Number 2166; Mission Santa Cruz, vol. 1, Burial Number 1668; Mission Santa Clara de Asís, vol. 1, Burial Number 2217; Mission San Francisco de Asís, vol. 1, Burial Number 1961; Mission San Carlos Borromeo, vol. 1, Burial Numbers 1368 and 2414; Mission San Antonio de Padua, vol. 1, Burial Numbers 1418, 1459, and 1920; Mission San Jose, vol. 1, Burial Numbers, 623, 990, 3472, 4205 and 4660. For the locations of these books see n. 6 above.

[8] Josef Manuel Rodriguez, *La Caridad del Sacerdote para con los Niños Encerrados en el Vientre de sus Madres Difuntas y Documentos de la Utilidad y Necesidad de su Practica* (México, F. Zuñiga y Ontiveros, 1772), pp. 37-44. This book deals only with the performance of the cesarean operation on dead pregnant women.

[9] See Bancroft (n. 2), pp. 622-623; Lyman (n. 3), p. 151; Harris (n. 4), p. 40.

TABLE 1

Cesarean Operations Performed in the Alta California Missions between 1799 and 1833

Mission	Burial Number	Date of Operation	Result	Performed by
Santa Clara de Asís	2217	1-26-1799	8 mo. fetus	Fr. Josef Viñals and Fr. José Viader
San Carlos Borromeo	1368	10-25-1801	alive ?	Fr. Josef Viñals
	2414	9-25-1822	alive ?	Fr. Vincente Sarria
San Antonio de Padua	1418	2-19-1802	8 mo. fetus lived 1 day	Fr. Florencio Ibañez
	1459	11-17-1802	7 mo. fetus lived ½ hr.	Fr. Florencio Ibañez
	1920	7-23-1819	7 mo. fetus alive ?	Fr. Juan B. Sancho
San Gabriel Arcangel	2166	3-20-1803	8 mo. fetus alive ?	Fr. Isadore Barcenilla
San Jose	623	8-27-1805	lived for a few minutes	Fr. Francisco Uria
	990	3-24-1808	lived for a few hours	Fr. Buenaventura Fortuny
	3472	12-21-1825	alive ?	Narcisco and Silvestre *
	4205	3-28-1829	alive ?	Narciso *
	4660	1-29-1832	alive ?	Fr. Narciso Duran
San Francisco de Asís	1961	11-12-1802	alive ?	Fr. Ramón Abella
Santa Cruz	1668	11-15-1826	twins, 1 male, 1 female, lived a short time	Fr. Luis Gil y Taboada

* Fr. Duran states that he supervised the operations performed by the Indians Silvestre and Narciso.

was doubt about their being alive. In most cases the sex and age are not given. The infant was referred to as "la criatura" and after baptism was usually buried with the mother.

The first cesarean operation in Alta California was performed at Mission Santa Clara de Asís by Fathers Josef Viñals and José Viader on January 26, 1799. A fallacy which persists in the literature is that this operation was performed on January 26, 1779. Dr. Nicolas Leon so states in his "Obstetrica en México." [10] He further claims that this was the first such operation performed in New Spain and Aristides A. Moll,[11] Edgar A. Hume [12] and José Alvarez Amezquita et al.[13] follow him in this attribution. As authority for this statement he quotes an article which he says appeared in Volume IX of the *Gazeta de México* and gives the date of the operation as January 26, 1779. The operation is mentioned in Volume IX of the *Gazeta* but the date stated is January 26, 1799.[14] A study of the Burial Register of this mission confirms that the operation was performed on the later date and there is no record of any cesarean section having been carried out previous to this time.

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[10] Nicolas Leon, *Obstetrica en México* (México: Vda. F. Diaz de Leon, 1910), p. 207.

Discussion

The idea of the cesarean operation is an ancient one. J. Paul Pundel in his *Histoire de l'Opération Cesarienne* states that the mythologies of many peoples contain tales of the deliveries of infants by this means.[15] The Lex Regia of Roman times required that the operation be performed post-mortem on all pregnant women in the hope of obtaining a viable infant and thus another citizen for the state.[16] The chief reason for the Roman Catholic Church's advocacy of the performance of the operation is to obtain the fetus for baptism and thereby secure its eternal salvation.

In the very early church, infant baptism was neither universal nor compulsory.[17] However, baptism at as early a date as possible became necessary after the fifth century when the church adopted the Augustinian doctrine that original sin is present from the moment of conception.[18]

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[11] Aristides A. Moll, "La investigación medica en la America Latina," *Boletin de la Union Panamericana*, 1933, *60:* 389.

[12] Edgar E. Hume, "Spanish colonial medicine," *Bulletin of the History of Medicine*, 1934, *2:* 221.

[13] J. Alvarez Amezquita, M. E. Bustamante, A. Lopez Pecazos and F. Fernandez del Castillo, *Historia de la Salubridad y de la Asistencia en México* (México: Secretaria de Salubridad y Asistencía, 1960), I, p. 157.

[14] *Gazeta de México*, May 29, 1799, *9:* 329.

[15] J. Paul Pundel, *Histoire de l'Opèration Cesarienne* (Brussels: Presses Academiques Europeenes, 1969), p. 21.

[16] *Ibid.*, p. 99. The Roman Lex Regis is attributed to Numa Pompilius (ca. 700 B.C.).

[17] *Encyclopedia of Religion and Ethics*, edited by James Hastings (New York: Charles Scribners' Sons, 1928), p. 393.

[18] St. Augustin expounds his doctrine of original sin in Book I of "De nuptis et concupiscentia," *Patrologiae Latinae* (Paris: J-P. Migne, 1863) 10, pp. 412-453. He states in "De gratia Christi et de peccato originali," Book II, Chap. 41, *Pat. Lat.*, 10, p. 409, that his inspiration for the doctrine of original sin was a statement by St. Ambrose in his "De poenitentia," Book I, Chap. 3, *Pat. Lat.*, 16, p. 490 who quotes David, Psalm 50.5

St. Augustin (354-430) also states that he sees no reason why embryos cannot share in the resurrection.[19] A result of the acceptance of these two concepts was a church concern for the fate of the soul of the unborn infant in the womb of the dead pregnant woman.

It is possible that cesarean operations were performed from the fifth century onward. However, the operation is not given much attention in the church literature until the thirteenth century when it begins to appear in the reports of various synodal councils. For example, it is mentioned in the reports of the Councils of Canterbury (Canon 14) in 1236,[20] of Cologne (Canon 4) in 1280 [21] and of Treves (Canon 114) in 1310.[22]

St. Thomas Aquinus (1227-1274) states in his "*Summa Theologica*" that if the mother dies with the child still in her womb, she is to be opened and the child removed and baptized if still alive.[23] St. Charles Borromeo (1538-1584) makes a similar statement in his *Acta Ecclesiastica Mediolanensis* which was first published in 1599.[24]

Pope Paul V (1550-1621) who standardized the ritual of the Western Church in 1614, introduced the following statement about the performance of the cesarean operation into the Rituale Romanum: "Si mater pregnans mortua fuerit, foetus quam primum caute extrahatur, ac, si vivus fuerit, baptizetur." [25]

There was a general resurgence of interest in the performance of this operation in the mid-eighteenth century. It was probably occasioned by the publication of Msgr. Francesco Emanuello Cangiamila's *Embrilogia Sacra, ovvero del "Uffizio de' Sacerdoti, Medici, e Superiori, circa l'Eterna Salute de' Bambini Racchiusi nell'' utero* in four books in Palermo in 1745.[26] This book which was translated into Latin,[27] French [28]

"Ecce enim in iniquitatibus conceptus sum, et in delictis peperit me mater mea." This is psalm 51.5 in the standard revised version of the English Bible.

[19] St. Augustin, *De Civitate Dei* (Basel: Ambrosium et Aurelian Frobenius, 1570), Book XXII, Chap. 13, Col. 1361.

[20] Charles Jean Hefele, *Histoire des Conciles,* translated by Dom T. Leclercq (Paris: Letouzey et Ané, 1907-1938), 5, p. 1575.

[21] *Ibid.*, 6, p. 260.

[22] *Ibid.*, p. 620.

[23] St. Thomas Aquinus, *Summa Theologica,* translated by the Fathers of the English Dominican Province (New York: Benziger Bros., 1974), part 4, Q. 68, art. 11, p. 2045.

[24] St. Charles Borromeo, *Acta Ecclesiastica Mediolanensis* (Milan: P. Pagnonio, 1863), 1, p. 562.

[25] Pope Paul V, *Rituale Romanum,* edited by Pope Benedict XIV (Rome, 1874), p. 5.

[26] Francesco Emanuello Cangiamila, *Embriologia Sacra, ovvero dell' Uffizio de' Sacerdoti,*

and Spanish[29] deals with the performance of the cesarean operation on both living and dead pregnant women. Msgr. Cangiamila and Abbé Dinouart both state that the reason for writing the book was that surgeons and midwives were neglecting to perform the operation because it so seldom resulted in a viable infant.[30] While both authors quote a number of instances in which the child and the mother, or one or the other, survived the procedure, they point out that the main purpose of the operation is to secure the fetus for baptism.

This book so impressed King Charles of Sicily, later King Charles III of Spain, that he issued a pragmatic in 1749 which made the performance of the operation on dead pregnant women a legal obligation for all parties concerned and also prescribed the penalties for failure to perform it. The pragmatic deals with both the performance of the cesarean operation and the procurement of abortions in such detail that only the more pertinent portions of it's fourteen sections are presented in translation.

Charles, by the Grace of God, King of the Two Sicilies, of Jerusalem, Infante of Spain, Duke of Parma. of Plasencia, etc.
To the Viceroy and Captain General of this Realm, to those Venerable Christian Fathers, Archbishops, Abbots, Priors, Parish Priests and their vicars and assistants; to the Presiding Judges of the Royal Courts, to Princes, Dukes . . . Greetings.

It is without a doubt despicable that, in many cities and places in this Realm, pregnant women have died without any effort having been made to remove the fetuses from their wombs . . . by which means it is possible to save the fetus from certain death, or at least baptize it so that it is able to enter the Kingdom of Heaven. But it has happened more than once, that the mother has been buried with the infant in her womb without anyone having .had the least compassion for this helpless creature. Moved by this situation and encouraged by many concerned parties, we have appointed

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Medici, e Superiori, circa l'Eterna Salute de' Bambini Racchiusi nell' Utero (Milan: G. Cairoli, 1760). Msgr. Cangiamila was the Inquisitor General of Sicily at the time he wrote this book.

[27] Francesco Emanuello Cangiamila (trans.), *Sacra Embryologia sive de Officio Sacerdotum, Medicorum, et Aliorum circa Aeternam Parvulorum in Utero Existenium Salutem* by F. E. Cangiamila (Ghent: R.F. Walwein, 1775).

[28] L'Abbé Dinouart (trans.), *Abregé de l'Embryologie Sacrée ou Devoirs des Prêtres, des Médecins, des Chirurgiens et de Sages-Femmes envers les Enfants qui sont dans le Sein de leurs Mères* by F. E. Cangiamila (Paris: Nyon, 1762). Abbé Dinouart states in the introduction to this work that his translation is from the Latin.

[29] Joaquin Castellot (trans.), *Embriologia Sagrada ó Tratado de la Obligación que tienen los Curas, Confesores, Medicos, Comadres, y otras Personas, de cooperar a la Salvación de los Niños que no han nacido, de los que nacen al parecer muertos, de los Abortivos, de los Monstruos, etc.,* by F. E. Cangiamila (Madrid: Panteleon, 1785) Castellot made his Spanish translation from Abbé Dinouart's French version.

[30] Cangiamila (n. 26), p. 56; Dinouart (n. 27), p 103.

a commission to examine the matter; and this commission has suggested that we should issue a pragmatic with the force of law and prescribe what should be done in such matters. . . .

1. Whenever a woman, of whatever class or condition, expires who the husband, the parents or servants think might be pregnant, each and everyone of them is obligated, after ascertaining that she is truly dead, to see that a cesarean operation is performed to remove the fetus from the womb and that it is baptized. To this end they are to notify in advance the surgeon or other trained person so that they can be prepared to perform the operation and if, for any reason, it cannot be performed immediately they should keep the abdomen of the dead woman warm by applying hot packs. . . .

XIV. Finally, we order that anyone who violates this Pragmatic which we wish to be perpetually valid, whether he be husband, a relative of the pregnant woman or any other person, who by fraud or negligence, or for any reason, interferes with the cesarean operation or endangers the fetus, or who maliciouały promotes an abortion, be reported as a homicide and be treated as a criminal. . . .

And so that all will have knowledge of this, we order that it be published and posted according to the law. . . .

Issued in Palermo, 9 August 1749 [31]

The obligation of performing a post-mortem cesarean operation on pregnant women was introduced into New Spain by Don Frey Antonio María de Bucareli y Ursua. Shortly after arriving in Mexico to assume his duties as Viceroy, he issued the following order:

Considering the importance (as was pointed out to me by the Minister of his Majesty's Court) of imposing the practice of carrying out the cesarean operation in all areas of this Viceroyalty as advocated by the Reverend Father Josef Manuel Rodriguez of the Franciscan Order in his recently published work *La Caridad del Sacerdote para con los Niños Encerrados en el Vientre de sus Madres Difuntas, y Documentos de la Utilidad y Necesidad de su Practica:* I advise you that whenever official assistance is requested and deemed necessary in carrying out this operation, it will be furnished promptly under penalty of 500 pesos; if necessary the surgeons should be compelled to perform it; also the parents, spouses or relatives of the dead woman, if they oppose or refuse to permit the operation, should be reported to the authorities so that proper punishment can be meted out.

God Bless you for many years Mexico, 21 November 1772 [32]

Viceroy Bucareli's order was implemented by an edict sent out on December 4, 1772 by Don Alonso Nuñez de Haro y Peralta, the Archbishop of Mexico.

We, Alonso Nuñez de Haro y Peralta, by the Grace of God and the Holy See, Archbishop of the Holy Metropolitan Church of Mexico, of the Council of Your Majesty, etc.

Among the grave obligations of our pastoral ministry, we consider the eternal salva-

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[31] Castellot (n. 29), pp. 277-285.

[32] Bandos de Bucareli 1771-1774, M 381, Biblioteca Nacional, Mexico.

tion of the souls of our subjects to be of primary importance. . . . Having before me the Pragmatic published in 1749 by our King Charles in his Kingdom of Sicily . . . we send a formal order to all priests and vicars in our Archdiocese, that always when they find a pregnant woman in danger of death, they send for a surgeon, and after determining that the woman is truly dead, have the cesarean operation performed and baptize the infant. And because we have been informed that in many cities of our Archdiocese there are no surgeons or other persons capable of performing the cesarean operation, it is our wish that the priests have in their homes and study a small book recently published by Father Josef Manuel Rodriguez, OFM, which describes an easy method of performing the operation, so that they may carry it out themselves when there is no lay person capable of doing it. . . . We exhort all secular priests and monks to do all in their power to lessen the horror associated with this operation so necessary for the salvation of infants who are so unfortunate as to lose their mothers before being born. As evidence of our interest in this subject and of the religious importance of it, we offer an indulgence of eighty days to persons who perform the operation or notify the priests or authorities of any pregnant woman in danger of death with the purpose of saving the offspring by means of the operation and it's subsequent baptism. And so that our edict will come to the attention of everyone, we order that it be posted in the proper place in all the parishes of our Archdiocese.
Given under our Seal in the City of Mexico 4 December 1772 [33]

Evidently the missionaries in Alta California were familiar with these instructions since five of the fourteen cesarean operations were performed before the second pragmatic was issued by King Charles IV in 1804. For example Father Jose Viader of Mission Santa Clara de Asís copied Father Josef Manuel Rodriguez' instructions for performing the operation in his scrapbook and also mentions that Archbishop Nuñez de Haro y Peralta had decreed an indulgence of eighty days for all those who assisted in its performance.[34]

The pragmatic of Charles IV is not as lengthy or as detailed as that of Charles III and is as follows:

Charles IV, by the Grace of God, King of Spain and the Indies.

It has been brought to my attention . . . that many spiritual and secular evils have resulted from not following the instructions for performing the cesarean operation. . . . I have discussed this subject with my Council of the Indies and my Secretary of State and on December 25, 1802, ordered the College of San Carlos to prepare instructions, which approved by the court surgeon, are to accompany my Royal Decree; and it is my will that the following instructions be obeyed:
First, that in towns where there are physicians, he who attends any pregnant woman is to advise the parish priest as soon as she dies; and if he happens to be a surgeon, and even if he is not surgeon, he should proceed immediately to perform a cesarean

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[33] Arquidiocesis, M 384, Biblioteca Nacional, Mexico.

[34] Father José Viader's scrapbook is in the Mission Santa Clara Archives, Santa Clara University Library, Santa Clara, Ca. This book has an index but no pagination.

operation, after ascertaining according to the said instructions that the woman is truly dead.
Second, that the priest, as well as the surgeon should, without fail come immediately to the house of the deceased at whatever hour of the day or night they are called.
Third, that in towns where there is no physician, the parish priest in agreement with the authorities should select the person best fitted to perform the operation in exact accordance with the instructions which the priest should have at hand.
Fourth, and last, that for this purpose the priest and the authorities should have in their possession the order regarding the subject which was issued to them by the church and government. The former should not permit the burial of any pregnant woman, regardless of class, without first verifying that the operation has been performed; and the latter must report to their superiors that all has been done according to the instructions and also any omissions they observe in a matter so important to humanity.
Therefore, I order the Viceroys, the Courts and Governors, and I pray and charge the Archbishops and Bishops in my Dominions of the Indies and the Philippines to forward the accompanying instructions to the district authorities and the diocesan priests and to impress upon them that this is my will and sovereign command.
Given in Aranjuez, 13 April 1804.[35]

On July 19, 1804, Viceroy Josef de Iturrigaray issued the following order from Mexico City:

Attend and carry out that which is ordered in this Royal Decree. It is to be entered in the official government records. Have it printed along with the instructions which accompany it and distribute as directed.
Mexico 19 July 1804.[36]

Father Estevan Tapis was president of the missions at this time and his copy of the instructions which accompanied King Charles IV's Pragmatic is found in the Book of Patentes of Mission Santa Barbara. It evidently was forwarded to him from Culiacan, Mexico by the Bishop of Sonora who had jurisdiction over the missions in such matters.

Method of Performing a Cesarean Operation after the Death of the Mother.

It is not easy for one who lacks knowledge of anatomy to carry out a cesarean operation after the death of the mother because he needs the same instruction as that required for carrying it out on a living woman. . . . However, since the spiritual and temporal welfare of the infant is of major concern, and since trained surgeons cannot be found for all the cases which need the operation, other help which does not have formal training is justified, and as exact and intelligible instructions as possible are presented for performing the operation.
1. Before opening the abdomen, determine whether or not the mother is living or dead for which purpose spirits of ammonia should be applied to the mouth, the nostrils

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[35] Reales Cedulas, vol. 192, exp. 35, Archivo General de la Nación, Mexico.
[36] Reales Cedulas, vol. 192, exp. 168, Archivo General de la Nación, Mexico.

and eyes, a pin should be introduced beneath the fingernail of any finger, or any other known stimulants used; but if, after any of these diligently applied measures, no signs of life are evidenced, proceed immediately with the operation.

2 If the infant presents itself in the normal manner, it should be delivered as usual.

3. Although the operation should be performed as soon as possible, it should be carried out even after many hours have passed.

4. Also it should be done in early pregnancies and the infant should be baptized in such a manner that the water touches it directly. The operation in this form requires more knowledge than is possessed by one who is not a surgeon, therefore we will not explain the various circumstances which must be dealt with, such as emptying the bladder, opening the abdomen between the pyramidal muscles, opening the fetal membranes with care, etc. However, supposing that the pregnancy is more advanced and that the uterus is located in mid-abdomen and extends up to the border of the chest, which is more usual, the operation should be carried out as follows:

Operation

The only instruments needed are a sharp scalpel for the incision and another with a blunt tip, or if these are lacking, a razor or even a penknife will suffice. Place the cadaver on the bed, or on whatever is available, slightly on the side without exposing more than is necessary; compress the abdomen slightly and make an incision a little more than six inches long, equal to about one-third of a Castillian vara; this incision should include the skin, the muscles and the peritoneum which are closely adherent and difficult to separate. These are retracted and the abdomen is opened. To do this without injuring the viscera, such as the intestines, stomach, etc., a small incision is made first and two fingers are introduced through it to guide the blunt tipped scalpel or razor until the incision is of the proper length. The incision is made in the line where the abdomen is most extended, or where fetus is nearest. It should be two finger-breadths below the lowest ribs and four finger-breadths above the umbilicus, so that the cut is directed from the front toward the spine. The incision in the abdomen completed, a similar incision is made in the uterus which is a large meaty sac in which the fetus is contained; then with equal care open the fetal membranes which are like a sort of tissue or sac and which envelop the fetus; remove the blood with sponges or cloths and proceed as follows: if the fetus shows no signs of life, do not remove it before baptizing it conditionally; if it lives and seems robust, remove it by grasping it by the feet, or in whatever manner is easiest, and baptize it, sprinkling the redeeming water on the head. After removal, tie the cord one or two finger-breadths from the umbilicus and sever it another two finger-breadths from the ligature; then the placenta is removed by pulling gently on the cord, or if it is adherent to the uterus, it is separated gently with the fingers, or preferably with the edge of the palm. It is better not to suture the abdomen of the mother, but only to approximate the edges of the wound and fasten a towel around the body from the back to the front.

Furthermore

When a hernia or rupture occurs, the uterus and the placenta and membranes are in the site of the incision, etc. These and other abnormalities are omitted because they require the attention of a competent surgeon to be handled with success. We are obliged to state that all small incisions will be a handicap to persons who lack knowledge of the procedure and will lead to poor results.

On the commission of the Council of the Royal College of San Carlos, we have proclaimed the foregoing method.

Madrid 25 October 1803

/S/ Dr. José Ribes
/S/ Dr. Manuel Bonafos

This is certified to be a true copy of the original
Madrid 12 November 1803 /S/ Dr. Sebastian Aso y Travieso

In conformance with the order of the King directed to this Diocese by the Supreme Council of the Indies, His Reverence, the Bishop in the City of Culiacan endorses this order.

14 September 1804 /S/ José Dario Rouset[37]

Father Vicente Francisco de Sarria's treatise on the cesarean operation was circulated through the missions in 1830, but only one operation was performed after this date. The treatise is not presented because it is readily available in Dr. Sherburne F. Cook's English translation.[38] Father Sarria states in the letter which accompanied the *Descripción de la Operación Cesaria* that he has never seen any circular or official correspondence pertaining to the ministry which treats of or makes the slightest mention of the cesarean operation.[39] This is a strange and mystifying statement in view of the existence of the earlier regulations and instructions regarding the performance of the operation and the fact that he had done one in 1822.[40]

It is of interest that in spite of the strong insistence by both the government and the church on the performance of the cesarean operation that only fourteen were done in the sixty-one years between 1772 and 1833. In the absence of any recorded explanation for the rarity of its use, it can only be speculated that perhaps this was due to a reluctance to perform the operation on the part of the missionaries, or an antipathy towards it among the Indians, or a combination of the two factors.

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[37] Libro de Patentes, Mission Santa Barbara Archives, Santa Barbara, Ca. There is another copy in Father Viader's scrapbook (n. 34).

[38] Cook (n. 1), part I, pp. 108.

[39] Carta Circular, C-C-26, Bancroft Library, University of California, Berkeley. This letter is in the folder with a copy of Father Sarria's *Descripción de la Operación Cesaria.*

[40] See Table 1.

THE INTRIGUING NUCLEUS OF DEITERS

Notes on an Eponym

FRANCIS SCHILLER

Ill understood, and almost entirely left to the mercy of nature's healing power, vertigo is the disturbance of a function that in health hardly enters our consciousness. An equilibratory or stato-kinetic "sense" is an extrapolation derived from a disorder, since equilibrium rests upon the unconscious reflex mechanism that Aristotle understandably omitted from his count of five senses. There remains acceleration and deceleration, both linear and angular, but these had not become a general concern before the steam engine and its even faster progeny got into their stride.

The name of Deiters in this context is obvious to any student of any major anatomy book. The lateral vestibular, or Deiters' nucleus is the best known portion of the relay station that connects the otic labyrinth and its nerve to the central nervous system. The nucleus is an essential link in the automatic muscle responses when the body—or head—moves, or is being moved as a whole, in health or disease. Although the authors of modern textbooks cannot, perhaps, be expected to refer to Deiters' work, someone by that name implicitly must have provided the first or the best early description. Yet one would be hard put to give an unqualified answer to the person who cared to question this assumption. It is easy enough to consult any book on anatomical eponyms and learn the title and year of Deiters' pertinent publication (1865).[1] But this is only where the problem begins.

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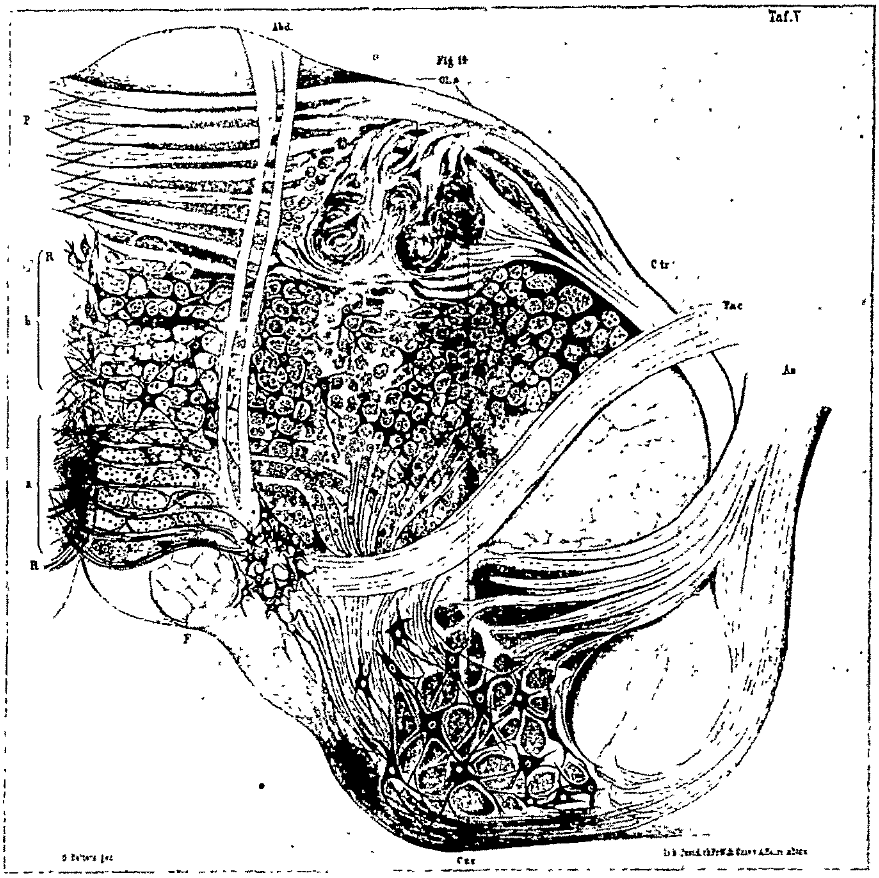
[1] Otto Deiters, *Untersuchungen über Gehirn und Rückenmark des Menschen und der Säugethiere,* ed. Max Schultze (Braunschweig: Wieweg, 1865). Ludwig Edinger, *Vorlesungen über den Bau der nervösen Centralorgane des Menschen und der Thiere,* 5th ed. (Leipzig: Vogel, 1896), p. 363, says that the significance of the nucleus is still unknown and its connection with the auditory nerve yet uncertain: "Hence it is better to give it the name of its discoverer and call it Deiters' nucleus, after Deiters whose work on the oblongata merits our highest esteem." (A statement not quite accurate in its first part, as we shall see.) In another passage (p. 89) Edinger, the most widely read neuroanatomist of his day, is "led to the conclusion that this part of the oblongata is an important center for the entire statics [*Statik*] of the body." Four years later (*ibid.,* 6th ed., 1900, p. 99), Edinger is more explicit if still tentative: "Deiters' nucleus (nucleus tractus acustico-spinalis) is perhaps a part of



"Transverse section through one half of the medulla oblongata close to its transition into the pons, with the fiber bundles of abducent, facial, and acoustic nerves. *P*—pyramidal fibers; *RR*—raphe, with formation reticularis to the right; *Cr c*—crura cerebelli ad medullam oblongatam [restiform body] with the numerous large ganglion cells that *seemingly* belong to the acoustic nerve. . . ." [My italics!]
From Otto Deiters, *Untersuchungen über Gehirn und Rückenmark des Menschen und der Säugethiere,* Braunschweig: Wiewek, 1865. pl. V, fig. 14

the mechanism required to transmit the impressions from the labyrinth to the musculature of the body." Not only from the labyrinth, for on p. 340 he again refers to the nucleus stressing its "magnitude and importance as it connects part of the vermis and cerebellar nuclei to the centers for the static and other nerves subserving the position of the eyes . . . the posture of the body, and the coordination of its static functions."

To start with, Deiters died two years before his book was published and he left it not only uncorrected but unfinished. More important is the fact that he displayed no notion that a vestibular nucleus or nerve exists. This is no adverse criticism of a very remarkable young man who met with a tragic end. Only in the decade after Deiters' premature death a multifocal interest in vertigo sprang up, in a mutually independent flock of physicists, psychologists, and physicians. It was only in the seventies and eighties of the last century that speed began to affect people's lives.

By then, as might be expected, some illustrious forerunners were almost forgotton. At the beginning of the nineteenth century when romanticism and science entered such a spectacular union, the phenomenon of vertigo had fascinated both Erasmus Darwin and Jan Purkyně. Purkyně in particular devoted five papers of self-observation to the subject, but he surmised that the rotating brain itself might generate the catastrophic sensation. Not even Purkyně suspected a part of the inner ear to function as an organ of balance.[2]

In regard to the structure of this organ, the early anatomists had done their spade work. Galen already had spoken of a "labyrinth."[3] In the sixteenth century Falloppio,[4] and perhaps Eustacchio,[5] found it consisting of three tiny canals; in the seventeenth, Duverney gave those canals and the vestibulum their accepted name;[6] another century later Cotugno discovered the fluid inside them;[7] and to Scarpa we owe the ganglion of the vestibular nerve.[8] But the non-auditory function of the semicircular canals remained undisclosed until by 1828 Flourens had differentially destroyed each one of them in his pigeons.[9] In so doing he was depriving the birds not, indeed, of their hearing but of their ability to maintain an adequate posture. Flourens' separation of "a nerve of the semicircular canals" from "the true acoustic" happened in Purkyně's day. Johannes Müller was sufficiently impressed to include, in the fourth edition of his famous textbook of physiology and with his concept of specific nerve energies, those "ampullar nerves" capable of responding with vertigo to every stimulus.[10] The stage was well set but the action slow. Menière's paper of 1861 stressed the triad of vertigo, tinnitus and deafness, resulting from destructive disease of the inner ear rather than from cerebral apoplexy.[11] His discourse passed as a monologue before a scanty and skeptical audience. While Menière did remember Flourens he was himself hardly remembered until the early part of this century, when a later generation of otologists took up the matter—still not giving their late confrere a full understanding.[12]

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[2] Edwin G. Boring, *Sensation and Perception in the History of Experimental Psychology* (New York: Appleton-Century-Crofts, 1942), pp. 535-544; 568-570 (bibliography).

[3] Adam Politzer, *Geschichte der Ohrenheilkunde* (vol. 1, 1907; vol. 2, 1913. Hildesheim: Olms, 1967), vol. 1, p. 27 (ref. 8, p. 30).

[4] Gabriele Falloppio, *Observationes anatomicae* (Venice, Ulm, 1562), pp. 29-30. "The second cavity . may be called the labyrinth. . From here issue three underground passages [*cuniculi*—Falloppio uses this mining term rather than *canalis,* the waterpipe or channel] and they return to it; from this fact the cavity gets its name. One of them is inferior . . the other passage originates in an angle of the cavity that is anterior to the former, rises upwards . . . [etc.]" Falloppio does distinguish this second cavity from the middle ear which is his first, and the cochlea which is the third. (He also speaks of the "membrana mollissima ac tenuissima" lining the cavities." But nowhere does he use the word *vestibulum.* This is contrary to Joseph Hyrtl, *Onomatologia antomica* (1880) (Hildesheim-New York: Olms, 1970), p. 616, where Falloppio is credited with introducing the term. See n. 6 below.

[5] Bartholomeo Eustachio, *Tabulae anatomicae,* ed. Jo. Maria Lancisi (Rome: Gonzago, 1714), pl. 44 & 45. Eustachio died in 1574, leaving his plates without captions or comments. *Idem, Epistola de auditus organis,* in *Opuscula anatomica* (Leyden: van der Linden, 1907) pp. 125-142, also has no reference to the semicircular canals.

[6] Joseph Guichard Du Verney, *Traité de l'organe de l'ouïe, etc.* (Paris: Michallet, 1683), pp. ij-iij; 31-34; 98-105. On p. 37 he writes: "Les deux autres branches de la portion molle sont destinées pour le vestibule . . ." He goes into full detail about their distribution to the individual semicircular canals which he thoroughly describes. "Portion molle" of course is

The neuroanatomical side of the problem that concerns us here was vigorously and acrimoniously pursued in the 1880s.[13] In the microscopic battle over what these investigators were calling the "origin" of the eighth nerve, Deiters' nucleus was significantly fought over. The eminent con-

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the old term for the acoustic portion of that twin nerve whose *portio dura* is the facial nerve. Du Verney seems to be the first author to use the term *vestibule,* naturally without attaching to it any function other than the auditory. In gross-anatomical terms a vestibular *nerve* does not exist. There are vestibular nerve *roots* discernible at the peripheral vestibular end and at the central medullary end. They issue from the bipolar neurons of Scarpa's ganglion and, more or less separated from the cochlear fibers, they run inside the eighth nerve—which we still call the "acoustic." (A. T. Rasmussen, "Studies of eighth cranial nerve in man," *Laryngoscope,* 1940, *50*: 67-83.)

[7] Domenico Cotugno, *De aquaeductibus auris humanae dissertatio* (Naples: St. Thomas Aquinae Typogr., 1760).

[8] Antonio Scarpa, *Anatomicae disquisitiones de auditu et olfactu,* in *Opere* (Florence: Batelli, 1795), vol. 5, p. 417 ff.

[9] Marie Jean Pierre Flourens, "Expériences sur les canaux sémicirculaires de l'oreille dans les oiseaux" (1828), *Mém. Acad. Sci. Paris,* 1830, *9*: 455-466.

[10] Politzer (n. 3 above), vol. 1, p. 407.

[11] Prosper Menière, "Maladies de l'oreille interne offrant les symptômes de la congestion cérébrale apoplectiforme," *Gaz. méd. Paris,* sér. 3, 1861, *16:* 88; 239; 379-381; 597-601.

[12] Henry L. Williams, *Menière's Disease* (Springfield, Ill.: Thomas, 1952), pp. 3-17.

[13] Otto Marberg, "Die Geschichte der Lehre vom Nervus Octavus," in Politzer (n. 3 above), vol. 2, pp. 36-42.

testants were Flechsig[14] with his Russian assistant Bekhterev[15] in Leipzig, to cross arms with Forel[16] and his Russian assistant Onufrowitz, in Zürich. They found support in another Russian (naturalized)—von Monakow[17]—also in Zürich. A second front for the Leipzig team had Baginsky[18] (in Berlin) as its target; Sigmund Freud (Baginsky's friend) contributed from Vienna in 1886.[19] When the dust had settled, posterity proved Bekhterev victorious. Bekhterev's own small celled *superior* vestibular nucleus today bears his name, next to Deiters' large celled *lateral* vestibular nucleus. Schwalbe's[20] *medial* and Roller's *inferior* nucleus[21] soon completed the quartet.[22] It was also Bekhterev who identified the anterior root of the eighth nerve as *ramus vestibularis,* the posterior as *ramus cochlearis.* He so fulfilled Flourens' postulate of the eighth nerve's dual components, in conformity to its dual function. Finally Bekhterev insisted on the intimate relationship between Deiters' nucleus of 1865 and the vestibular portion of the eighth nerve—the portion he, Bekhterev, had discovered around 1887.[23] Here he was in total disagreement with the Zürich neurologists. Forel maintained, in full agreement with Monakov, that the "so-called nucleus of Deiters has nothing to do with the acoustic nerve."[24] Had Monakov not just shown that on the contrary those large cells Deiters had described must be related to the spinal cord? Monakov had transected an animal's cervical cord and subsequently found those large cells in the medulla oblongata degenerated. To top it all had not Deiters himself—we may be surprised to read—denied that these large cells were part of the acoustic nerve?

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[14] Paul Emil Flechsig, "Zur Lehre vom zentralen Verlauf der Sinnesnerven," *Neurol. Zentralbl.*, 1886, *5*: 545-551.

[15] Vladimir Michailowitch, "Ueber die innere Abtheilung des Strickkörpers und den achten Hirnnerven," *Neurol. Zentralbl.*, 1885, *3* (4): 145-147. *Idem*, "Zur Frage über den Ursprung des Hörnerven und über die physiologische Bedeutung des N. vestibularis," *ibid.*, 1887, *6*: 193-198.

[16] August Forel, "Vorläufige Mittheilung über den Ursprung des Nervus Acusticus," *Neurol. Zentralbl.*, 1885, *4:* 101-103. August Forel and B. Onufrowitz, "Weitere Mittheilung über den Ursprung des N. acusticus," *ibid.*, 1885, *4:* 193-194.

[17] Constantin von Monakow, "Experimenteller Beitrag zur Kenntnis des Corpus restiforme, des 'äusseren Acusticuskerns' und deren Beziehung zum Rückenmark," *Arch. Psychiat.*, 1883, *14:* 1-5.

[18] Benno Baginsky, "Ueber den Ursprung und den zentralen Verlauf des N. acusticus des Kaninchens," [Virchows] *Arch. f. path. Anat*, 1886, *105:* 28-46 [not to be confused with Babinski].

[19] Sigmund Freud, "Über den Ursprung des N. acusticus," *Monatschr. f. Ohrenheilk.*, 1886, *20:* 245-251; 277-282.

[20] Gustav Albert Schwalbe, *Lehrbuch der Neurologie,* in C. E. E. Hoffmann, *Lehrbuch der Anatomie des Menschen* (Erlangen: Besold, 1881), vol. 2, pt. 2, pp. 667-669, Schwalbe described a "third gathering of ganglion cells," in addition to (1) the main or dorsal acoustic nucleus and (2) Deiters' nucleus. (The latter he preferred to call *lateral acoustic nucleus,* as Meynert, Clarke, and Stieda had done.) The "third gathering" Schwalbe found "lateral to the anterior acoustic root," i.e. the vestibular. It had been identified before, he says, by Meynert as *anterior* or *accessory acoustic nucleus;* by Henle as *nucleus acusticus lateralis;* by Huguenin as *superior portion of the anterior nucleus!* Schwalbe stressed the smallness of its "unique" globular cells (15-20 micron as opposed to the cells of Deiters' nucleus 60 and up to 100 micron long.) "Indifferent" eponyms may indeed at times be less confusing, easier to remember, hence preferable!

[21] Christian Friedrich Wilhelm Roller (the Younger), "Eine aufsteigende Acusticuswurzel," *Arch. f. mikrosk. Anat.*, 1880, *18:* 403-408. *Idem,* "Die cerebralen und cerebellaren Verbindungen des 3.-12. Hirnnervenpaares. Die spinalen Wurzeln der cerebralen Sinnesnerven," *Allg. Ztschr. f. Psychiat.*, 1882, *38:* 228-264. Roller's obscurity is compensated by no less than three nuclei named—inconstantly—after him. The most common coupling is Roller's "sublingual" or small celled nucleus of the hypoglossus (*Idem, Arch. f. mikrosk. Anat.*, 1881, *19:* 383-395). Next comes Roller's lateral nucleus of the accessory nerve (*Idem, Allg. Ztschr. f. Psychiat.*, 1881, *37:* 469-489). Finally we have Roller's above quoted description (1882) of a "spinal root of the auditory nerve . . . developed from fasciculus cuneatus." He also says that these "ascending root fibers of the acoustic nerve" appear in the "sparsely cellular

The fact had to be faced, eventually, that not all of the eighth nerve is acoustic. It also turned out later, as it often does, that both sides were correct. Logically opposed as they seemed to be at the time, the clashing viewpoints of Leipzig and Zürich became compatible at the beginning of this century, after the facts of the neuron and the synapse had been firmly established. There was no longer any difficulty in regarding Deiters' nucleus as a halfway house, a relay station in a chain where the endings

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gray mass replacing fasciculus gracilis." Higher up this "ascending bundle" is "interspersed with abundant gray matter" and "may be followed up to the acoustic nucleus in which the well known large cells appear—*nucleus magnocellularis nervi acustici,*" i.e. the nucleus of Deiters. Roller's tract "merges with the inner root of the eighth nerve." Edinger, in the 6th edition of his *Vorlesungen* (n. 1 above), speaking of the "far less well known . . . other branch of the 8th nerve, the vestibular," says that "part of its fibers . . . send down descending acoustic roots (Roller)." He gives no references, but note that he has judiciously changed Roller's *ascending* into *descending.*

[22] The vestibular nuclei are a cluster of nerve cells, hard to separate even in lower mammals where they have mostly been studied, except for the large cells on which Deiters commented. They are impossible to gather in a single section of the medulla oblongata; in toto they can be conveyed to the student's eye only by a means of a schematic drawing. A. Brodal and O. Pompeiano, "The vestibular nuclei in the cat," *J. Anat.,* 1957, *91:* 438–354, comment that "some confusion exists . . . with regard to the delimitation and nomenclature of the vestibular nuclei," also that there are more cell groups than accounted for by the classical four nuclei, and even more vestibular fibers than can be accounted for by those (see illustrations). A. T. Rasmussen, "Secondary vestibular tract in the cat," *J. Comp. Nerol.,* 1932, *54:* 143-159, says that Deiters nucleus is a term "used rather broadly and possibly includes some of the median and superior nuclei."

[23] See n. 15 above.

[24] See n. 16 above.

of Bekhterev's incoming vestibular neurons make contact with the large motor cells of the tract Monakov had transected—the tract later called "vestibulo-spinal." Although in the 1880s the germs of our concepts were in the air, the old anatomical but unphysiological idea had not been entirely overcome that all nerves "originate" in the central nervous system, even those that are sensory and lead into it.

Deiters himself was the inaugurator of the chain concept.[25] He is better known with regard to the neuron theory; here few pioneers compare in importance with him. If Remak discovered the axis cylinder (or axon) and its continuity with the nerve cell[26]—both forming a unit—Deiters enabled those who came after him to accept these units (or neurons) as discontinuous, yet contiguous in relation to each other. And while nerve cells have more than one branching "process," only one of them is an axon for sending out impulses; the others, which Deiters called "protoplasmic prolongations" (later known as "dendrites"), function as receiving organs, antennae as it were. Deiters wrote:

> I finally must discuss the relationship between nerve cell and nerve fiber, hence the manner in which the cell acts throughout the system of the cerebrospinal organ. To this question, we find, extraordinarily divergent answers have been given by the investigators so far. Some have arrived at their results with extraordinary ease, while others in the wake of Kölliker have stoutly upheld the opinion that at present we know too little that is precise and secure; the very construction of a hypothesis is therefore unthinkable. First among the authors whose credulity has gone farthest is Schröder van der Kolk. . . . In general authors, who without further ado thought themselves entitled to publish definite statements, saw an axis cylinder in every cell prolongation. (Stilling, Jacubowitsch). . . . The only observation apt to provide a better insight—Remak's—has been ignored. . . . Remak's statements . . deserve the greatest consideration. With these I fully concur, insofar at least as I assume that there is a fundamental difference between the axis cylinder and the rest of the protoplasmic processes. But I must extend Remak's statement as valid for every better known nerve cell, while he limited it by referring to one kind only, namely the motor cell.[27]

The proof, admittedly not easy, Deiters based on the fact that one prolongation—the axon—is not and cannot be as infinitely branched as are the others; the latter are furthermore "naked," whereas the axon is myelinated.

He wrote with the vigor of youth and the self-assurance, not to say arrogance, of one determined to see everything critically, precisely, and for himself. This compulsion caused him—almost as much as it had caused his forerunner Remak—numerous difficulties with his contemporaries. Unwilling to betray his one and only love—neurohistology—he would forego pursuits more popular and more conducive to an easy career. His seniors preferred men even younger than Deiters for being more practical and compliant. Nevertheless he was the first to give a course at the University of Bonn, his *alma mater* and city of birth, in auscultation and percussion, and he found a little time to take the violinist's or violist's part in a quartet. Physically delicate and morally touchy, he is also described as having "little natural gaiety in him." Ironically, he was to fall victim, as it were, to the well meant advice of his friends: they urged him to relax. As the result of a trip—or so it was thought—to a friend in Tübingen, and cutting short his stay, he contracted typhoid and died in December 1863, barely 29 years old. M.D. for seven years, Privatdozent for five, he practically exhausted his early manhood behind a microscope. The study of atrophic muscle fibers came first, under the tuition of his distinguished chief, Max Schultze. Two monographs on the organ of hearing ("Deiters' cells" in the organ of Corti) were similarly inspired by Schultze's work and, presumably, by the most famous member of the Bonn faculty at the time: Helmholtz. Deiters also combined a year of military service in Berlin with attending Virchow's institute. By 1865 Max Schultze had gathered, prefaced and published the unfinished pages and the vivid drawings of Otto Friedrich Carl Deiters, the deceased son of the local professor of law.[28]

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[25] See n. 1 above, p. 185.

[26] Alfred Meyer, *Historical Aspects of Cerebral Anatomy* (London-New York: Oxford Univ. Press, 1971) has an excellent discussion and references, pp. 163-170.

[27] See n. 1 above, pp. 70-71.

It was Deiters' emphasis on those particular cells as they meet the eighth nerve on its entry into the medulla oblongata, that made them eponymic. Conspicuously large, they are also well provided with dendrites, the "protoplasmic prolongations" Deiters so ably launched as a general attribute of nerve cells. For it must be admitted that he neither was, nor claimed to be, the first to see the relationship of this nucleus to the eighth nerve, and stressed again that he denied their connection. And they are not so related if, as we have seen, the generally accepted view is one of restricting the eighth nerve to the function of hearing. The cells of Deiters' nucleus are indeed not "sensory" nerve cells, but central neurons in the motor limb of a reflex arc: the origin of the vestibulospinal tract.[29]

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[28] Verein der Bonner Aerzte, "Nekrolog (Dr. Otto Deiters)," *Deut. Klin.*, 1864, *16:* 13-15. E. Nieschlag, "Otto Deiters (1834-1863)," *Med. Welt*, 1965, *4:* 222-226.

[29] Arthur van Gehuchten, "Connexions centrales du noyau de Deiters et des masses grises voisines (faisceau vestibulo-spinal etc.)," *Le Névraxe*, 1904, *6:* 21-73.

In the last decade of the nineteenth century so much had become clear, but at the beginning of the sixties Deiters could not venture to say more than this:

> The large cells at the origin of the acoustic nerve have nothing to do with that nerve; they belong to the crura cerebelli ad medullam oblongatam [restiform body]. The acoustic nerve partly circles and partly transfixes them before it joins the extended sensory portion of the cord, in the manner of a sensory nerve. This only in passing.[30]

Let us note, also in passing, that he was wrong in assigning the large cells to the restiform body. Essentially, however, he meant this passage to illustrate his view that the cranial nerves in the medulla oblongata follow the "schema of the spinal cord." Obviously such particularly large cells could only be motor. They served as an excellent example for his novel idea [31]—valid as we know as an intimation of the synapse—that central "junctions" (Knotenpunkte) are a structural principle of the nervous system.[32] "Each termination of a fiber system is the beginning of a new one," he says; consequently "there are ganglion cells which decidedly do not serve as the terminations [origins] of nerves." To this variety belongs "the network of cells of the largest kind crowded into a small area." [33] Once more he returns to it:

> Where the restiform bodies join the medulla oblongata they enclose a gray mass lying in the closest vicinity to the entering acoustic fibers. Hence this might easily be taken for the origin of that nerve, as indeed it has been. . . . The origin of the acoustic has been sought in these very striking gigantic ganglion cells lying almost squeezed against its point of entry (Plate V, Fig. 14, Cr. C.).[34]

On the 315 pages of the posthumous "Investigations into the human and mammalian brain and spinal cord" this (plus the legend to his Fig. 14) is all the space Deiters has devoted to the nucleus named after him. Only one additional comment exists in the book; it is by its editor. Max Schultze in his preface regrets that Deiters has left an all too sketchy account of the acoustic nerve:

> Still we learn that the large cells in the crura cerebelli ad medullam oblongatam, generally considered to be the acoustic nucleus, by no means belong to it. The notes in the manuscript intended to fill this gap are incomplete and contain nothing to further our understanding, hence I refrain from including them.[35]

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[30] See n. 1 above, p. 85.
[31] *Ibid.*, p. 231.
[32] *Ibid.*, p. 185.
[33] *Ibid.*, p. 231, Fig. 14, Cr.c.
[34] *Ibid.*, p. 294.
[35] *Ibid.*, p. x.

Now, to confuse a little our present issue, Schultze too proposes to name a nucleus after Deiters. The cell group he has in mind—"a sizable nucleus"—lies in the reticular formation. Here Deiters claimed a "nucleus for the lateral tract" of the spinal cord; its function is to make contact with the corpus restiforme.[36] In other words, had Schultze's suggestion prevailed we would presumably be calling today the origin of the reticulospinal tract after Deiters. After all, he was the discoverer of one of its nuclei as well as that of the vestibulospinal tract.

Deiters did not refer by name to the authors who, in his opinion erroneously, interpreted as acoustic the giant cells in the path of the restiform body. Monakow lists nine such names, in fact he suggests more by adding an "etc." to Dean, Clarke, Stieda, Stilling, Meynert, Henle, Huguenin, Wernicke, and Roller.[37] Most of them followed Deiters in time. But Lockhart Clarke, for instance, had briefly written in 1861 that, in addition to the "principal nucleus of the auditory nerve"—the acoustic tubercle—"another portion . . . is in contact . . . with the inner side of the restiform body . . . containing large nerve-cells with branched and exceedingly long processes. . . ."[38] What made Deiters' fame in this matter, then, is not a discovery but a dissenting and enlightened, if not altogether correct interpretation.

The nucleus was named after Deiters by an anatomist in Turin: Giambattista Laura. In 1881 Laura repeated in London, and in French, what he had written in Italian before:

> The large cells of that nucleus are regarded by all and even the most recent observers as the external acoustic nucleus . . . but they do not end there; they do not send their axons into the root of the nerve but, on the contrary, direct them inward and forward, in a large bundle running across the root of the facial nerve and towards the center of the *spinal cord* [Ital. mine]. To this nucleus I have given the name "nucleus

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[36] *Ibid.*, p. xi, referring to pp. 202-229. According to J. Dobson, *Anatomical Eponyms* (London, Baillière, Tyndall, & Cox, 1946), p. 61, Deiters discovered not only the lateral vestibular nucleus but also the reticular formation. A. Meyer (n. 26 above), p. 70, likewise credits Deiters with having coined *formatio reticularis*. Deiters probably was the first to use this particular version of the term (n. 1 above, p. 198). But he did so as though it were current and undeserving of an originator's demonstration. This network of fibers, first considered vascular, Meyer says, had been seen before by Arnold (1838) and by Stilling (1843), then called *processus reticulares* by Lenhossék the Elder (1855), and "reticular fibers" by Schroeder van der Kolk (1859)—all still without Deiters' emphasis on the *cellular* aspect of the "beams [Balken] of gray matter" lying within this formation.

[37] N. 1 above; similar also in Schwalbe, n. 20.

[38] J. Lockhart Clarke, "Notes of researches on the intimate structure of the brain. Second series," *Proc. Roy. Soc. London*, 1861, *9:* 359-365.

of Deiters" because that great observer was the only one to throw doubt on its connection with the acoustic nerve.[39]

True, its neurons are not primary ones, and they are not connected with the "acoustic." Nevertheless they are connected with the eighth nerve, that is with its "vestibular" portion discovered by Bekhterev, as we saw, a few years later.[40] By that time Laura's nomination of Deiters for the nucleus was generally accepted, if not Laura's reasoning. To some extent the eponym was and has remained paradoxical. Yet Laura's choice seems a fair one. Historical justice was done, as in the case of Luigi Rolando whose name adorns that cortical fissure flanked by vertical gyri, although it was Vicq d'Azyr who had them illustrated first.[41] But as they stood for a new principle—Rolando's idea of the regularity in the seemingly chaotic cerebral sulci and gyri—so Deiters had presented the nucleus as an example for the system of relay stations that pervades the brain and spinal cord. (He called them "junctions" [Knotenpunkte] in the youthful spirit of railroad engineering.)

Eponyms notoriously do not always mark strict "firsts." Conferring them also is not unlike the Roman Catholic practice of granting sainthood to foremost propagators of the Faith. The analogy may be more than casual. For not only does an observer's devotion, occasionally to the point of martyrdom, justify the parallel; there also is perhaps, even going as far back as the labors of Hercules, a basic trend in Western tradition that links the saint to the pioneer. Eponyms of course have since Jakob Henle[42] had their detractors as well as their continued advocates. The association with worship has perhaps been a major reason for deploring their use. Be that as it may, clinicians, anatomists, and all manner of scientists will retain some of them, out of convenience, piety, or vanity. Historians feel called upon to do their postmortem examinations; after scanning the books they may come up with a tortuous verdict.

The line from Terentius is often quoted about the reader's grasp deciding the fate of a book—*pro captu lectoris habent sua fata libelli.* Labels too—eponymic tags—are subject to this fate.

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[39] Giambattista Laura "Des origines profondes de quelques nerfs craniens," *Trans Internat. Med. Congr. London,* Aug. 1881, pp. 182-184 (183). *Idem,* Nuove ricerche sull'origine reale dei nervi cerebrali (glossofaringeo, acustico, facciale, abducente e trigemino," *Accad. Sci. Mem. Torino,* 1880, *32:* 77-11.

[40] See n. 15 above.

[41] Paul Broca, "Note sur la topographie cérébrale et sur quelques points de l'histoire des circonvolutions," *Bull Acad. Méd. Paris,* 1876, *41:* 824-834. F. Schiller, "The rise of the 'enteroid processes' in the 19th century," *Bull. Hist. Med.,* 1965, *39:* 326-334.

[42] Dobson (n. 36 above), p. 7.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

ANNOUNCEMENTS

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE

Elections

President (2-year term).	Lester S. King, Chicago, Ill.
Vice-President (2-year term):	John Duffy, College Park, Md.
Council (3-year terms):	George E. Gifford, Jr., Boston, Mass. Charles Rosenberg, Philadelphia, Pa Ilza Veith, San Francisco, Calif. Dora B. Weiner, Purchase, N.Y.

Committee Chairmen, 1974-75

Garrison Lecture Committee, J. Harvey Young; *Nominating Committee,* James H. Cassedy; *Program Committee,* Guenter B. Risse; *Osler Medal Committee,* Charles G. Roland; *Welch Medal Committee,* Stanley W Jackson; *Local Arrangements Committee,* Lawrence C. Parish.

Awards

The Welch Medal was awarded to Dr. Walter Pagel "for his extensive and most valuable publications concerning early biology and medicine which have not only decisively contributed to the understanding of these subjects but also raised broad philosophical issues for both science and medicine."

The Osler Medal was awarded to David B. Lovejoy, Jr., University of Rochester School of Medicine and Dentistry, for his essay, "The Hospital and Society: The Growth of Hospitals in Rochester, N.Y., in the 19th Century." Honorable mention was given to Helen Zia, Tufts University School of Medicine, for "The Development of the Health Delivery System in the People's Republic of China"; and to Amy Pruitt, Harvard University Medical School, for "Studies of Aphasia in the 19th Century."

Annual Meeting, 1975

The Association will hold its 50th anniversary meeting at the Warwick Hotel, Philadelphia, Pa., April 30-May 3, 1975. Persons interested in having papers considered for presentation are asked to submit a 200-300 word abstract to the Chairman of the Program Committee, Dr. Guenter B. Risse, Department of the History of Medicine, University of Wisconsin Center for Health Sciences, 1305 Linden Dr., Madison, Wisconsin, 53706.

As in previous years, papers may deal with any aspect of the history of medicine or the biomedical sciences, but this year preference will be given to topics dealing with American medicine in view of the upcoming bicentennial celebrations. Presentations will be strictly limited to 20 minutes. Time for discussion will be provided after each paper or group of papers at the discretion of the session's presiding officer.

Contributions of non-members as well as members will be considered. Participants are reminded that the *Bulletin of the History of Medicine,* official organ of the Association, reserves the right to consider for publication any papers delivered at the meeting.

William Osler Medal Student Essay Contest, 1975

Students in medical schools of the United States and Canada, and graduates of the class of 1974, are eligible to enter the Osler Medal contest. Essays in the history of medicine, of a maximum length of 10,000 words, must be submitted by March 1, 1975, to the Chairman of the Osler Medal Committee, Dr. Charles Roland, Mayo Medical School, Rochester, Minnesota, 55901. Information about rules and eligibility can be obtained from Dr. Roland.

NATIONAL NEWS

Boston Medical Library

Dr. Lloyd Hawse and Dr. George E. Gifford, Jr., have been elected President and Secretary respectively of the Library's Board of Trustees. With the assistance of Mr. Richard J. Wolfe, Curator of Rare Books, Countway Library, they have instigated a series of "critiques" in which a particular book or subject is presented by a discusser, with group discussion following. Participants include faculty, students and guests who are invited "to enjoy coffee, conversation and the company of bibliophiles."

College of Physicians of Philadelphia

After forty years of distinguished service, first as Librarian and since 1953 as Curator of Historical Collections, Dr. Walton B. McDaniel, 2d, retired on October 31, 1973. The occasion was marked by a meeting of the Council of the College at which Dr. McDaniel was named an Honorary Fellow and was presented with a bound volume of fifty letters from friends and colleagues. A long-time member of the American Association for the History of Medicine, Dr. McDaniel was its Garrison Lecturer in 1959. The *Bulletin* wishes to join in paying tribute to Dr. McDaniel for his contributions to librarianship and to medical history, and to express best wishes to him on his retirement.

Duke University

A library of some 3,500 volumes in the history of science has been presented to Duke Medical Library by Mr. and Mrs. Samuel Barchas of Sonoita, Arizona. The collection incorporates one of the collections of the late Herbert Evans and includes many important books in biology and medicine. Rooms for the collection and a proposed Barchas Center for the History of Science are being planned. In recognition of the gift, Duke has undertaken an acquisitions program of more than $100,000 a year for rare books in medicine and science. A program of scholarly publications and translations is also planned.

G. S. T. Cavanagh has resigned as Director of the Medical Center Library to devote full time to historical collections. Warren Bird has succeeded him as Director.

History of Behavioral and Social Sciences

Cheiron: The International Society for the History of Behavioral and Social Sciences will hold its seventh annual meeting at Carleton University, Ottawa, Ontario, June 5-8, 1975.

International Academy of the History of Medicine

At the Academy's fourth Symposium in Vienna in September 1973, the following officers were elected: George Rosen, Department of the History of Science and Medicine, Yale University (President); José M. López Piñero, Instituto de Historia de la Medicina y de la Ciencia, Universidad de Valencia (Secretary); John Z. Bowers, The Josiah Macy, Jr. Foundation, New York City (Treasurer).

The next meeting of the Academy will take place in New Haven, Conn., in 1976, the exact date still to be determined.

Josiah Macy, Jr. Foundation

Sixteen fellowships in support of study in the history of medicine and the biological sciences have been awarded for the year 1974-75. Of these nine are new fellowships—all for predoctoral study—and seven fellowships were renewed. Recipients are: Toby Appel, Steven D. Helgerson, Pauline Mazumdar, Robert Miciotto (Johns Hopkins); Nancy E. Gallager (U.C.L.A.); Jane Maienschein (Indiana); James Bono, Robert McCormick, Dov Ospovat, Karen Reeds, David Rosner (Harvard); Dale C. Smith (Minnesota); Malcolm Kottler, Merriley Borrell Trench (Yale); Ann Carmichael, Peter English (Duke).

Information on the Fellowship Program may be obtained by writing to the Fellowships Office, Josiah Macy, Jr. Foundation, One Rockefeller Plaza, New York, N.Y. 10020.

Logan Clendening Traveling Fellowship

Funds left by the late Dorothy Hixon Clendening Clark have made possible the renewal of the Clendening Traveling Fellowship which provides $1,000 for three months travel and study of medical history. The Fellowship is open to medical students or college seniors accepted for medical schools. Applications must be submitted by March 15, 1975. Further information and application forms may be obtained from Robert P. Hudson, M.D., Chairman, Department of the History of Medicine, University of Kansas Medical Center, Kansas City, Kan. 66103.

National Endowment for the Humanities

As part of its program to make the humanities more influential in American life, the Endowment this summer is sponsoring three month-long seminars for medical practitioners "to study under the direction of humanists who have themselves seriously examined the human questions arising out of everyday practice." The seminars, each limited to about 15 participants, will be directed by Charles E. Rosenberg (history), William F. May and David H. Smith (ethics), and H. Tristram Engelhardt, Jr. (ethics and philosophy). The first will take place July 5-Aug. 9, at the University of Pennsylvania, the second on the same dates at Williams College, and the third at the University of Texas Medical Branch, Galveston, Sept. 9-Oct. 4. Tuition is free, and participants will receive expense stipends of $1,500.

Osler Library, McGill University

Mr. Philip Teigen has been appointed History of Medicine Librarian in the Osler Library. He succeeds Miss Ellen Wells, now Associate Rare Books Librarian at Cornell University. Mr. Teigen, who has a masters degree in library science from the University of Wisconsin, is completing a doctorate in the history of science at the same university. He has worked as teacher and librarian and since 1970 has been assistant to John Neu, bibliographer for the history of science.

University of Chicago

Two Fishbein Lectures were given in the spring of 1974. On April 5, Professor R. S. Westfall, Department of History and Philosophy of Science, University of Indiana, spoke on "The Role of Alchemy in Newton's Scientific Career." Professor Saul Benison, Department of History, University of Cincinnati, lectured on May 24. His topic was "Speculation and Experimentation in Early Polio Research."

Allen G. Debus, Professor of the History of Science, has been reappointed Director of the Morris Fishbein Center for the Study of the History of Science and Medicine for the years 1975-1978.

University of Texas Medical Branch

In the rare book section of the Moody Medical Library an exhibit of rare book binding has been put on display. It traces the steps in hand book binding and includes finished examples. The binding is the work of Zoltan Olah, who learned his craft in Rumania where he worked before immigrating to the United States. In January 1974, Mr. Olah joined the staff of the Moody Medical Library where he is responsible for binding of the library's large collection of rare books.

Award

On August 6, at the annual meeting of the American Institute of the History of Pharmacy, the Edward Kremers Award for distinguished writing in pharmaceutical history was presented to Dr. David F. Musto, Professor of Psychiatry and History at Yale University.

Death

We have learned with great regret of the death on January 16 of Dr. José Joaquín Izquierdo (1893-1974), Professor Emeritus of Physiology, Universidad Nacional Autónoma de México. Dr. Izquierdo, who was for many years an Honorary Member of the American Association for the History of Medicine, will be best remembered by readers of the *Bulletin* for his extensive writings in the history of physiology.

Request for Information

Dr. Bruce S. Schoenberg (Mayo Clinic, Rochester, Minn. 55901) is seeking information concerning Henri Morau, French physician and researcher, and his work on tumor transplantation in mice. Morau was born in 1860 and died "relatively young." He published a number of works between 1889 and 1896. Dr. Schoenberg would appreciate hearing from anyone familiar with biographical material or the existence of a portrait of Morau.

PROGRAMS OF MEETINGS

Bay Area History of Medicine Club, San Francisco

March 13, 1974: "Dr. Thomas Glass of Exeter," Reinhard S. Speck.

April 17, 1974: "From Degenerescence to Psycho-analysis," Ruth Friedlander; "Early Measurements of Blood Pressure in Man, A Controversy over Priority," David B. Gordon.

College of Physicians of Philadelphia, Section on Medical History

May 8, 1974: Kate Hurd Mead Lecture: "A New Look at Major Walter Reed." William B. Bean.

George W. Corner History of Medicine Society, Rochester, N.Y.

April 18, 1974: "Government Money and the Development of American General Hospitals in the 19th Century," David Lovejoy.

May 16, 1974: "William W. Reid, Rochester Physician and Pioneer of Orthopedics," Joseph V. McDonald.

Handerson Medical History Society, Cleveland, Ohio

April 15, 1974: "Baths and Bathing," A Benedict Schneider

Johns Hopkins Medical History Club, Baltimore, Md.

April 11, 1974: "The Pavlovian Laboratory of the Johns Hopkins Medical School, 1929-1969," W. Horsley Gantt.

Mayo Foundation History of Medicine Society, Rochester, Minn.

July 10, 1974: "Richard Caton and the Electrical Activity of the Brain," Bruce S. Schoenberg.

University of Virginia Medical History Society

April 17, 1974. "Where is Fancy Bred: A Selected History of Neurology," Eugene S. Flamm.

Washington Society for the History of Medicine, Washington, D.C.

May 11, 1974: "Dieting in History," Jeanne Brand.

William Snow Miller Medical History Seminar, University of Wisconsin-Madison

April 8, 1974 (with the Center for Medical Sociology and Health Services Research): "Medical History and the Health Systems Innovator," J. Thomas May.

April 10, 1974 (with the Department of the History of Science). "Medical Education in the Middle Ages," Charles Talbot.

BOOK REVIEWS

Hunter's Lectures of Anatomy. Amsterdam, New York: Elsevier Publishing Co., 1972. 1 vol. (v.p.) $12.00.

In 1959, at the auction sale of a deceased person's effects in Adelaide, South Australia, an alert lady of literary interests, Miss Nell Dowd, acquired two notebooks containing altogether 299 pages of neat and legible writing which turned out to be notes taken by an English medical student, Charles White, from the anatomical lectures of Dr. William Hunter. On one page of the second notebook, where the lecturer spoke of a patient who had sustained a war wound and "is now living," the date 1752 is entered as a footnote. No other students' notes on Hunter's lectures as early as this are to be found in the United Kingdom, as Miss Dowd explains in her brief introduction. In 1752 William Hunter had been teaching anatomy privately for six years, and for three years had been settled in the large house in Covent Garden where he conducted his school until the final move to Great Windmill Street. He was just then completing his studies for his grand work, *Anatomy of the Gravid Uterus.* His brother, John Hunter, who was to win greater fame, had been assisting him for three years.

Miss Dowd has identified the writer of the notes and has traced the manuscript from London to Manchester, where Charles White became a well-known practitioner, and thence, in the twentieth century, to Australia. The Elsevier Press has published the two notebooks in facsimile in one volume, with a brief foreword by Sir Charles Illingworth, surgeon and medical historian of Glasgow, and a three-page introduction by Miss Dowd. There are no explanatory notes, nor is there an index; the reproduction is so exact that pages 21 to the end at page 114 of the second notebook are unnumbered, as they were left by Charles White or whoever made the fair copy from his classroom jottings. This careful record of the great surgeon-obstetrician's early teaching of anatomy is thus left to the reader to make what he can of it.

It would be unwise to regard a student's notes as accurately revealing his teacher's thought. Only upon one topic, to be mentioned later, are White's notes of critical value for the history of Hunter's ideas. What can be gotten from them is a picture of what a young man about to enter practice had learned from his study of anatomy.

The skeleton, muscles, and peripheral nerves are described in detail, the brain only in a rudimentary way There is no mention of two roots (dorsal and ventral) of the spinal nerves, although Hunter told his hearers that a member may be deprived of motion and yet retain sensation, and vice versa. The heart, the arteries and the veins are traced in detail, but it will be a surprise to readers not familiar with eighteenth-century teaching of human anatomy, to find no reference whatever to capillary blood vessels, not even in the lungs, 91 years after Malpighi had shown with utmost simplicity and clarity how the terminal arteries and the smallest veins are connected through the capillary network, in the frog's lung, and by implication throughout the body. William Hunter, as quoted by White, told his students that the pulmonary arteries end in a fine reticulum upon the [air] cells, called by White the "Rete Malpighi," but he did not say how the veins begin. Hunter used the microscope to some extent—e.g., he mentioned in his lectures examining with a lens the chorion and the pupillary membrane of the fetus, and the tarsal glands of the eye—but the beautiful injections in which he and his brother took pride were made with colored wax or with pigment suspended in varnish, and did not enter the finest vessels. Charles White and his fellow students must have pictured the arterioles as open-ended, pouring the blood into tissue spaces to be picked up by open-ended venules. In glands and the "screening" membranes—e.g. peritoneum and pericardium—the terminal arterioles were thought to be so fine that they allowed only the watery part of the blood to be secreted.

Charles White's notes support Hunter's side of the famous controversy with Monro over priority in announcing the view that the lymphatic channels are the principal absorbent vessels throughout the body, as the lacteals are in the intestines, for that interpretation is clearly quoted (p. 13). Yet there is almost no description of lymphatics other than the lacteals and the thoracic duct. It was not until the very year when White was taking his notes that John Hunter found that by injecting non-particulate fluids into the tissue spaces with a sharp needle he could break into the local lymphatics, and began to make the preparations that would fully reveal their extent and distribution.

Hunter's controversial disposition is apparent in many derogatory references to the opinions of other anatomists. The seriousness of the lectures is relieved here and there, for the modern reader, by amusing quaintness of thought or wording—"Pulses are an improvement in Physick; before the time of Hippocrates they were not attended to"; the omentum "is a preparatory organ for the bile, the blood passing through it carrying an oil supposed to be necessary for the composition of that fluid"; the gall bladder "in a standing position . . . lies forwards and downwards; its fundus is raised at a full meal and is depressed when the stomach is empty, whence arises that uneasy sensation which attends hunger."

Miss Dowd has rescued for us an instructive picture of a great teacher and an attentive student engaged in the age-old study of human anatomy and physiology.

Reviewed by GEORGE W. CORNER, M.D., Professor Emeritus of Embryology, The Johns Hopkins University, and Executive Officer of the American Philosophical Society, Philadelphia, Pa.

HARRY WAIN. *A History of Preventive Medicine.* Springfield, Ill.: Charles C Thomas, 1970, x + 407 pp. $14.75.

This book is apparently designed for, and may appeal to, the non-professional general reader or to the physician with little knowledge of the history of medicine but will be of little value to those seriously interested in medical history. About half of the volume is given over to a somewhat sketchy history of medical thought and discovery, presented largely through a series of disconnected biographical sketches, each of a few paragraphs. The other half deals with specific diseases. Separate chapters are devoted to such topics as plague, smallpox, syphilis, diphtheria, tuberculosis, poliomyelitis, arthropod-borne diseases and deficiency diseases.

Discussion of developments prior to 1900 is based on the author's reading of standard and readily available source material. The author condenses in a few paragraphs the salient facts as to the background and contributions of each of several hundred contributors to medical knowledge. The reader without ready access to encyclopedias or to other historical volumes will find here interesting brief biographical sketches. The chapters on specific diseases contain valuable background material sadly lacking in most textbooks of medicine. Possibly these are the book's greatest value to the professional reader.

The student of preventive medicine will, however, be disappointed and mystified by some of the chapters on specific diseases and the progress of the 20th century. He may well wonder why the chapter on tuberculosis contains no reference to the highly successful programs for eradication of bovine tuberculosis and their impact on reduction of extrapulmonary infection, the only reference being limited to three brief paragraphs in the chapter on milk-borne diseases. Similarly he may wonder at the complete omission of any reference to goitre prevention through supplemental iodine intake. He may ask why there is no mention of the prevention of tetanus among the wounded of World Wars I and II, true triumphs of military preventive medicine. He will be surprised to find no reference to typhus vaccine and only a six-line mention of yellow fever vaccine, a discovery honored by award of the Nobel prize, but he will search in vain for any discussion of the vaccine's significance or more than a passing reference to jungle yellow fever. He will likewise be surprised at the scarcity of discussion of public controversies that have attended smallpox vaccination, pasteurization of milk and fluoridation of water. He can hardly overlook the superficial attention given to the highly controversial aspects of poliomyelitis vaccine development. He will wonder at the author's (or publisher's) inconsistent disregard for accepted practices of capitalization of scientific names of insects and pathogens, and at the number of typographical errors.

All in all, while the casual reader will find here a volume that can be picked up for a few minutes of interesting but not too serious reading, the student of public health history will be disappointed by the many omissions, by the lack of balance that runs throughout the volume, and by the inadequate discussion of significant progress of the current century.

Reviewed by GAYLORD W. ANDERSON, M D., Dr.P.H., Dean Emeritus, School of Public Health, University of Minnesota, Minneapolis, Minn.

James L. Larson. *Reason and Experience. The Representation of Natural Order in the Work of Carl von Linné*. Berkeley: University of California Press, 1971. vii + 178 pp. Ill. $7.50.

This is an important contribution to the field of Linnean studies. Scholars concerned with the understanding of the setting and functioning of the categories of the Linnean taxonomic endeavor will, no doubt, give Larson's book a prominent place in their working libraries.

Not dealing as such with classification of animals, minerals or diseases, the work is centered on botanical taxonomy as the core of Linné's thinking, and it is organized in order to give the reader an experience of the conflicting strains inherent in

Linnean thought and inherited, the author believes, from the previous botanical tradition.

The first chapter ("The System of Nature and the Natural Method") is as well an attempt to place Linnean botany in its proper historical background. Contrary to Sachs, Larson sees as a continuous development the history of botany from the herbalists to Linné. Cesalpino's thought is no more understood as a disruptive intervention but rather as the locus of an evolutive transformation by which is made clearer the common conceptual basis of the systematic work of the herbalists and of the thought of the Aristotelians. From Cesalpino on, then, botany would have been an Aristotelian science, ruled by the two structural ideas of hierarchy and continuity of natural forms, and embodying an organography providing an (equivocal) rationale for the discrimination between marks or *differentiae,* by assertions on the relative importance of the organs bearing these marks.

In pre-Linnean botany these *a priori* considerations supported by the principle of finality had, however, to be grossly coupled with practical requirements for plant identification, themselves often leading to groupings contradicting actual observation of natural affinities. "This unstable amalgam of opposing tendencies, of observation and practical determination, lay at the root of systematic instability, and the inability of naturalists of this period to set forth a generally acceptable or applicable system."

Central to Larson's book is the idea that these conflicting trends "complemented, contrasted and combined with one another at every level of the Linnean system."

It is to a thorough analysis of the constitution of these levels that the next three chapters, "Classes and Orders," "Genera," "Species and Varieties," are devoted. "Concerned less with results than with the formative forces in Linné's system," the author, with ingenuity and coherence, has written these chapters on the same pattern, in such a way that each one replicates in its structure the dual tradition haunting, and largely shaping, the system. The reader is successively informed of the theoretical constructs used to think, articulate and justify the constitution of each taxon, then of the practical approaches actually used to meet the requirements of identification and grouping, and, last, of the resulting agreements and discrepancies.

Since Linné considered his genera to be natural, one could think that there should have been, at this level at least, a tight coordination between theory and practice. Larson, however, points out very convincingly that it was not the case. For instance, particular species were in practice used as types in the building up of genera, and plant *habitus* also played an important role, despite the fact that these procedures were not and could not be embodied in the strict theoretical formulation of the method. This inadequacy could of course just be amplified at the level of higher taxa. Linnean classes and orders were by no means natural, and Larson thinks (agreeing with Henri Daudin) that the fragments of natural method were juxtaposed with a practical systematic construction but were "never quite integrated in that construction." As for the species (concerning which Larson provides what is probably the best discussion to be found in Linnean historiography), their treatment also implies the same disparity. Though, for instance, Linné devoted much work trying to account theoretically for what he believed to be the formation of new species through hybridization, this had no effect on his practice as taxonomist; he continued to define species by "constant morphological discontinuity." This recurring internal conflict, the author shows in his last chapter, also had its effects on the Linnean understanding and practice of nomenclature.

Of this rich book, full of detailed analysis, it is just possible to give here a schematic idea. Larson's description of the conflicting strains present in Linnean taxonomy is thorough and convincing. It should remain a definitive acquisition for our knowledge, even though many a reader, probably, will question the thesis that this conflict was inherited from a botanical tradition stemming from the herbalists. Where Larson says that after the publication of Bauhin's *Pinax* in 1623 "a lethargy settled upon systematics" for more than a century, one may wonder if rather than using the metaphor of a lethargy (which maintains the idea of a continuity) we should not plainly acknowledge this gap as indicative of a real historical discontinuity. The continuity would then just be a retrospective construct concealing the fact that the whole intellectual framework of sixteenth-century botany was constituted and functioning according to rules and purposes quite extraneous to the field later illustrated by Ray, Tournefort and Linné. (No herbalist for instance would have conceived of nature as a cryptogram as Linné and many learned men of his time did.) To ascertain the reality of this discontinuity would not lessen the strength of the main thesis of this excellent book.

Reviewed by CAMILLE LIMOGES, Ph.D., Director of the Institute for the History and Sociopolitics of Science, University of Montreal, Montreal, P.Q., Canada.

JOHN H. WINSLOW. *Darwin's Victorian Malady: Evidence for Its Medically Induced Origin.* Philadelphia: American Philosophical Society, 1971 (Memoirs of the American Philosophical Society, vol. 88). 94 pp. $2.00.

Charles Darwin suffered from a variety of symptoms, including anorexia, nausea, vomiting, headache, skin eruptions, and easily provoked states of exhaustion. The nature of these symptoms has been a matter of much dispute, with the accent now on psychological understanding, now an organic explanation. In his review of this dispute, the author dwells at some length on the psycho-analytic approaches, which considered the symptoms Darwin's self-inflicted punishment for unconsciously wishing the death of his father and (or) a device by which Darwin successfully protected himself against unwanted intrusions threatening concentration on his work. Winslow argues that neither antagonism to his father, who was not "a cold Victorian father-figure" (p. 10), nor feelings of guilt can be proved. Nor was Darwin a hypochondriac, as has often been assumed.

On the other hand, the list of former diagnoses of organic illnesses, which Winslow submits (p. 47), is a formidable one, ranging as it does from sequelae of severe seasickness, suffered during his voyage on the Beagle, to appendicitis, brucellosis, peptic ulcer, pyorrhea, gout, malaria, inherited neurasthenia, Chagas disease (Brazilian trypanosomiasis). Instead of these diseases—with or without the addition of a neurotic element—the author proposes chronic arsenic poisoning caused by the prescription of Fowler's solution and creating a clinical picture congruent with Victorian dyspepsia (p. 24). According to Winslow, this was a malady from which many eminent

Victorians (e.g. Carlyle, Thomas Huxley, George Eliot) suffered, and which he names "Fowler's Disease" (pp. 79-84).

The detailed juxtaposition of Darwin's symptoms and symptoms "which may occur in chronic arsenic poisoning" (p. 59) is impressive, as is the juxtaposition of the latter and the symptoms in dyspeptics as described in 1846 by Sir James Clark. It is indeed possible that many so-called "dyspeptics" suffered from chronic arsenic poisoning helped by the action of other drugs, such as calomel (cf. p. 86, footnote), and Darwin cannot be excluded as a possible victim.

The author admits that only positive findings in Darwin's remains, "or some other evidence of his having taken arsenic for a long period of time" (p. 74) would fully close the subject. But what direct evidence has the author adduced that Darwin took arsenic at all?

As far as I see, the only testimony quoted is the remark by John M. Herbert in 1882 that while at Cambridge (1828-1831), Darwin "had at one time an eruption about his mouth for which he took small doses of arsenic" (p. 43). Whatever the condition of his mouth may have been (eczema of the lips has been suggested), we cannot tell whether it was a passing minor illness, or a manifestation of the complex accompanying him throughout his life. By 1831, Darwin complained about various symptoms and of feeling miserable (p. 48). The likelihood that this was due to continuous arsenic medication is diminished by the continuation of Herbert's reminiscences. "He [i.e. Darwin] told me that he had mentioned this treatment [i.e. of his mouth by arsenic] to his Father and that his Father had warned him that the cure might be attended with worse consequences—I forget what he said the risk was but I think it was partial paralysis." One warning voice—how many more there were in Europe and Anglo-Saxon countries should be examined in detail.

While preparing for the *Beagle,* Darwin requested of his sister that she ask their father "if he thinks there would be any objection to my taking arsenic for a little time, as my hands are not quite well, and I have *always observed* [Winslow's emphasis] that if I once get them well, and change my manner of living about the same time, they will generally remain well. What is the dose?" *Pace* Winslow, Darwin does not say what medicine (if any) he previously used or found effective for his hands, and his whole query, especially regarding the proper dose, argues against continued use of arsenic and for heeding his father's earlier warning. The fact that he next wrote his sister that he would not take any arsenic (p. 44) corroborates this interpretation. This is all the material the author adduces, and its nature is ambivalent. It does not point to prolonged use of arsenic; and it does not reveal use of arsenic prior to the symptoms of Darwin's illness.

Although the case for Darwin does not seem to be proved, and the controversy between psychologists and organicists is likely to continue, this little book is of considerable interest. Even if "Fowler's Disease" were not as wide-spread as the author contends (a contention that needs further investigation), he has at least reminded us forcefully of the negative impact of much medical treatment at the very time when the foundations of the new scientific medicine were laid. We have long been aware that some traditional treatment was ineffective, and even injurious in individual cases. We must reckon with the possibility that at times it even proved harmful on a large scale.[1] It is not only the thought of iatrogenic disease that is disturbing. Equally disturbing is the long time that elapsed between Fowler's recommendation of arsenic in 1786 (p. 46) and the full appreciation of the "sinister long-term effects" of arsenic poisoning in the second half of the present century (cf. p. 75 ff.).[2]

Fifty years ago, Von Ziemssen's popular formulary advised caution in the use of preparations containing arsenous acid "because of poisoning (gastric and intestinal catarrh, conjunctivitis, bronchitis, peripheral paralysis)." Nevertheless, the drug was recommended in chronic skin diseases, especially psoriasis, acne, neuralgias, chorea, paralysis agitans, Addison's disease, Basedow's disease, pernicious anemia, malignant lymphomas, leukemia, chronic malaria, as well as for the improvement of the general nutritional condition, especially where fattening was desirable. Then came a *nota bene*. "Preparations of arsenic must always be given upon a full stomach and in slowly increasing doses (possibly to the maximal dosis and more)!" followed by the above mentioned *caveat*.[3]

As in the case of x-rays, decades passed before the potential threat was fully realized. With greater suspicion, the danger might have been appreciated earlier. Nevertheless, long-term effects obviously need time to display their full range, a time which exceeds that of experimental tests and clinical trials. The recent discovery of the carcinogenic effect upon young girls of diethylstilbestrol taken by their pregnant mothers some fifteen years before is a case in evidence. Every drug when admitted to general use goes on "historical trial" the length of which is unpredictable.

Though the nature of Darwin's disease may still be open to debate, we must be grateful to the author for stimulating a discussion of more than "merely" historical interest.

Reviewed by OWSEI TEMKIN, M D., William H. Welch Professor Emeritus of the History of Medicine, The Johns Hopkins University, Baltimore, Md.

[1] Professor William Coleman's review of this book in *Science* (13 May 1972, *176:* 669 f.) also raises the question of "the social framework within which Darwin and contemporary dyspeptics suffered."

[2] Actually the time span is even greater, since arsenic, in different forms, was used before and after Fowler.

[3] Von Ziemssen's *Rezepttaschenbuch für Klinik und Praxis*. Dreizehnte Auflage. (Leipzig: Georg Thieme, 1922), p. 56.

[4] After submission of my manuscript, W. D. Foster's review in *Isis*, 1972, *63:* 591-592, came to my attention. On the basis of original Darwinian material at Down House, which he was able to consult, Dr. Foster has reached the conclusion that there is no evidence for Darwin's having taken arsenic and that Darwin's symptoms and appearance do not specifically point to arsenic poisoning. Dr. Foster himself leans towards a psychogenic explanation of Darwin's ill health (see his article in this *Bulletin*, 1965, *39:* 476-478).

LEONARD A. STEVENS. *Explorers of the Brain*. New York: Alfred A. Knopf, 1971. ix + 348 + x pp. $7.95.

As scientific medicine becomes increasingly complex there is a growing need for authors who can distill with accuracy the essence of advances in a form readily comprehensible, not only to the informed layman, but also to the medical practitioner who has neither the time nor the special knowledge to tackle the land-slide of literature which now threatens to inundate him. Mr. Stevens is one of these gifted writers and he has tackled the complex history and present-day status of neuro-biology with skill, accurate information, and discernment in a clear and interesting style, readily understandable to those lacking biological training or technical jargon. Not being content to absorb information from books and papers and then regurgitate it, he has sought advice and elucidation from leading neuroscientists themselves.

Almost the first two-thirds of the book is devoted to the history of nerve function from the Ancient Greeks to about 1970. The best way to understand complicated modern theories is to trace them historically, thus proceeding necessarily from the simple to the complex, and this is so in the case of nervous transmission. In the chapters on early notions there are a few errors of fact but these are minor and some are probably the fault of defective secondary sources. Occasionally the author elaborates less important issues at the expense of the more significant ones but, on the whole, his account is accurate and well balanced and he guides the reader through complexities with a skill that makes abstruse problems seem easy.

The rest of the book deals with the electroencephalogram, brain stimulation, and with the problem of memory, again with an historical approach. In the case of the last of these, the author gives less prominence to the work of J. Z. Young on the octopus brain than is due, and, on the whole, perhaps over-emphasizes American contributions, outstanding as they certainly have been. The book ends with a sixteen page bibliography and good index.

The publishers are in error when they claim that this is ". . . the first historical narrative of the long and difficult effort to understand how the brain and nervous system function. . . ." We need only recall Dr. M. A. B. Brazier's excellent survey of neurophysiology in the *Handbook of Physiology* (1959). Nor are Sherrington and Ramón y Cajal "lesser-known explorers"! They are however correct when they state that Mr. Stevens has ". . . a complete grasp of the subject—and the ability to communicate both the technical aspects with the human excitement of these scientists' achievements. . . ." His book can be recommended as an informative and stimulating presentation of a vital area of research, suitable for a medical, scientific, and lay reader.

Reviewed by EDWIN CLARKE, M.D , F.R.C.P., Director of the Wellcome Institute for the History of Medicine, London, England.

JAMES F. GIFFORD, JR. *The Evolution of a Medical Center.* A History of Medicine at Duke Universitiy to 1941. Durham, N.C.: Duke University Press, 1972. viii + 249 pp. Ill. $8.75.

Numerous historiographical lenses are used in viewing the details of American medical history. In this instance, an oil immersion lens is used to portray the intricate network of events pertinent to the development of another American medical school. A reader must be willing to accept the tediousness of fine adjustments and occasional globules of distortion. Otherwise, the scholarly techniques are impeccably sound and the resulting views sharp and clear.

In writing the first of two volumes about the development of health care institutions at Duke University, Gifford studied numerous secondary sources, analyzed unpublished manuscripts and records, and gathered additional information with personal interviews.

He begins by scanning the social features of Durham, North Carolina, between 1865 and 1889. Although this attempt to locate the Duke institutions within the social setting of Durham is laudable, it is unfortunate that evolving Durham is seldom reviewed in the subsequent eight chapters. Chapter two is entitled "The American Medical Revolution: Medicine at Trinity? 1889-1923." By no stretch of one's imagination can the story about medical education at Trinity College between 1889 and 1923 be used to exemplify the principal characteristics of the reforms in American medical education between 1889 and 1923. These reform measures cannot be blamed for the five unsuccessful attempts to establish a medical school in Durham. Indecisiveness, civic jealousies, and lack of funds were instrumental in these failures. It is noteworthy that President Few and Trinity's trustees recognized the model of the Johns Hopkins University Medical School and refused to create an inferior institution.

Part I of the book also includes the establishment of the Duke Endowment, the selection, in 1927, of Wilburt C. Davison as the first dean of the new medical school, and the activities of Davison in selecting the faculty and in constructing the hospital and medical school buildings.

In the summer of 1930, the hospital and schools began functioning, and the events of another decade are recounted in Part II. In this part, there is a definite improvement in focus. Although the title of chapter eight does not accurately reflect a discussion of hospitals, the overall image of Part II is striking, with scenes that include the financing of health care delivery, the training of health professionals, the explorations of laboratory investigators, and the emergence of Duke University Medical School and Hospital as a regional medical center by 1941.

World War II produced numerous dislocations in this regional medical center, events that will be described in a projected second volume.

Reviewed by CHESTER R. BURNS, M.D., Ph.D., James Wade Rockwell Assistant Professor in Medical History, Institute for the Medical Humanities, University of Texas Medical Branch, Galveston, Texas.

WILLIAM FREDERICK NORWOOD. *Medical Education in the United States before the Civil War.* New York: Arno Press & The New York Times, 1971. xvi+487 pp. $21.00.

When Williaim F. Norwood, who for many years has been professor of the history of medicine at Loma Linda University in California, published this excellent history of medical education in the United States in 1944, it was hailed as a landmark work. Genevieve Miller, reviewing it in the *Bulletin* (1945, *17*:530-32) called it a book of "the highest quality which will be the basic book on its subject for some time to come." Little did anyone suspect then that nearly thirty years later, the book still stands virtually alone. Not only has relatively little appeared about medical education before the Civil War, the period that Norwood covered so thoroughly, but no one has taken up volume two, or the story from 1865 to the 1960s. This is a pity, because we sorely need such a work. The ahistorical charges that are being levelled at poor Abraham Flexner, for instance, are sufficient evidence for the gap.

When I reread Norwood's book for this review, I was impressed by the breadth and depth of his scholarship as well as by the vast amount of information the book contains about medicine in America. I have often used the book to look up facts about one or another school. But there is much more here. There is a great deal of information about doctors and medical practice in the American colonies, chapters on medical legislation and medical societies, the preceptorship, military medicine, the cost of medical training, as well as much about the curriculum and its contents. In addition to all this, we are rewarded with one of the best bibliographies available. Unfortunately, because it is only a reprint, not a new edition, items appearing after the original publication are not included. While not a large body of work on medical education has appeared since then, George Corner's history of the Universiity of Pennsylvania's medical school, Whitfield Bell's *John Morgan,* and David Cowen's Rutgers-Geneva story are notable exceptions. But much has been written about medicine in America in general, also, of course, not cited here. Yet, it is probably safe to say that all of the work done since 1944 merely adds to and extends Norwood, it doesn't materially change his interpretations. Thus, it is a great pleasure to welcome this book back into print.

Reviewed by GERT H. BRIEGER, M.D., M.P.H., Ph.D., Associate Professor of Medical History, Duke University Medical Center, Durham, N.C.

THE BULLETIN RECENTLY RECEIVED*

Otto Baur. *Der Mensch-Tier-Vergleich und die Mensch-Tier-Karikatur.* Eine ikonographische Studie zur bildenden Kunst des neunzehnten Jahrhunderts. Inaug Diss. Köln, 1973. 348 pp.

Amariah Brigham. *Remarks on the Influence of Mental Cultivation and Mental Excitement upon Health* (2d ed., 1833) and *Observations on the Influence of Religion upon the Health and Physical Welfare of Mankind* (1835). Facsimile Reproductions with an Introduction by Ernest Harms. Delmar, N.Y.: Scholars' Facsimiles & Reprints, 1973. xvi, 130, 331 pp. Ill. $25.00.

Saul Nathaniel Brody. *The Disease of the Soul: Leprosy in Medieval Literature.* Ithaca and London: Cornell University Press, 1974. 223 pp. $9.50.

John Camp. *Magic, Myth and Medicine.* New York: Taplinger Publ. Co., 1973. 192 pp. Ill. $8.50.

Antonio de Gimbernat. *Oración inaugural sobre la importancia de la anatomia y la cirugía* (1733). Valencia, 1971 (Cuadernos Valencianos de Historia de la Medicina y de la Ciencia, XI, Serie B). xxxiii pp.

Paul Ghalioungui. *The House of Life (Per Ankh): Magic and Medical Science in Ancient Egypt.* Amsterdam: B M. Israël, 1973. x+198 pp. Ill. Hfl. 48.

Felix E. Goodson. *The Evolutionary Foundations of Psychology A Unified Theory.* New York: Holt, Rinehart and Winston, 1973. xi+228 pp.

Luis S. Granjel. *La frenología en Espana (vida y obra de Mariano Cubi).* Salamanca, 1973 (Cuadernos de Historia de la Medicina Española. Monografías, XXIV). 74 pp. Ill.

——— *Publicidad therapéutica en la España de "Entreguerras."* Salamanca, 1974 (Cuadernos de Historia de la Medicina Española, Monografías, XXVI). 153 pp. Ill.

The History and Philosophy of Knowledge of the Brain and Its Functions. An Anglo-American Symposium, London, July 15th-17th, 1957. Amsterdam: B. M. Israël, 1973. x+272 pp. Ill. Hfl. 48.

Heinz Hürzeler. *Robert Whytt (1714-1776) und seine physiologischen Schriften.* Zürich: Juris Druck & Verlag [1973] (Zürcher medizingeschichtliche Abhandlungen, N.R., Nr. 96). 85 pp. Fr. 18.

Osman Karamehmedovic. *Ernst Tavel (1858-1912), Bakteriologe und Chirurg in Bern* Bern-Stuttgart-Wien: Verlag Hans Huber, 1973 (Berner Beiträge zur Geschichte der Medizin und der Naturwissenschaften, N.F., Bd. 7). 71 pp.

Huldrych M. Koelbing. Monica Schär-Sent, Antoinette Stettler-Schär, and Hans Trümpy. *Beiträge zur Geschichte der Lepra* (with summaries in English). Zürich: Juris Druck & Verlag, 1972 (Zürcher medizingeschichtliche Abhandlungen, N.R., Nr. 93) 105 pp. Ill. Fr. 24.

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

Francisco López de Villalobos. *El sumario de la medicina con un tratado de las pestiferas bubes* (Introducción, edición y notas de Maria Teresa Herrera). Salamanca, 1973 (Cuadernos de Historia de la Medicina Española. Monografías, XXV). 294 pp. Ill.

Richard H. Mead. *In the Sunshine of Life. A Biography of Dr. Richard Mead, 1673-1754.* Philadelphia: Dorrance & Co., 1974. 196 pp. Ill. $7.95.

Henry Miller. *Medicine and Society.* New York: Oxford University Press, 1973. 87 pp. $3.00.

Jaroslav Nemec. *International Bibliography of the History of Legal Medicine.* Bethesda, Md.: National Library of Medicine [1973] (DHEW Publ. No. (NIH) 73-535). viii+224 pp. $5.50 ($6.90 foreign).

Noel Poynter. *Medicine and Man.* Baltimore, Md.: Penguin Books, 1973. vi+196 pp. $2.25 (paperbound).

Douglas H. Robinson. *The Dangerous Sky. A History of Aviation Medicine.* Seattle: University of Washington Press, 1973. xxiv+292 pp. Ill. $15.00.

Hedwig Schleiffer (Comp.). *Sacred Narcotic Plants of the New World Indians: An Anthology of Texts from the 16th Century to Date.* New York: Hafner Press, 1973. 156 pp. Ill. $5.95.

Martin Schrenk. *Ueber den Umgang mit Geisteskranken. Die Entwicklung der psychiatrischen Therapie vom "moralischen Regime" in England und Frankreich zu den "psychischen Curmethoden" in Deutschland.* Berlin-Heidelberg-New York: Springer-Verlag, 1973 (Psychiatry Series, Bd. 10). ix+194 pp. Ill. $44.00.

Rudolph Siegel. *Galen on Psychology, Psychopathology, and Function and Diseases of the Nervous System. An Analysis of His Doctrines, Observations and Experiments.* Basel-New York: Karger, 1973. ix+310 pp. $42.00.

Michael M. Smith. *The "Real Expedición Maritima de la Vacuna" in New Spain and Guatemala.* Philadelphia: American Philosophical Society, 1974 (Transactions of the American Philosophical Society, N.S., vol. 64, pt. 1). 74 pp. $4.00.

Marion Spaude. *Eugen Albert Baumann (1846-1896). Leben und Werk.* Zürich: Juris Druck & Verlag, 1973 (Zürcher medizingeschichtliche Abhandlungen, N.R., nr. 98). 46 pp. Ill. Fr. 10.

Perry A. Sperber. *Drugs, Demons, Doctors and Diseases.* St. Louis. Warren H. Green, 1973. x+294 pp., $15.00.

Thomas B. Turner. *Heritage of Excellence. The Johns Hopkins Medical Institutions, 1914-1947.* Baltimore & London: The Johns Hopkins University Press, 1974. viii+645 pp. Ill $17.50.

Alexander Walker. *Documents and Dates of Modern Discoveries in the Nervous System.* A facsimile of the original edition, London, 1839, with an introduction by Paul F. Cranefield. Metuchen, N.J.: Scarecrow Reprint Corp., 1973 (New York Academy of Medicine, History of Medicine Series, no. 40). xi+xii+172 pp. $7.50.

Washington University School of Medicine Library. *Archives Procedural Manual.* St. Louis: Washington University School of Medicine Library, 1974. v+118 pp. Ill. $5.00.

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CYNICISM AND MEDICINE

FRIDOLF KUDLIEN

That striking phenomenon which L. Edelstein[1] has called "the Christian glorification of disease" prompts us to ask whether this is indeed a purely Christian attitude. Bearing in mind the well-known fact that Stoic philosophy contributed greatly to early Christian thought, it would be logical to seek for parallels there (as we shall below). But we must not forget the less reputable but closely allied Cynic movement which especially through the so-called "Cynic/Stoic diatribe"—a type of public speech or sermon—influenced the Christian practice of preaching. An added incentive to the study of Cynic attitudes is the fact that Dudley's brilliant analysis[2] does not pay particular attention to our present theme.

What was the Cynics' attitude to health and disease and the physician's rôle in human society? One should not think of it as being a highly characteristic one. When speaking of these things the Cynics, like the Stoics, often reflected moral commonplaces. Furthermore, since Cynicism as such never formed a genuine philosophical school—

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[1] See his "The distinct Hellenism of Greek medicine," in *Ancient Medicine*, ed. O. and C. L. Temkin (Baltimore: The Johns Hopkins Press, 1967), p. 387.

[2] D. R. Dudley, *A History of Cynicism* (Hildesheim: Georg Olms, 1967 [repr.]).

it was, in fact, "impatient of theory" [3]—we should hardly expect its adherents to speak with one voice. Rather, we can expect two diametrically opposite attitudes. The first is a radical re-evaluation of health, one of the chief traditional values,[4] in line with the Cynics' general re-evaluation of accepted values.[5] Prizing above all *autarkeia* or personal independence in every possible respect, the Cynics could logically cease to view health as a good in itself and disease, correspondingly, as an evil. Secondly, an opposite reaction would come from the Cynics' emphasis on a simple life according to nature: one would expect to find the Cynics propagating gymnastics and dietetics, for the restoration and preservation of good health. We do, in fact, find just these two conflicting attitudes in Cynic "philosophy"—a conflict due in part to a certain evolution in its thinking.[6]

We can begin with Diogenes of Sinope, the founder of Cynicism (his main activity is to be dated from about 350 B.C. on). His standpoint seems to have been the following: The very existence of medicine and physicians (who are in the same company, here, with steersmen and philosophers) can make us believe that "man is the most sensible of all animals" (whereas the existence of some other professions such as that of prophets, for instance, may provoke the opposite judgment).[7] This apparently follows partly a Sophistic [8] and partly a Socratic tradition according to which it is the skill of the craftsman that matters particularly in human society.[9] Furthermore, Diogenes is said to have highly appreciated gymnastic training. On the one hand, this should be used, as he occasionally recommends, not for producing athletes but only for "red colour's" sake, and for bodily health.[10] On the other hand, Diogenes held gymnastics to be important not merely for bodily health, but also for "arriving at virtue"; in this connection, he stated that physical and mental training are interdependent, and that "good health and strength are equally included among the essential things for the soul as for the body."[11] On the whole, this looks definitely as if body and soul were, for Diogenes, as they had been for Pythagoras (in flat opposition to Socrates and Plato),[12] of absolutely equal rank. We may also say with some certainty that Diogenes and his circle stressed, in this connection, the importance of right, i.e. natural, nurture, especially when attacking (as they constantly did) the failures of luxury.[13] Apparently, nurture of the "thinnest" kind came near to "divine" nurture and was therefore no hindrance for spiritual activities,[14] while it helped, on the other hand, to approach the state of "most exact health" (including that of the body), just as Pythagoras had recommended.[15] In view of such weighty evidence in favor of health and physicians, we may rightly assume that Diogenes, when attacking people who prayed for health and in the same breath "gormandized against health,"[16] did not principally mock the anxious desire for health (as manifested in such prayers) but merely wished to reveal the self-contradictory behaviour of human beings.

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[3] *Ibid.*, p. xi.

[4] See E. Schwarz, *Ethik der Griechen*, ed. W. Richter (Stuttgart: K. F. Koehler Verlag, 1951), pp. 59 ff., and E. Kornexl, *Begriff u. Einschätzung der Gesundheit des Körpers i. d. griech. Literatur von ihren Anfängen bis zum Hellenismus.* Commentationes Aenipontanae, vol. XXI (Innsbruck: Universitätsverlag Wagner, 1970), *passim.*

[5] Dudley (n. 2 above), p. xi.

[6] A few words about the inconsistency in the Cynic attitude towards health are to be found in Kornexl (n. 4 above), p. 104 f.

[7] See Diogenes Laertius, *Lives of Philosophers,* bk. VI, § 24.

[8] See F. Wehrli, *Theorie u. Praxis.* Gesammelte Schriften z. antiken Gedankenwelt (Zürich-München: Artemis Verlag, 1972), p. 28.

[9] Cf. Dudley (n. 2 above), p. 27. For the Cynics' positive attitude to medicine as a craft, see also A. C. Bayonas, "Travail manuel et esclavage d'après les Cyniques," *Rendiconti dell'Istituto Lombardo,* 1966, *100*: 383-388.

[10] See Diogenes Laertius (n. 7 above), § 30.

We can then say that Diogenes valued health and, accordingly, medicine. As for his personal pupils, they seem to have shared his standpoint: Onesicritus praised an idealised Indian people and country "for their longevity . . . and for their simple and healthful life" and because "they cultivate no science except that of medicine."[17] When the Indian Gymnosophists (an ascetic sect) told their visitor Onesicritus that, in their opinion, Diogenes himself was still too much "subject to the law" (and not to nature) and that bodily disease is a "most abominable thing" which one may eventually get rid of by suicide, this doubtless marks a clear difference of standpoint between them and the early Cynics.[18] And Crates, though a cripple, practised gymnastics as recommended by his master.[19]

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[11] *Ibid.*, § 70; cf. also Dudley (n. 2 above), pp. 33 and 217.

[12] See Wehrli (n. 8 above), p. 200.

[13] See Dudley (n. 2 above), p. 219 f. For a direct reference of Diogenes to the medical importance of low diet, see Kornexl (n. 4 above), p. 103.

[14] See P. Wendland, "Philo u. d. kynisch-stoische Diatribe," in his *Beiträge z. Geschichte d. griech. Philosophie u. Religion* (Berlin: G. Reimer, 1895), p. 13.

[15] See Wehrli (n. 8 above), p. 201.

[16] See Diogenes Laertius (n. 7 above), § 28

[17] See Dudley (n. 2 above), p. 40.

[18] See F. Jacoby (ed.), *Die Fragmente der Griech. Historiker,* pt. II, sect. B (texts) (Leiden: E. J. Brill, 1962 [repr.]), p. 729, ll.13 ff. (Onesicritus fr. 17).

[19] See Dudley (n. 2 above), p. 47.

On closer scrutiny, however, the attitude of the earliest Cynics was sufficiently ambivalent as to admit of future development. On the one hand, the "toils" or *askesis* (forming an essential part of Cynic life) served for Diogenes the purpose of "overcoming everything."[20] This could also be understood as if such an "ascetic" training was to lead to an indifference in regard to diseases, for instance. Gymnastics and frugal diet would then not primarily serve the restoration of bodily health, which would itself become not so much an unquestioned value but rather something "indifferent," too. In addition, there was a particular Socratic tradition[21] according to which the wise man should make himself independent from his "flesh." Therefore, one had to fight against all *lypai* (pains, including diseases) that as such nail the soul to the body. Here we meet with the "dualism" which later on will attract our attention again, and which tends, when separating soul and body, towards a devaluation of the latter. One of the crucial problems was this: How could one get rid most effectively of those "pains" which are normally outside our control? The answer of the Socratic Antisthenes, a precursor of Cynicism, was that one should use toils as strictly as possible. Here, one may notice at least a bit of that "ascetism" which does not so much wish to *heal* bodily ailments but which rather *despises* or disregards them. Certainly, some Cynics would not accept this consequence and would especially reject the idea that pain can become, or actually is, an "indifferent" thing. Indeed, one of the early Stoic philosophers, Dionysius "the Renegade," refused to accept the truth of this doctrine when he himself suffered severely from a painful disease.[22] Such possible backsliding would rather support than weaken the rigorists among Stoics and Cynics as well, and cause them to keep their own strict course.

It is against such a background that we can observe a real change in the Cynic attitude. As it seems, this took place in the course of the 3rd century B.C., certainly not without contacts with, or influences from, Stoicism. For Bion of Borysthenes (who may have died ca. 260 B.C., as an old man), body is a *somation* which we may happily leave behind us if it is "worn-out like a cloth."[23] This is a remarkable statement. Even if it may well fit into the Cynic efforts at *autarkeia,* it bears some peculiar features. First, one must remember, in this connection, that in a discussion as portrayed by Xenophon[24] which is concerned with the duties and tasks of the best ruler, it is said that an army certainly needs good physicians. But even the best physicians are only like "menders of torn clothes" since they were accustomed to treat diseases which already exist as such, whereas the true ruler must try to prevent the very rise of diseases. This doubtless corresponds to philosophical ideas with which also Cynicism was well acquainted.[25] Philosophers like the Cynics claimed to be "doctors" in a virtually analogous way since they had to offer a "preventive medicine" for the soul. The regular physician, even if only a "mender of torn clothes," may still belong to the category of good craftsmen in which he was placed by Diogenes. But here, all depends on the emphasis. And it certainly means a shift of emphasis whether one praises such craftsmen (as Diogenes did) or devaluates the subject they are concerned with—in this case, the body (as Bion did). For the term "somation" as used by Bion has clearly derogatory connotations. In just this sense, we shall meet it again below.

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[20] *Ibid.*, p. 33.

[21] For this and the following, see *ibid.*, p. 12 f.

[22] For Dionysius see [apart from Dudley (n. 2 above), p. 13, n. 1] J. v. Arnim's article "Dionysius no. 119," in Pauly-Wissowa, *Realencyclopädie d. class. Altertumswissenschaft*, vol. V, pt. 1, 1903, col. 973 f.

[23] See O. Hense (ed.), *Teletis Reliquiae*, 2nd ed. (Tübingen: I. C. B. Mohr, 1909), p. 15, l.14 and p. 16, l.7; cf. also Dudley (n. 2 above), p. 67.

When looked at from this point of view, Bion's statement also certainly touches the dualism, i.e. the devaluation of the body in favor of the soul, as it spread so remarkably from the 3rd century B.C. on (we shall come back to that immediately). To sum up, one may be justified in saying that Bion—though his "philosophy" as a whole was certainly scraped up too hastily and in a very cursory fashion[26]—has put forward, in this regard, something new.[27] This is in accordance with Dudley's[28] observation that sometimes "tendencies inherent in earlier Cynicism were found when developed to lead to very divergent conclusions." Here we may also stress the fact that Bion advised people to "adapt yourself to conditions."[29] This might possibly mean that, in regard to diseases, it was not necessary to do much since death was not so bad, after all.[30]

But it is time to cast a look upon Stoic utterances about health and disease. From the 3rd century on, Cynic and Stoic philosophy are closely allied to each other, be it in conformity or rivalry. Now there were, for our theme, two opposite standpoints held by Stoics, and there was, between them, a comparatively broad spectrum of nuances. First, we have the statement of Zeno, the founder of Stoicism, that health as well as disease is an "adiaphoron," an "indifferent" thing.[31] More explicitly, health as such is not a "good"[32] and diseases as such (including bodily pains and crippledness) "must not be avoided, and are not detrimental."[33] Consequently, one must not deliberately seek for health, the less so since this is outside our control.[34] But there were apparently Stoics who felt somewhat uneasy, in this regard. Thus, a different standpoint came up that is characterised by the following statements: Health is "worthy of some estimation."[35] It is to be classified as a *proēgménon,* i.e. something that "has a value."[36] Moreover, for at least some Stoics, the *proēgména* "are close in a way to the character of the goods" which at least means that, among the "indifferent things," they are to be "preferred" to their bad counter-parts, the *apoproēgména.*[37] Hence, one might expect that there was only one step toward the statement that health itself *is* actually a "good"—a step which, as we are told, was taken by Posidonius.[38] Consequently, followers of this standpoint were to admit that "there are reasons enough for rejecting a disease," so that the phenomenon of disease was to be grouped among the *apoproēgména.*[39] As for the dualism, we observe the same split of opinion in Stoicism: for one group of the Stoics, body and soul were not decisively interdependent, so that one could say that the soul is not affected by a bodily disease.[40] Here, the soul was usually given a clear preference to the body whose estimation, then, became necessarily a lower one. Accordingly, medicine was defined as a "technē for the ill body" (whereas philosophy was to be concerned with the soul). As such, medicine was then nothing but a *meson,* something not definitely good in and by itself. When the physician as such was defined as "physician of the body" (in contrast to the philosopher), his importance and estimation were correspondingly impaired. For the opposite group of Stoics, body and soul formed an inseparable unity and influenced each other constantly. Though we seem to lack, for this group, an explicit positive statement about medicine and physicians, it goes without saying that the main representatives of the Middle Stoa were positively interested in medicine. One may only recall that the founder of the Pneumatic school of physicians, Athenaeus of Attalia, was a personal pupil of the famous Stoic Posidonius.[41] And we have, for this group, Cicero's (*De natura deorum,* II, 133 ff.) praise of the human body as a perfect work of divine providence—a passage that virtually goes back to Chrysippus.[42]

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[24] See his "Cyrupaedia," bk. I. ch. 6, § 16.

[25] See R. Höistad, *Cynic Hero and Cynic King* (Uppsala: C. Blom, 1948), p. 101 f.

[26] Dudley (n. 2 above), p. 65.

[27] This is to modify Dudley's judgment (n. 2 above, p. 68).

[28] *Ibid.,* p. 59.

[29] *Ibid.,* p. 65 f.

[30] *Ibid.,* p. 67.

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[31] See J. v. Arnim (ed.), *Stoicorum Veterum Fragmenta* (Stuttgart: G. B. Teubner, 1964 [repr.]), vol. I, p. 47, l.25.

[32] *Ibid.,* vol. I, p. 85, l.2.

[33] *Ibid.,* vol. III, p. 35, l.29 f.

[34] *Ibid.,* vol. III, p. 47, ll.7 ff.

[35] *Ibid.,* vol. III, p. 15, l.27 f.

[36] *Ibid.,* vol. III, p. 31, ll.2 ff.

[37] *Ibid.,* vol. III, p. 31, l.18 ff. For a good account of the Stoic doctrine of "adiaphora" and "proegmena" and the difficulties involved, see I. G. Kidd in A. A. Long (ed.), *Problems in Stoicism* (London: Athlone Press, 1971), pp. 150-172.

[38] See L. Edelstein, *Posidonius I: The Fragments,* ed. I. G. Kidd (Cambridge: University Press, 1972), p. 164 (fr. 171-173). However, the interpretation of these testimonies seems to be controversial; see Kidd in Long (n. 37 above), p. 162 f. I prefer Edelstein's interpretation (*Amer. J. Philol.,* 1936, *57*: 308 f.) according to which Posidonius, fr. 170, l.26 f., does not contradict fr. 171.

[39] See v. Arnim (n. 31 above), vol. III, p. 31, l.32 f.

Of what kind was the relation between Stoicism and Cynicism? In the earlier period, Cynic influence upon Stoic philosophy was particularly strong.[43] If there was any criticism or enmity (especially on the part of Cynicism), this was rather concerned with single features in the behavior of Stoic individuals and did not touch the philosophical basis.[44] However, from the Middle Stoa on, a real aversion to Cynicism originated and developed, although this never led to a definite break between the two philosophies.[45] More precisely, it was the "left wing" of the Stoa (called "the most manly Stoicism") which remained essentially influenced by Cynicism.[46]

As for the explanation of this divergent development, our present enquiry may suggest an answer not explicitly discussed by Dudley. Just with regard to the evaluation of body, health, disease and medicine, we find the same discrepancy in Cynicism as in Stoicism. On the one hand, Bion, as we have seen, marks a step forward towards rigorism. Furthermore, one can certainly not follow Dudley[47] in saying that *all* Cynics after Diogenes continued to feel "a particular reverence for doctors" (for details, see below). And it meant also a difference from Diogenes when nurture was no longer held by some Cynics to be a measure of keeping a state of health but came to serve merely as a sort of necessary evil, since it was now simply accepted as keeping someone alive (*diamonē*).[48] Also, the devaluation of the body in favor of the soul made further progress in Cynicism. We shall especially see how such metaphors as "body-prison" or "body-grave," which chiefly stemmed from Socratic-Platonic tradition,[49] were taken over by Cynic-Stoic literature. Correspondingly, (bodily) health was certainly no longer a value or "good" for the more rigorous Cynics, as it failed to be for like-minded Stoics. On the other hand, there was a more liberal wing of Cynicism too. For Teles (who was really a man of compromise),[50] health is to be counted among "those goods which belong to the body," [51] a statement which departs somewhat from the views of Diogenes for whom health included body and soul as well. But it formed a genuine compromise between the (rigorous) body-soul dualism and the (liberal) re-justification of health as propogated by Middle Stoic philosophers and inaugurated by the concept of *proēgména* which enabled Stoic philosophers to avoid "Cynic narrowness and fanatism." [52] In the light of this re-justification of health, it seems particularly interesting that even Middle Stoic philosophers who otherwise rejected Cynicism continued admiring Diogenes,[53] who had esteemed health as an unquestioned "good." Finally, one should not forget that strict rigorism, when concentrating on the devaluation of bodily health and regular medicine and maintaining the "indifference" of disease, could also lead the Cynics into an existential dilemma, so to speak. We are told that Bion, when he was severely ill and saw death before him, came to reject his former doctrines, and even returned to magic medicine.[54] This story may well be a malicious invention. But nevertheless it gives voice to a real dilemma which in the same way occurred to the Stoics (remember Dionysius "the Renegade").

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[40] For this and the following, references are to be found in my paper, "Der Arzt des Körpers und der Arzt der Seele," *Clio Medica,* 1968, *3*: 6.

[41] See Edelstein-Kidd (n. 38 above), p. 16 (testim. 51).

[42] See v. Arnim (n. 31 above), vol. II, p. 328, ll.3 ff.

[43] See Dudley (n. 2 above), pp. 96-102.

[44] See Gerhard in Pauly-Wissowa, *Realencyclopädie,* vol. XI, pt. 1, 1921, col. 306.

[45] Cf. Dudley (n. 2 above), pp. 102 ff.

[46] *Ibid.,* p. 99.

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[47] *Ibid.,* p. 80. Incidentally, when the Cynic poet Cercidas mentions the god Paian, this must not be a reference to medicine. In this context, Paian would rather serve as a common, non-specific "Nothelfer" as he actually did very often (cf. v. Blumenthal in Pauly-Wissowa, *Realencyclopädie,* vol. XVIII, pt. 2, 1942, col. 2341, ll.56 ff.).

[48] Cf. Wendland (see n. 14 above), pp. 8 ff. (esp. p. 10, with n. 4, and p. 13 f.).

[49] The history of these metaphors is pursued at great length by P. Courcelle ("Tradition platonicienne et traditions chrétiennes du corps-prison," *Rév. des Etudes Lat.,* 1965, *43*: 406-443; "Le corps-tombeau," *Rév. des Etudes Anciennes,* 1966, *68*: 101-122).

[50] Cf. Dudley (n. 2 above), p. 122.

[51] See Hense (n. 23 above), p. 22, l.5 f.

[52] See Dudley (n. 2 above), p. 99.

[53] *Ibid.,* p. 103.

We must now turn to the 1st century A.D. and the emergence of Christianity. Some reasons why, in the meantime, Cynicism had apparently ceased to be a conspicuous phenomenon are discussed by Dudley.[55] To be sure, one of the main reasons, as Dudley has pointed out, was that Cynicism necessarily became obsolete after the Stoa had entered the panorama of Greek philosophy and life, simply because the Stoa was a regular philosophy and could as such express more consistently and convincingly all the themes in which Cynicism was also interested. But there is no doubt that Cynicism survived during that period, if only as an essential part in the "left wing" Stoicism, or through that particular literary genre called "diatribe" which was developed by Bion. However, another important reason (not adduced by Dudley) for the eclipse of Cynicism is that its rigorism and emphasis on *autarkeia,* leading to a contempt of nearly all social as well as bodily conveniences, could not be attractive so long as it offered no hope or alternative. To preach the contempt of earthly life is useless if one fails to hold out the prospect of another more desirable world in Heaven. What comfort could be got from Bion's dictum that death is not so bad after all? So, one might expect that rigorous Cynicism would only revive with the spread of belief in a future life. Such ideas, of course, would only find sufficient attention in a period of severe instability, in an "age of anxiety," as E. R. Dodds has called the first centuries A.D.

About 50 A.D., we meet Demetrius the Cynic, who displayed traits of religious colour (he preached total resignation to the will of God).[56] This is certainly no longer an absolute *autarkeia* but clearly points to an increasing importance of religious feeling. Now, we find in the 1st century A.D., among Pagans who were influenced by Cynic/Stoic ideas, many references to the worthless nature of the body. For instance, Seneca has called the body a *corpusculum* (which is, in this context, a literal translation of the Greek *somation*) and has described it as a "prison." [57] In connection with our theme, it is particularly worth remembering that Seneca, who reflected here as elsewhere Cynic/Stoic ideas, had in him much of the preacher too. Indeed, his ideas (and his way of presenting them) were so attractive for early Christianity that he was declared to have been himself a Christian, already in the time of St. Augustine.[58] At the same time, we meet the Stoic philosopher Epictetus, whose sermons were delivered as *diatribai.*[59] Actually, he shared much more than mere form with Cynicism, and he can also be seen to serve as a connecting link with Christianity. First, the "diatribe" as such offered an influential model for the Christian practice of preaching.[60] Secondly, Epictetus' statements about health, disease, medicine and physicians alone can be found again to a considerable degree in Christian literature. Finally, when he says that God had advised Diogenes,[61] this statement (which flatly contradicts the old Cynic attitude to gods and religion) makes it particularly clear how far religious feeling had meanwhile infiltrated Cynicism.

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[54] See Diogenes Laertius (n. 7 above), bk IV, §§ 54 ff.

[55] Ch. VI of his book.

[56] Cf. *ibid.,* p. 127.

[57] See Courcelle (n. 49 above), p. 414, n. 2.

As for the evaluation of the body, Epictetus' standpoint, ambiguous as it is, may well reflect the discrepancies in Cynicism and Stoicism. On the one hand, he admits [62] that a Cynic, too, "needs a certain kind of body" which at all events should not suffer from consumption or otherwise look too miserable. But this is evidently said for tactical reasons only. You may then justifiably claim, as Epictetus says, that the Cynic way of life "does not injure even the body." In his opinion, Diogenes "used to go around with a radiant complexion" only because he wanted "to attract the attention of the common people by the very appearance of his body." How far remote this is from the true motives of Diogenes' attitude! With its rather ironical note, Epictetus' remark points quite frankly to the fact that the necessity of having a more or less sound body was admitted only half-heartedly, so that the contempt in the background was scarcely concealed. We must then not be surprised to find Epictetus explicitly devaluing the body which is occasionally called "the most unpleasant and dirtiest thing of all"[63] or (a bit more conciliatory) a "small sumpter-mule" which one may easily leave behind.[64] Or, it was called a *somation,* being "by nature a corpse."[65] Apart from the fact that the usage of diminutive forms is typical for the "diatribe,"[66] Epictetus makes it finally clear that Bion's usage of *somation* must indeed have been derogatory and that *somation* was a key word in Cynicism. This becomes clear also from the identification of body and corpse, a topic that belongs to the "body-grave" tradition and for which there are further references in Epictetus.[67] This devaluation of the body must be seen, of course, against the background of dualism. Hence, it may be well understandable that, for Epictetus, "it is more necessary to cure the soul than the body, since it is better to die than to live in a bad state."[68]

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[58] See O. Rosbach in Pauly-Wissowa, *Realencyclopädie,* vol. I, pt. 2, 1894, col. 2247, ll.9 ff.

[59] We have used the edition (with an English translation) of W. A. Oldfather, *Epictetus: The Discourses as Reported by Arrian, the Manual, and the Fragments,* 2 vols. (London and Cambridge, Mass.: W. Heinemann and Harvard University Press, 1966-7. The Loeb Classical Library). Cited hereafter as Oldfather.

[60] Cf. U. v. Wilamowitz, *Antigonos von Karystos* (Berlin: Weidmann, 1881. Philolog. Untersuchungen, 4), pp. 292 ff. (esp. p. 313 f.), and P. Wendland, *Die hellenistisch-römische Kultur,* 2nd ed. (Tübingen: I. B. C. Mohr, 1912), pp. 75 ff. (esp. pp. 91 ff.).

[61] See "Discourses," bk. III, ch. XI, § 19 (Oldfather, vol. II, p. 129).

[62] *Ibid.,* bk. III, ch. XXII, §§ 86 ff. (Oldfather, vol. II, p. 161).

What did early Christians think about body and dualism? There was an almost fantastic "contempt for the human condition and hatred of the body"[69] which could lead Christians either to declare that the body as such is "bound to dissolution" and therefore lacks any importance,[70] or to call it (like Epictetus) "clay and gore" and "a filthy bag of excrement and urine."[71] This was naturally said with the strongest favor of the soul. We shall soon see that dualism was pointed to by early Christians chiefly in their discussion of the rôle of regular medicine. Here we must only add that the Christian attitude to the body was in no way unanimous. Side by side with, or rather as a reaction against, the "hatred of the body," we find in the writings of the early Church Fathers the argument that "in itself, bodily nature is not involved in evil."[72] That may be an allusion to the Stoic doctrine of "indifference" and its possibly favorable aspects. Moreover, one could declare the body to be a "vessel" or "cloth" for the soul. Therefore the body must be carefully dealt with, so that it can "fulfil its obligations to God and the Church."[73] This is the more interesting since the "body-cloth" metaphor was originally used in a negative sense (see above). The Christian usage shows that there were also very positive aspects in it.

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[63] *Ibid.,* fr. 23 (Oldfather, vol. II, p. 467). If here, Epictetus has to admit that "I must serve God . . . for that reason, I remain and endure to wash this miserable paltry body," this reminds us of Christian argumentation (see below) but seems more forced than this.

[64] *Ibid.,* bk. IV, ch. I, §§ 79 ff. (Oldfather, vol. II, p. 269 f.).

[65] *Ibid.,* bk. III, ch. X, § 15 (Oldfather, vol. II, p 77).

[66] See R. Helm, art. "Kynismus," in Pauly-Wissowa, *Realencyclopädie,* vol. XII, pt. 1, 1924, col. 21 ll.50 ff.

[67] See Courcelle (n. 49 above), p. 105 notes 1 and 2.

[68] "Discourses," fr. 32 (Oldfather, vol. II, p. 475).

[69] See E. R. Dodds, *Pagan and Christian in an Age of Anxiety* (Cambridge: University Press, 1965), p. 35.

[70] For references, see Kudlien (n. 40 above), p. 9.

[71] See Dodds (n. 69 above), p. 29.

[72] *Ibid.,* p. 29, n. 3 (end).

Bodily health and disease had likewise to suffer from a permanent divergent evaluation. Epictetus says that the Cynic is proud of being healthy just in spite of the fact that he fails to possess, or to belong to, anything or anyone ("and yet you see how healthy I am").[74] This clearly points to the old *autarkeia,* as far as that ideal included a positive attitude to health. Less positively, Epictetus alludes to health in the Stoic sense of "adiaphoron." Is it as such a "good"? No; but to be healthy "in a good way," this is "good."[75] This could very easily lead to negative conclusions. Health must not be sought for, since it is in any case outside our control[76] (which follows an older Stoic statement). Frankly, what "good" would lie in health? And what is really bad in being ill? Illness will the sooner bring the soul to get rid of the body—a desirable goal, after all.[77] Moreover, illness can in itself be useful.[78] This also resembles an older Stoic statement. But in this outspoken form, Epictetus' remark offers a true connecting link with the Christian "glorification of disease."

What had early Christians to say about health and disease? Of course, (bodily) health and its normally high reputation must suffer as soon as one stresses excessively the importance of the soul. Furthermore, it must loose its rank as soon as disease is conceived of as a god-sent thing, serving positive purposes. Against such a background, bodily health might become a "perniciosa sanitas" which averts men from the salvation of the soul, whereas disease could become a "salubris infirmitas."[79] At least one could follow some Stoics in saying that health of the soul was more important than that of the body—a standpoint shared, if not by all, then by most of the early Christians.[80] As for disease, one could say that it served four positive purposes: (a) it punished the sinner, to make him a better man; (b) it tested the just man; (c) it preserved one from sin; (d) it anticipated the Last Judgment.[81] But this apparently did not mean that one should simply praise disease. To be sure, some Christians may have shared this opinion. But, though all Christians held a disease (like health) of the soul to be more important than a bodily disease,[82] they could still care for remedies against bodily diseases. We shall soon discuss their attitude to medical practice. Here we may only add that a Christian who shared in general the attitude to bodily diseases, but had really experienced what they could mean, might frankly confess that life is miserable without health.[83] This shows that also here, actual experience could modify an otherwise perhaps too rigorous standpoint. So, the Church Fathers could say that health is indeed a "good" (a bodily one, as one might hasten to add) and that one should enjoy bodily health, "in accordance with one's nature." [84]

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[73] For references, see H. Schadewaldt, "Die Apologie der Heilkunst bei den Kirchenvätern," *Veröffentlichungen d. Internat. Gesellschaft f. Gesch. d. Pharmazie,* 1965, *26*: 123.

[74] "Discourses," bk. IV, ch. VIII, § 31 (Oldfather, vol. II, p. 387).

[75] *Ibid.,* bk. III, ch. XX, § 4 (Oldfather, vol. II, p. 119).

[76] *Ibid.,* bk. IV, ch. I, § 76 f. (Oldfather, vol. II, p. 269).

[77] *Ibid.,* bk. III, ch. X, § 14 (Oldfather, vol. II, p. 77).

[78] *Ibid.,* bk. III, ch. XX, § 4 f. (Oldfather, vol. II, p. 119).

[79] See Schadewaldt (n. 73 above), p. 126.

[80] See G. Müller, "Arzt, Kranker u. Krankheit bei Ambrosius v Mailand," *Sudhoff's Arch.,* 1967, *51*: 211.

Finally, what was the attitude of Epictetus and the early Christians to physicians and medicine? Epictetus not seldom mentions the medical profession with some respect,[85] though the physician is categorically restricted, in his opinion, to the sphere of the body.[86] However, as an adherent of the Cynic *autarkeia,* Epictetus objected to the patient's dependence on the physician, and the latter's unduly influential position.[87] So we may say once again that Epictetus' attitude is ambiguous. Among early Christians, one will find some people whose hostility against physicians and medicine is almost outrageous. Tertullian (an African) attacks not only Pagan physicians like Hippocrates and Herophilus (in the *De anima*), but medicine per se.[88] He was apparently a difficult case for the early Church, a rebel, or even a heretic, who was himself full of radicalism as well as of contradictions.[89] Furthermore, there was Markion, whose hatred was so great that he even deleted in the Bible those words which refer to St. Luke as "the beloved physician." [90] But

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[81] *Ibid.,* p. 213.

[82] See Schadewaldt (n. 73 above), p. 124.

[83] For St. Basilius' respective utterances, see P. Lain Entralgo, *Heilkunde in geschichtlicher Entscheidung* (Salzburg: Otto Müller Verlag [1950]), p. 112 f.

[84] For Ambrosius, see Müller (n. 80 above), p. 211, with n. 96.

[85] See *e.g.* "Discourses," bk. III, ch. XVI, § 12 (Oldfather, vol. II, p. 109).

[86] *Ibid.,* bk. III, ch. II, § 1 (Oldfather, vol. II, p. 29).

[87] *Ibid.,* bk. III, ch. X, §§ 13 ff. (Oldfather, vol. II, p. 77).

[88] See Schadewaldt (n. 73 above), p. 126 f.

[89] See H. Koch, "Tertullianus no. 1," in: Pauly-Wissowa, *Realencyclopädie,* vol. V A, pt. 1, 1934, col. 833 ff.

[90] See Schadewaldt (n. 73 above), p. 127.

Markion was one of the most famous Christian heretics. Furthermore, Tatian and Makarios argued that one is not allowed to cure diseases since they are sent from God, so that to take medicine would make you an atheist.[91] Makarios was an Egyptian monk and thus belonged to a community of very hard-boiled Christian rigorists, whereas Tatian was the leader of an ascetic sect called "Encratites" which "stood for long on the border-line between orthodoxy and heresy." [92] This sect was explicitly said to be "more Cynic than Christian," [93] and it too was certainly very rigorist. But even here, we meet with some mitigating tendencies with regard to the evaluation of health, disease and medicine. When actually ill, even these people called for, and accepted, the help of physicians.[94] Moreover, a few of them conceded that one was allowed to indulge in bodily instincts, as far as necessary to keep oneself *healthy*.[95] This reminds us of the Cynic *diamonē* which, however, only served the purpose of keeping one *alive*. In so far, early Christian rigorism, in its mitigated form, even surpassed the mitigating tendencies in Cynicism. But this fits well into the development of early Christianity. Granting that it is correct to say that the early Church tolerated Cynicism,[96] one should still add that it was the more liberal tendency of Cynicism which is concerned. The same holds good for dualism as mirrored in the rivalry between the body-physician and the soul-physician.[97] Radicalists might argue that the traditional physician lacked any importance or justification. But most Christians would rather concede that he, too, has an important function in human society, and that medicine was to be accepted as long as its representatives did not forget that its success entirely depended on the will of God, and that the soul was ultimately more valuable than the body.[98] Also the fact that medicine had much to do with philanthropy played an effective rôle in making it more acceptable for Christians. One should also not forget that some

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[91] *Ibid.*, p. 126. For Tatian and Makarios and some other pertinent references, see also H. J. Frings, *Medizin u. Arzt b. d. griech. Kirchenvätern bis Chrysostomos* (Bonn: Rhein. Friedr.-Wilhelms-Univ., 1959. Diss. phil.), pp. 14-16.

[92] See H. Chadwick, "Enkrateia," in Th. Klauser (ed.), *Reallexikon f. Antike u. Christentum*, vol. V, 1962, col. 355 (top).

[93] Cf. Dudley (n. 2 above), p. 174.

[94] For references, see Frings (n. 91 above), p. 16.

[95] See Chadwick (n. 92 above), col. 356 (end).

[96] See Dudley (n. 2 above), p. 174.

[97] For the following, see my "Der Arzt des Körpers" (n. 40 above), pp. 13-15; Frings (n. 91 above), pt. B, § 6; and G. Müller (n. 80 above), pp. 201-209.

[98] See Origenes as quoted by Frings (n. 91 above), p. 17 f.

Christians were themselves physicians.[99] All this contributed to the "apology of medicine" (H. Schadewaldt) which was not so much an apology as a restoration of sound equilibrium.

This last remark does not mean that body-hatred and neglect of health and medicine were marginal things for early Christianity. Dodd's book has made it sufficiently clear that "anxiety" was a wide-spread feeling at that time, and that this prepared the soil on which body-hatred with its implications could grow. However, pursuing the development of the Cynic attitude to medicine and related problems, we have seen that, from Bion on, there were closely allied ideas in Cynicism and Stoicism as well. But generally, despite tendencies which went *some way* to "glorifying disease," common sense prevailed both in Pagan and Christian circles.

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[99] For this, see *e.g.* Lain Entralgo (n. 83 above), p. 115 f.

ROMANTICIZING THE ANCIENT MEDICAL PROFESSION: THE CHARACTERIZATION OF THE PHYSICIAN IN THE GRAECO-ROMAN NOVEL *

DARREL W. AMUNDSEN

The references to physicians most commonly quoted from Greek and Latin authors of the period of the late Roman Republic and the Empire are decidedly slanted against the medical profession. When the physician is the target of the epigrammatist's biting wit [1] or the object of the satirist's venom,[2] he naturally fares poorly. The writers of fable and folktale were inclined to weave tales illustrating the foibles of character-types and physicians were certainly not left unscathed.[3] The ancient historians found physicians noteworthy primarily when they helped change the course of history by lending their skill to court intrigue.[4] Roman writers of rhetoric employed physicians in their works often in adverse roles, particularly as poisoners.[5] Quasi-scientific writers included in their works attempted exposés of the medical profession in general or specific physicians in particular.[6] Often medical writers surpassed their non-medical critics in their vituperation of fellow physicians. Galen is perhaps the most spectacular in that regard, terming the physicians of Rome as differing from country brigands in only one respect: namely, that the former carry on their thievery in the city.[7]

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[1] *E.g.*, Martial, *Epigrams,* I, 30; I, 47; II, 15; V, 9; VIII, 74; X, 56; X, 77; XI, 71; Lucilius, in *Anthologia Palatina,* XI, 257; Nicarchus, *ibid.*, XI, 112, 113, 114, 116, and 124; Lucian, *ibid.*, XI, 401; Strato, *ibid.*, XI, 117; Callicter, *ibid.*, XI, 118, 119, 121, 122; Palladas, *ibid.*, XI, 280; Agathias Scholasticus, *ibid.*, XI, 382; Ausonius, *Epigrams,* 4, 80, 81; Hierocles and Philagrius, *Facetiae,* 3, 139, 142, 175a, 176, 183.

[2] *E.g.*, Juvenal, *Satires,* III, 58 ff.; XIII, 120 ff.

[3] *E.g.*, Babrius, *Fables,* 75; Phaedrus, *Fables,* I, 14.

[4] *E.g.*, Livy, *History,* XL, lvi, 11; XLII, xlvii, 6; Tacitus, *Annals,* 12, 67, 2; *Agricola,* 43, 2; Capitolinus, *Gordiani Tres,* XXVIII, 5 f., in *Scriptores Historiae Augustae.*

[5] *E.g.*, Calpurnius Flaccus, *Declamationes,* XIII; pseudo-Quintilian, *Declamationes Maiores,* 8; *Declamationes Minores,* 321; Libanius, *Progymnasmata,* VII, 3.

[6] *E.g.*, Pliny the Elder, *Naturalis Historia, passim,* especially Bk. XXIX; Aulus Gellius, *Noctes Atticae,* XVIII, x.

[7] Kühn, XIV, 622. There are, of course, some well-known exceptions to this attitude, such as Cicero's much publicized affection for his physician (*e.g. Epistulae ad Atticum,* XV, 1 a. 1;

The vast majority of classical literature that has survived was written by the educated upper class for their patrons and peers. A notable exception is the genre of prose fiction, the romance or novel, condemned as lacking in literary artistry and didactic motivation, lowbrow literature written for consumption by the lower strata of the literate public.[8] Five Greek or Roman authors of extant prose fiction employ physicians as minor characters:[9] Apuleius in the *Metamorphoses,* Xenophon of Ephesus in his *Ephesian Tale,* Heliodorus in the *Ethiopian Romance,* the anonymous author of *Apollonius, Prince of Tyre,*[10] and Aristainetos, a writer of love stories.[11] The first two date from the second century

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Epistulae ad Familiares XIII, 20) and the younger Seneca's similar sentiments (*e.g., De Beneficiis* VI, 16, 1-4). It should be noted that their affection was the result of a close personal, not merely a professional, relationship.

[8] Cp. P. G. Walsh, *The Roman Novel* (Cambridge University Press, 1970): ". . . fiction was widely regarded as a sub-literary genre, as a disreputable pursuit for men with intellectual talent" (p. 2). Cp. also B. E. Perry, *The Ancient Romances* (Berkeley, Calif.: Univ. of California Press, 1967): "The novel appears first on a low and disrespectable level of literature, adapted to the taste and understanding of uncultivated or frivolous-minded people. As such, it is ignored or despised as trivial by the prevailing literary fashion of the time" (p. 5). The Greek novels that have survived differ from the Latin in that the former were extremely stereotyped and limited in plot. The basic story in the Greek romance involves a young man and woman who fall in love, become separated and while separated endure sundry misfortunes and attacks on their chastity, and finally are reunited with all ending happily. The two extant Latin novels, the *Satyricon* of Petronius and Apuleius' *Metamorphoses* (also known as the *Golden Ass*), bear little resemblance to the Greek romances, tending to be comic in tone and of no fixed plot structure. The writers of Greek romances were naive and wrote for a naive audience. They were not men of social prominence as were Petronius and Apuleius That men of the stature of Petronius and Apuleius tried their hand at prose fiction did not cause their literary peers to hold the genre in higher regard. As Perry comments: "When Macrobius (*Commentarii in Somnium Scipionis,* I, 2, 8) expresses surprise that Apuleius, the professed student of Plato, should have stooped to the writing of fiction, he voices thereby not only the ancient reaction toward such a performance on the part of a self-respecting rhetor, but likewise that of the modern philologian." ("An interpretation of Apuleius' Metamorphoses," *Trans. Am. Philol. Assoc.,* 1926, *57*: 238.)

[9] There are medical references in two novels in which physicians do not appear as speaking characters. See E. T. Sage, "Medicine in the romance of Petronius," *Annals Med. Hist.,* 1935, *7*: 192-196, and A. M. G. McLeod, "Physiology and medicine in a Greek novel: Achilles Tatius' *Leucippe and Clitophon,*" *J. Hellenic Studies,* 1969, *89*: 97-105.

[10] Although the earliest extant version of the *Appolonius, Prince of Tyre,* is in Latin, it is best regarded, in line with the definitions set forth in note 8 above, as a Greek romance composed in Latin or a Latin translation of a work originally written in Greek. See Perry, *The Ancient Romances,* pp. 294 ff., and Walsh, *The Roman Novel,* p 1

[11] Aristainetos' works fall into the category of epistolography, a genre that is distinguished from the romance. The subdivisions of epistolography are quite varied, one being that of love letters, which are actually short love stories in the form of letters. These differ from the romance stylistically but are so closely similar to it in subject matter that the inclusion of Aristainetos in this paper is warranted.

A.D., the second two from the third century A.D.[12] and Aristainetos from the fifth century A.D. The purpose of this paper is to investigate the depiction of members of the medical profession in these novels in order to supplement and enhance our appreciation of the way in which the practitioners of the healing arts were viewed under the Roman Empire.

There are three types of activities in which the physicians in the writings under consideration engage. One is the diagnosis of a specific type of illness. Another is the reanimation of a body thought by all to be dead. The third, which will be addressed first, relates to medical ethics and deontology: namely the reaction of physicians to requests for poison.

In the *Ephesian Tale* of Xenophon of Ephesus, Anthia and her husband, Habrocomes, had become separated and she had fallen into the hands of a Cilician, Perilaos, who was compelling her to marry him. Finding no other means of escaping this impending marriage, she resolved to commit suicide. She accordingly beseeched a certain physician named Eudoxos, a fellow native of Ephesus, who was also in dire straits, to help her. After exacting from him an oath that he would assist her, writhing at his feet she implored him to provide her with poison. She then gave the physician twenty minae of silver and her jewelry and promised to arrange his passage back to Ephesus. Eudoxos, whose name means "a man of high repute," anguished over the problem for a long while. He pitied the girl for her misfortunes, he longed to return to Ephesus and he coveted the money and the jewels. So he agreed and left to get the poison. He returned shortly with a drug, however not a poisonous one, but a soporific, so that no real harm would come to the girl and he could then make arrangements for travel and be on his way. This is the last we see of Eudoxos. Anthia, however, waited to take the drug until just before the marriage was to be consummated. She was then taken for dead and deposited in a funerary chamber where she was later discovered alive by grave robbers.[13]

In this story the physician is placed in an uncomfortable situation where he is torn between ethical constraints and the desire for and need of money. Let us consider the motivating force behind his decision to substitute a soporific for a poisonous drug. Of course, we are dealing with fiction. One could say that the real motivating force was the necessity for the author to keep Anthia alive until she could be reunited with her husband, whom she erroneously assumed to be dead. But regardless of the naivete of the author that is often so evident in this work, Xenophon surely wished to create a situation credible to his audience. Was the physician's motivation his pity for the girl? If Anthia preferred death to her unwilling marriage, would she not also have preferred death to entombment alive thus being thrown into the hands of chance, or perhaps to a fate worse than that which she was seeking to avoid? Since his passage back to Ephesus had been arranged before she was to take the drug, the physician surely was not motivated by fear for his own safety since he would have been well on his way before her apparent death would have been detected; moveover, the act of supplying poison to a suicide was not a punishable crime.[14] Surely his decision was based in part on a moral conviction that to assist the suicide of a young person who was in good health was contrary to the basic precepts of his calling.[15] But the money, jewels and passage to Ephesus proved too tempting to be refused. Accordingly, he accommodated himself without excessively compromising his conscience and comes off as a character by no means to be despised.

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[12] Where the dating of the novels is in dispute, I have followed Perry, *The Ancient Romances*.

[13] III, V ff. I have employed G. Dalmeyda's edition of the Greek text of Xenophon's *Ephesian Tale* (Paris: Société d'Editions "Les Belles Lettres," 1926).

The motif of the substitution of a soporific for a poisonous drug is a commonplace in literature after this time.[16] This is not to imply that the example from the *Ephesian Tale* was either the first in Western literature or necessarily the source for later stories. The following tale from the *Metamorphoses* of Apuleius was, however, the source frequently used for tales written during the Renaissance.[17]

Using the traditional elements of the so-called Potiphar's wife motif,[18]

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[14] There is no evidence that it was a criminal act, under the laws of the Greek city-states, to provide a person who wished to commit suicide with a suitable drug. Although the *Ephesian Tale* is set in the Greek world, it was written during the time of the Roman Empire where this act was not illegal

[15] See below, note 23.

[16] See, *e.g.*, S. Thompson, *Motif-Index of Folk-Literature,* 6 vols. (Bloomington: Indiana University Press, 1955-58), K518.4 and T37.0.1., and D P. Rotunda, *Motif-Index of the Italian Novella in Prose* (Bloomington: Indiana University Press, 1942), K512.4 and K1852.

[17] *E.g.*, Juan Timoneda, *El Patranuelo,* #20.

[18] This is one of the most widely spread themes in folk-literature. For its background in the classical world, see S. Hammer, "De Narrationum Apulei Met. Libro X-^{mo} Insertarum Compositione et Exemplaribus," *Eos,* 1923-24, *26*: 8 ff. S. Trenkner, *The Greek Novella in the Classical Period* (Cambridge University Press, 1958), pp. 64 ff.; M. Braun, *History and Romance in Graeco-Oriental Literature* (Oxford: B. Blackwell, 1938), pp. 54 ff.

Apuleius created a story[19] that he embellished with the addition of the character of a venerable physician whose foresight caused what would otherwise have been a tragedy to end happily. A woman lusted after her stepson. The stepson spurned her advances and she, so wounded, sought to kill him. She sent her slave to procure poison from a physician, had it mixed with wine and awaited the opportunity to administer it to her stepson. Her own son, however, unknowingly drank the potion and was then entombed. She charged her stepson with murder and he was brought to trial. Fortunately for the side of justice, the physician who had sold the drug to the slave was one of the judges. This elderly physician, who is described as having a very high reputation and the greatest influence,[20] waited to address the court until after the innocent stepson had already been found guilty. He then described how the slave had come to him and, offering a very large sum of money (one hundred gold aurei, to be exact), had asked for a quick and efficient poison on the grounds that he was acting on behalf of a sick friend who was suffering terribly from an incurable disease and wished to be released from his agony. Noticing the clumsy and disconcerted nature of the slave's babble, the physician had given him a drug, certain that the slave was up to no good. As soon as this was revealed in court, the slave began to tremble and sweat. He was then subjected to torture to exact a confession, but, when this proved unsuccessful, the physician continued: "When this fiend came bothering me for a poisonous drug, I decided that it would be a betrayal of my profession to supply anybody with the means of murder, since medicine was meant to be used for the preservation, not the destruction, of mankind. Yet I feared that an untimely refusal would provoke him to devise some other way of carrying out the nefarious project on which he was so obviously set—either by buying poison elsewhere, or by having recourse in desperation to the knife or some such weapon. So I gave him a drug, but a sleeping-drug compounded of mandragora, an herb valued for its numbing effect that can hardly be distinguished from the repose of death." Thereupon, all went out to the boy's tomb and, just as the boy's father was raising the coffin's lid, the drugged child began to stir. Then the stepson was acquitted, the stepmother banished, and the slave crucified.[21]

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[19] All the Graeco-Roman novels included in this paper contain what are known as "Milesian Tales." These are stories having no direct effect on the main course of events that are told by characters in the novel. All the material in this paper taken from Apuleius falls into this category, while that from the other authors does not.

[20] Walsh, *The Roman Novel,* is surely misinterpreting when he refers to the physician in this tale as "an old apothecary" (p. 172). The distinction between a *pharmacopola* or *pharmakopōlēs* and a *medicus* or *iatros* is well-known.

The fact that the physician waited until the trial was over before telling of his involvement is, of course, nothing more than an artificial device on the part of Apuleius for prolonging the suspense and should not affect our appreciation of the character. This physician was in a position quite different from that in which Xenophon's Eudoxos found himself. Poison was being sought from Eudoxos by a person wishing to commit suicide whereas the physician in Apuleius' narrative was approached by a third party. If Apuleius' physician had in fact sold poison to the slave and had been identified later as the supplier of the drug, he would have been liable for prosecution under the provisions of the *lex Cornelia de sicariis et veneficis* for homicide.[22] Then the burden would have been on his shoulders to prove that he had no knowledge that the drug was to be used for murder. It was partially this enlightened self-interest that prompted him to substitute a soporific, for it is clear from his speech that, if he had been certain the drug was in fact intended for use in a suicide, he would not have hesitated to supply it.[23]

I shall briefly mention the tale of the one truly degenerate physician portrayed in the novels under consideration. This story, told by Apuleius, involves a wicked woman who wished to kill her husband. He happened to be ill and in need of medical assistance so she went to a notoriously corrupt physician who had become wealthy through his vile practices. For a poison that was guaranteed to be effective, she offered him fifty gold aurei that she would pay to him after he had administered the poison to her husband. When he came to her house, relatives of the patient were present. For the sake of his unanticipated audience, the physician said that the drug he was about to give was never known to fail in alleviating intestinal trouble and carrying off the bile. Just as he was preparing to administer the medicine, the clever woman said that she thought he had better take a drink of the drug himself first so that her husband's relatives would know that there was no poison in it. This way she intended both to rid herself of a witness and an accomplice and to save the fifty gold pieces. The physician, not knowing what to do, imbibed some of the poison with the intent of rushing home to take an antidote. But the wretched woman detained him until she could see that the poison was taking effect. The physician managed to totter home and tell his wife what had happened and to instruct her to collect the fee.[24] When she attempted to do so, she in turn was poisoned by the same woman but managed to live long enough to report the affair to the local magistrate. The murderess was sentenced to be thrown to the beasts.[25]

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[21] X, 11. For Apuleius' *Metamorphoses,* I have used the third edition of R. Helm's Latin text (Leipzig: B. G. Teubner, 1931)

[22] See D. W. Amundsen, "The liability of the physician in Roman law," in *International Symposium on Society, Medicine and Law,* ed. H. Karplus (Amsterdam-New York: Elsevier, 1973), pp. 20 f.; R. G. Summers, *A Legal Commentary on the Metamorphoses of Apuleius* (unpublished dissertation, Princeton University, 1967), p. 318.

[23] On the general subject of suicide in the Graeco-Roman world, see D. Gourevitch, "Suicide among the sick in Classical Antiquity," *Bull. Hist. Med.,* 1969, *43*: 501 ff., and on this specific story, pp. 506 f. See also L. Edelstein, "The Hippocratic Oath: Text, Translation and Interpretation," in *Ancient Medicine: Selected Papers of Ludwig Edelstein,* ed O. and C. L. Temkin (Baltimore: Johns Hopkins Press, 1967), pp. 13 f. and notes 23 and 24

The role of the physician in this tale is undoubtedly one of pure poetic justice. This must have been especially pleasing in classical society where physicians were so often open to suspicion and accusations of poisoning.[26] The old cliché that the physician who can cure can also kill will probably always be with us.[27]

The anonymous romance, *Apollonius, Prince of Tyre,* contains a story involving a physician and his students: Apollonius and his pregnant wife were on a voyage when the latter reached term. The baby was delivered but the mother retained the placenta and appeared to have died. Weeping bitterly, Apollonius ordered her body to be placed in a coffin and the coffin to be put into the sea. On the third day, waves brought it to land close to Ephesus, near the estate of a physician who was taking a walk with his students along the shore. The tide had gone out and he saw the coffin and told his attendants to bring it to his house. Then the physician gently opened the coffin and observed an exceedingly beautiful girl therein. He then noticed that some money had been placed at her head along with a note. He read the note that instructed whoever should find the coffin to use half of the money in it to provide the body with a burial and keep the other half for himself. The physician swore that he would spend even more on the funeral than asked and ordered a funeral pyre to be prepared immediately. While the funeral pyre was being prepared, a student of the physician arrived who was described as a youth in appearance but in ability an old man. When he saw the girl's body, he asked his mentor where the new corpse had come from. The physician did not answer the question, but instructed him to take a bottle of ointment and pour it over the body of the girl as part of the last rites. The youth accordingly took the ointment and went to the funeral bier of the girl, drew back her clothing, poured the ointment and, with an inquisitive hand, proceeded to spread it over her body. To his astonishment, he thought that he had detected signs of life. He then felt for her pulse, checked her nostrils for signs of breathing, examined her lips with his own and felt a gentle breathing. He instructed his companions to apply heat over all her body. He next began to exercise her arms[28] so the blood that had been coagulated within her was liquefied through the action of the ointment. Certain that she was alive, he ran to his teacher and told him. The student then had her carried to his own[29] bedroom and placed her on the bed, heated some oil, dipped some wool in it and rubbed it on her chest. This action completed the process of liquification of the coagulated blood and her breath then began to penetrate throughout her innermost parts. The girl then revived and, in a weak and stammering voice, said, "Please, doctor, don't touch me in any way that isn't proper. For I am the wife of a king and the daughter of a king." The young man rushed out of the bedroom and said to his teacher, "Come and see your student's achievement!" When the physician went into the room and saw that the girl whom he had believed dead was alive, he said to his pupil, "I approve of your skill, I praise your practical knowledge, I admire your diligence. But listen, student: I don't want you to be deprived of the benefit of your skill. Here, take your fee. For this girl has brought her money with her." He then gave him half of the money that had been placed in the girl's coffin and ordered poultices and nourishing food for her.[30]

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[24] This, no doubt, was intended to emphasize physicians' greed, a popular topic in the classical world.

[25] X, 23 ff.

[26] There is a most interesting discussion of this subject by F Kudlien in "Medical ethics and popular ethics in Greece and Rome," *Clio Medica,* 1970, *5*: 97 ff.

[27] There are many examples of this prejudice in folk-literature, *e.g.*, S. Thompson, *Motif-Index of Folk-Literature,* K2041.1, P424.2; S. Thompson and J. Balys, *The Oral Tales of India* (Bloomington: Indiana University Press, 1958), K1825.1 6.

The fear of being buried alive, for the comatose to be mistaken for dead, is deep-seated in the human psyche.[31] One's fate in such a situation can depend on the efficiency of the physician by whom he is either treated or declared dead. Apocryphal or not, tales illustrating the demonstration of ability to detect life in a body apparently dead have been told concerning various physicians and healers. The story most frequently encountered that portrays an historical figure from the classical era involves Asclepiades of Prusa who lived in Rome during the first century B.C. While walking, Asclepiades came upon the funeral procession of a man whom he did not know. He stopped the cortege, examined the body and, detecting life, saved the man from being cremated alive. This incident was recorded by Celsus [32] and is twice mentioned by Pliny the Elder,[33] who refers to it as one of the major factors contributing to Asclepiades' fame. Writing for the popular audience, H. E. Sigerist describes this incident thus: "Then he had one of those strokes of luck which sometimes make a physician famous. A funeral train passed him. The 'corpse' had been placed upon the pyre, and the torch-bearers were were about to ignite it. But the man was not really dead! Asclepiades had him taken down from the pile and restored him to life. Naturally patients thronged round this miracle-worker. He seemed a messenger from heaven rather than an ordinary physician. The houses of the great were opened to him. . . ." [34] T. C. Allbutt remarked: "The multitude acclaimed Asclepiades as a god—to the annoyance, no doubt, of rival practitioners. This rather well-worn legend is used in building up the accusations of charlatanry against the too lucky physician." [35] Regardless of the attitudes of one's peers in the medical community, the sensation that such an act would have created among the general public is obvious. The melodramatic, romantic and glamorous possibilities inherent in a situation where an especially gifted physician saves the hero or heroine from such disaster were naturally attractive for the author of the *Apollonius, Prince of Tyre,* and the popular appeal of the narrative was enhanced by the inclusion of this incident.

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[28] The text here is uncertain.

[29] The students apparently lived with their physician-teacher during their time of instruction.

[30] XXV ff. I have used the second edition of Riese's Latin text (Leipzig, 1893).

[31] For the motif of one apparently dead reviving while being prepared for burial, see Thompson, *Motif-Index of Folk-Literature,* N694.

The last theme to be pursued is one that is in that grey area between the medical and the metaphysical: the diagnosis of the enervation and depression of a young man or woman suffering from lovesickness.

In Heliodorus' *Ethiopian Romance,* Charicles is telling Calasiris about the lovesickness of his daughter, Charicleia.

> I called in the most highly reputed physicians, as you suggested, to examine her, and promised them all my fortune as a fee if they could help her. As soon as they came

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[32] *De medicina,* II, 6, 15.

[33] *Historia naturalis,* VII, xxxvii, 124 and XXVI, viii, 15.

[34] *The Great Doctors,* 2nd ed., 1933 (New York: Doubleday Anchor Books, 1958), p. 38.

[35] *Greek Medicine in Rome* (New York: Macmillan, 1921), p. 184.

> in they asked what was the matter with her. She turned away from them and would not answer them a single word, but kept loudly repeating a verse from Homer, "O Achilles, son of Peleus, most excellent of the Achaeans." The learned Akestinos—you probably know the man—laid his hand on her wrist, though she was unwilling, and appeared to be diagnosing her condition from her pulse, which, I think, indicates the beating of the heart. He continued his examination for a long time, observed her narrowly up and down, and then said, "Charicles, it was futile for you to call us here. Medical skill cannot help in this case." I cried out, "Oh, my god! What are you saying? Is my darling daughter lost? Is she beyond hope?" "There's no need to get all excited," he replied, "just listen." He then drew me aside from the girl and the others and said: "Our art professes to treat diseases of the body, not those directly of the mind, except when the mind is affected along with the suffering body and accordingly is helped when the body is cured. This girl is ill, but not with an illness of the body, for there is no humoral superfluity, she is not suffering headache, nor is she burning with fever. There doesn't seem to be any other bodily affliction, either local or general. This is the case, beyond any doubt." When I earnestly begged him to tell me if he had discovered anything further, he said: "Isn't it obvious, even to a child, that the affliction is clearly love? Don't you see that she has dark rings under her eyes, that she glances dartingly to and fro, and her face is pale, she doesn't complain of internal discomfort, her mind is troubled and she cries out whatever thoughts happen to strike her, she's insomnolent for no apparent reason and is losing weight rapidly? You must find a saviour for her, Charicles, and that man would be no one other than the one she loves."[36]

The physicians who are called upon to handle a similar case are not as proficient as this learned physician, whose name in Greek means roughly "the healer." In the anonymous *Apollonius, Prince of Tyre,* the daughter of the king of Pentapolis had fallen in love with Apollonius. After a short time, she became weak and took to bed. Her father became alarmed and summoned physicians. They examined her all over and took her pulse, but were entirely unsuccessful in discovering the cause of her illness.[37]

The tale told by Aristainetos involves a clever physician whose ingenuity causes some entertaining developments:

> Charicles, the son of the most worthy Polycles, was ill in bed on account of a desire for his father's concubine. Although he feigned a hidden physical ailment, in reality the problem was lovesickness. At any rate, the father, being a good parent and loving his son very much, immediately summoned Panakeos, a suitably-named physician,[38] who, putting his fingers on the youth's pulse, observed skillfully his unsteady mood and noted with his eyes a diagnosable problem of the mind, but

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[36] IV, 7. I have employed Colonna's edition of the Greek text of Heliodorus (Rome: Typis Regiae Officinae Polygraphicae, 1938).

[37] XVIII.

[38] His name in Greek means "the one who cures all."

noticed no sickness at all well-known to physicians. For a long time, indeed, although he was a great physician, he was helpless. Then, by chance, the one whom the young man loved passed by. At once his pulse leapt erratically and his appearance seemed troubled and his countenance was no better than his pulse. From both sources Panakeos discovered what the youth's illness was and he did not arrive at that simply by skill but it came by chance and this gift of providence he kept to himself until the right time.

This was the first step he took in conducting his investigation. As he once again stood nearby, he ordered every girl and woman of the house to pass before the sick youth, not all together but individually, separated from each other by a short interval. And while this was happening, he took the youth's pulse, the unfailing guide for physicians, the unlying displayer of the condition residing within us. Now the youth, bedridden by desire, was unmoved by the rest of the women, but when the concubine whom he loved appeared, immediately both his appearance and his pulse again changed.

But the wise and excessively lucky physician still was going to make the proof of the ailment more certain in his own judgment, "saving the third for the saviour," as they say.[39] Using as an excuse that the illness required the preparation of medicines for the youth, he departed for a time. As he left, promising that he would supply the medicines on the next day, he encouraged both the patient and the grieved father to be of good cheer.

When, at the appointed time, he returned, the father and all the rest of the household addressed him as saviour and, coming forward, greeted him cheerfully. The physician, being bitterly troubled, responded harshly, shouting that he was withdrawing himself from the case. While Polycles was persistently begging and inquiring about the cause of the rejection, Panakeos, shouting quite immoderately, continued to be angry and acted as though he intended to depart immediately. But the father begged still more earnestly, kissing the physician's breast and embracing his knees. Then, pretending that he was yielding to force, Panakeos heatedly expressed his reason as follows: "Your son loves my wife desperately and wastes away with an unlawful lust; I resent him now and cannot endure the sight of this would-be adulterer." Then Polycles, when he had heard, was ashamed of his son's illness, and blushed before Panakeos. But, when he had gained control of himself, he did not hesitate to beg the physician to give up his wife, calling the matter a necessary, not an adulterous, means of deliverance. While Polycles was begging in this fashion, Panakeos cried out loudly such responses as one might expect from a man to whom it had been suggested that he should change from a physician into a pimp to promote adultery, especially that of his own wife, regardless of what words were used to describe the action. When Polycles again pressed him with entreaties and kept calling the affair a necessary solution, not an adulterous one, the calculating physician, as if he were seriously debating the matter, said to Polycles: "Now really, by Zeus, if your son were in love with your concubine, you wouldn't marry her off to him because he desired her."[40] After Polycles replied, "Yes indeed, I would, by Zeus," the wise Panakeos said, "Then direct your entreaties to yourself, Polycles, and urge the right things. For this lad loves your concubine. Now, if it is right for me to hand over my spouse to anyone whatever for the sake of his deliverance, as you say, it is indeed much more right for you to give your concubine to your endangered son." He spoke logically, he concluded powerfully, and he persuaded the father to be obedient to his own advice.[41]

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[39] Proverbial: cf. Aeschylus, *Choephori*, line 1073.

[40] In translating Aristaenetos, I have followed the Greek text as edited by R. Hercher (*Epistolographi Graeci*, Paris, 1873) except in this last sentence ("Now . . . him.") where

When Apuleius presents to us a lovesick woman, although he does not directly employ physicians in the diagnosis,[42] he uses this opportunity to criticize the medical profession for its lack of perception. The story involves the same woman we encountered earlier who was consumed with an unnatural passion for her stepson.[43] At first she was able to keep her lusts under control. "But when," in Apuleius' words, "her whole being was filled with an insane fire and raving Eros flared up excessively, she then succumbed to the raging god and, pretending to be faint, she disguised a wound of the soul as an illness of the body. Everyone knows the detriments to health and appearance that beset those who are ill and also those who are in love: an unsightly paleness, languid eyes, weak knees, restless sleep and a sighing that is more vehement because it is slowly tortured. You would have believed that she was tossing so with the heat of fever if it were not for her crying. Oh, the shallow intellect of physicians! What does the beating of the pulse indicate, what does the immoderate heat mean, why the weary panting and the incessent tossing and turning back and forth, from side to side? Good gods! How easy is the diagnosis although not to the ingenious physician, nevertheless to anyone who has experienced Venus' passion, when you see anyone burning without any bodily fire." [44]

The motif of lovesickness was common in classical literature and its first appearance antedates the romance by several centuries. The description of its symptoms by Sappho is very well known: "For when I see you, my voice fails, my tongue is tied, a thin flame creeps beneath my skin, I cannot see, there is a throbbing in my ears, the sweat runs down, I tremble all over, I am paler than grass, I seem not far from death. . . ." [45] In the *Hippolytus* of Euripides, Phaedra is described as

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I have adopted the emendation suggested by W. G. Arnott ("Some passages in Aristaenetos," *Bull. Inst. Classical Studies of the University of London*, 1968, *15*: 120 f.).

[41] I, 13.

[42] Lovesickness in the Graeco-Roman novels did not always lead to medical consultation, *e.g.*, in book one of the *Ephesian Tale* of Xenophon, both Habrocomes and Anthia were love-sick and their parents elected to consult the shrine of Apollo in Colophon concerning the cause of their ailment.

[43] Above, pp. 329 ff.

[44] X, 2.

[45] The translation is from A. R. Burn, *The Lyric Age of Greece* (London: E. Arnold, 1960),

being wracked with fever[46] and tossing to and fro in her bed,[47] being pale and wasted,[48] speaking frenzied and senseless words,[49] eating little,[50] and calling herself mad.[51] Theocritius,[52] Vergil,[53] Ovid,[54] and Propertius,[55] to mention just a few classical authors, poeticized the love-sick. This motif certainly was not limited to Greek and Latin literature but is commonly found in folktales[56] and was extremely popular as a theme in literature during the medieval era.[57]

The involvement of the medical profession in the diagnosis and treatment of lovesickness found its literary expression quite early in middle eastern literature. Written between 1300 and 1100 B.C.,[58] the following Egyptian poem portrays a love-sick youth's frustrations at the futility of seeking medical help:

> Seven days from yesterday I have not seen my beloved,
> And sickness hath crept over me,
> And I am become heavy in my limbs,
> And am unmindful of mine own body.
> If the master-physicians come to me,
> My heart hath no comfort of their remedies,
> And the magicians, no resource is in them,
> My malady is not diagnosed . . .
> Better for me is my beloved than any remedies. . . .[59]

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p. 231. Compare Catullus (poem number 51): Lingua sed torpet, tenuis sub artus/Flamma demanat, sonitu suopte / Tintinant aures, gemina teguntur / Lumina nocte.

[46] Lines 131 f.

[47] Lines 203 f.

[48] Lines 174 f.

[49] Lines 232 f.

[50] Lines 274 f.

[51] Lines 241 f.

[52] II, 85 f.

[53] *Aeneid,* IV, 65 ff.

[54] *Heroides,* VI, 27 ff.

[55] I, 5.

[56] *E.g.,* Thompson, *Motif-Index of Folk-Literature,* T24 and T24.1. "According to the Hindus there are ten stages of lovesickness: 1. love of the eyes, 2. attraction of the Manas or mind, 3. birth of desire, 4. loss of sleep, 5. loss of flesh, 6. indifference to objects of sense, 7. loss of shame, 8. distraction of thought, 9. loss of consciousness and 10. death." In *Love, War and Fancy—The Customs and Manners of the East from Writings on the Arabian Nights by Sir Richard Burton,* ed. Kenneth Walker (London: W. Kimber, 1964), p. 147.

[57] For a superlative study by a scholar well acquainted with medieval medical sources, see J. L. Lowes, "The loveres maladye of hereos," *Modern Philology,* 1914, *11*: 491 ff.

[58] *Ancient Near Eastern Text Relating to the Old Testament,* 2nd ed., ed. J. B. Pritchard (Princeton: Princeton Univ. Press, 1955), p. 467.

[59] As quoted by H. E. Sigerist, *A History of Medicine,* vol. 1 (New York: Oxford Uni-

It is not until the genre of prose fiction developed in classical literature that we find in Greek and Latin sources members of the medical profession being consulted in cases of lovesickness.

A very major difference between Heliodorus and Apuleius lies in their attribution of cause. The wise physician in the *Ethiopian Romance* employs a rudimentary knowledge of psychophysiology whereas Apuleius views the symptoms as feigned [60] in that the problem is viewed as being the direct result of the supernatural agency of love.[61] The view was popularly held in classical thought that love was a divinity that afflicted mankind in ways analogous to disease and medical authors of the highest stature addressed themselves to the effect that love could have on the individual.[62] Galen clearly recognized the psychosomatic basis of lovesickness and divorced it from the mystical realm, assuring his readers that there was nothing preternatural about lovesickness.[63]

The ability to recognize lovesickness was popularly held to be a mark of the most skillful physicians. Consequently there arose apocryphal tales attributing this ability to physicians venerated for their knowledge. The *Life of Hippocrates,* attributed to Soranus of Ephesus, relates that the father of medicine, because of his great reputation, was summoned to come to treat the waning Perdiccas, King of Macedonia. Hippocrates correctly determined that the king was in love with his late father's concubine and cured him of his affliction.[64]

Five classical authors, Valerius Maximus,[65] Plutarch,[66] Appian,[67]

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versity Press, 1951), p. 291, note 2. Similar sentiments are expressed in another love poem from Egypt: When in the house I lie all day / In pain that will not pass away / The neighbours come and go / Ah, if my darling to me came / The doctors she would put to shame / *She* understands my woe. As quoted by M. A. Murray, *The Splendor That Was Egypt* (New York: Hawthorn Books, 1963), p. 210.

[60] Not to be confused with the feigning of illness as a pretext for being with one's lover: see, e.g., Josephus, *Antiquities of the Jews,* II, 45, and Thompson, *Motif-Index of Folk-Literature,* K1514 11.

[61] Cp. C. S. Lewis, *The Allegory of Love* (Oxford: Clarendon Press, 1936): "In ancient literature love seldom rises above the levels of merry sensuality or domestic comfort, except to be treated as a tragic madness, an *atē* which plunges otherwise sane people (usually women) into crime and disgrace" (p. 4).

[62] *E.g.,* Aretaeus, III, 5; Oribasius, *Synopsis ad Eustathium,* VIII, 8; Caelius Aurelianus, *Chronic Diseases,* I, 5; and the passages from Galen cited below.

[63] Kühn, XVIII/2, p. 18: "Accordingly we know that neither epilepsy nor love is a divine disease."

[64] Sec. 5.

[65] *Facta ac Dicta Memorabilia,* V, vii, 3, ext. 1.

[66] *Life of Demetrius,* XXXVIII.

[67] *Roman History,* XI (*The Syrian Wars*), x.

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Lucian,[68] and the Emperor Julian,[69] relate a somewhat similar story concerning Erasistratus. The following is a composite of the five versions: [70]

Antiochus I Soter, son of Seleucus I, became obsessed with a burning passion for his young stepmother, Stratonice. He unsuccessfully attempted to suppress his desires, but at last resolved to do away with himself by neglecting his person and abstaining from food, under the pretence of having some disease. Erasistratus,[71] who was receiving an extremely high salary as court physician to Seleucus I,[72] was at first unable to form any diagnosis of Antiochus' malady. Finally, when he had observed that his body was free from all symptoms of disease, he conjectured that this was some affliction of the mind. Since Antiochus would not confide in him, Erasistratus took up a post by his patient's bed and observed him closely. He noticed that only when Stratonice appeared did the familiar symptoms develop. So he told Seleucus that his son had an incurable disease and added, after Seleucus had adequately wailed his despair, that his disease was love, love for a woman, but a hopeless love. Seleucus was astonished that there could be any woman whom he, king of Asia, could not prevail upon to marry such a man as his son, heir to the kingdom. When he asked the identity of the woman, Erasistratus replied, "Your son is in love with my wife." [73] Seleucus then pleaded with the physician to give up his wife for the health of his patient. Erasistratus maintained that even a father could not be expected to make such a sacrifice for his own son, much less a physician for his patient. When Seleucus insisted that he would surely make such a sacrifice himself, Erasistratus revealed the true situation to him and a solution was reached: Antiochus and Stratonice were married [74] and Seleucus gave to his son half of his kingdom.[75]

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[68] *De Syria Dea,* 17 f.

[69] *Misopogon,* 347 f.

[70] A *Quellenkritik* of these sources can be found in J. Mesk, "Antiochos und Stratonike," *Rheinisches Museum für Philologie,* 1913, n.s. *68*: 366 ff.

[71] Valerius Maximus wrote that the situation was "due to the foresight of the astrologer Leptines or, as some say, the physician Erasistratus " No other reference in classical literature to this astrologer is known.

[72] Whether for romantic or political reasons, Antiochus married his stepmother in 293/2 B.C. Since Erasistratus was born probably no earlier than 310 B.C., it is nearly impossible that he had already been appointed court physician to Antiochus' father at that time. Judging from a passage in Pliny the Elder (*Historia Naturalis,* VII, xxxvii, 123), the physician in question may have been Cleombrotus, father of Erasistratus Later tradition then transferred this tale from the father to the son owing to the greater subsequent fame of the latter. On the problem of identification, see M. Wellmann, "Zur Geschichte der Medicin im Alterthum," *Hermes,* 1900, *35*: 380 f.

[73] Two of our sources, Valerius Maximus and Julian, omit this bit of trickery and simply have the physician explain the situation to Seleucus without any subterfuge

Commenting on this tale, Galen wrote: "Erasistratus did not diagnose the lovesickness of the young man by reading auspices and he definitely did not, as some have written, detect love from the throbbing arteries of the youth, for there is no pulse especially characteristic of love. But, just as has happened to me once, when I was taking the pulse of a sick person, a certain woman of the household came into view and immediately the pulse became irregular and uneven and then, in a little while, as soon as the woman in question left, it returned to normal. For in this manner the changed pulse shows that some common, troubling ailment has occurred in the mind of the ill person." [76]

Apparently some people, especially those whom Galen, commenting on the Erasistratus tale elsewhere,[77] terms *hoi sophistai iatroi,* were interpreting this incident incorrectly, thinking that Erasistratus had been able to distinguish simply from the patient's pulse alone that he was suffering specifically from lovesickness. After chastising them for their ignorance, he recounts in detail a somewhat similar experience of his own: [78] He had been called to treat a woman suffering from insomnia and restlessness. After satisfying himself that there was no physiological cause, he determined that the problem was in her mind. He admits that it was quite by chance that he was able to narrow it down still further to lovesickness and conjectures that chance had played an important role in Erasistratus' diagnosis also. While he was with his patient, somebody happened to drop by and mention that he had seen a certain Pylades dancing. When Galen noticed that the woman's expression and color had changed, he felt her pulse and noticed that it was extremely irregular. Knowing that an agitated pulse can indicate other disturbances of the mind, on the next day he instructed one of his followers to come to the woman's house while he would be there and to mention that Morphus was to be dancing that day. When this was done, the woman's pulse was unaffected. The name of another dancer was mentioned on the next day with no effect. But on the fourth day, Pylades' name again came up causing the pulse to become disturbed. By careful watch on succeeding days, he confirmed that the woman was in love with Pylades.[79] Galen next proceeds to castigate those who had been unable to diagnose the woman's ailment: "What indeed was it then that escaped the notice of the physicians who had previously attended the woman? For such discoveries are arrived at by common deductions if one has even the smallest knowledge of medical science. As for me, I hold that it is because they do not have a clear conception of how an affliction of the mind usually affects the body; possibly because they do not know that the pulse is altered by strife and fears that suddenly disturb the mind." [80] Galen then indulges in some mild self-adulation, mentioning the advantages that Erasistratus had in his case, *i.e.*, having the loved-one living in the same home as the love-sick patient where Erasistratus himself was the physician-in-residence, whereas Galen, in visits over a period of a few days, had to rely simply on the mention of the name of the person who was the cause of the affliction.

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[74] Julian, the latest of our sources, maintains that Antiochus refused to take Stratonice from his father but, upon his father's death, married her.

[75] The similarity between this story and the one told later by Aristainetos (above, pp. 329 ff.) is so striking that one can assume a quite probable source relationship.

[76] XVIII/2, p. 40. See also Galen's comments in XVI, p. 309.

[77] XIV, p. 630. Not to be confused with the incident alluded to in XVIII/2, p. 40 (quoted above).

[78] J. Ilberg says that this is the first case of "Frauenbehandlung" in Galen's career about which we have any information. ("Aus Galens Praxis: Ein Kulturbild aus der römischen Kaiserzeit," *Neue Jahrbücher fur klassische Altertum,* 1905, *15*: 289)

Undoubtedly Galen would have found Heliodorus' physician's diagnosis of lovesickness primarily a lucky conjecture but surely not scientifically compatible with Galen's standards.[81] Apuleius' etiology of lovesickness he would have dismissed as superstition or poetic license.

Although the physician is certainly not idealized in the Graeco-Roman novel, he is indeed highly romanticized. That element of realism is lacking that would have been present had the physician been portrayed in the mundane atmosphere of routine treatment of his patients. Instead, he is made to respond to two types of challenges. One involves the ever-present moral dilemma inherent in the physician-patient relationship that arises from the physician's frequent opportunity for unethical conduct. The second category, including both the reanimation of a comatose person and the diagnosis of lovesickness, illustrates the extent of his proficiency. Here he is judged by his ability to deal with relatively unusual and unrealistic situations in which he has to demonstrate skills that were considered both in the popular mind and in the medical circles of that time to be distinguishing features of the most exceptional physicians.

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[79] A nearly complete translation of this episode can be found in A. J. Brock, *Greek Medicine* (London: J. M. Dent, 1929), pp. 212 ff. A modern psychophysiological discussion of these stories involving both Erasistratus and Galen, as well as parallel material from Avicenna, "The diagnosis of love-sickness: experimental psychophysiology without the polygraph," by M. Mesulam and J. Perry, has been published in *Psychophysiology*, 1972, *9*: 546 ff.

[80] XIV, p. 634.

[81] As an aside, it would be interesting to hear what Galen's reaction would have been if he had had the opportunity of reading the following self-diagnosis suggested by Bernard Gordon (late 13th-early 14th century A.D). After referring to Galen's diagnosis of lovesickness, Gordon wrote: "So, if anyone wants to know the name of the woman whom he loves, let him name many to himself and when he names the one whom he loves, immediately his pulse will be aroused. Then, that's the one" (I have here translated the Latin text as quoted by J. L. Lowes, "The loveres maladye of hereos," p. 500.)

Acknowledgment

I wish to express my gratitude to Professor Gary B. Ferngren, Department of History, Oregon State University, for his generous assistance in the preparation of this paper.

"DOCTOR WILLIAM CULLEN, PHYSICIAN, EDINBURGH": A CONSULTATION PRACTICE IN THE EIGHTEENTH CENTURY *

GUENTER B RISSE

> "For now thirty years I have served as a professor in their university and have devoted my time and labour to promoting the reputation of this School of Medicine. With what success my labours have been attented to and how much they have contributed to the increase of the students at the University, it is perhaps unbecoming for me to say, but I trust I may be allowed to claim some merit with the town of Edinburgh and some right to their favour." [1]

History has not been very kind to William Cullen, Britain's foremost clinician during the second half of the eighteenth century.[2] From the 1760s on, he was the most distinguished faculty member of Edinburgh's medical school, then the leading institution of learning in Western medicine. Cullen was greatly admired by medical students—one American called him "that shining oracle of Physic" [3]—while other colleagues addressed him as "the most illustrious physician in Europe." [4] He was

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* Read at the forty-sixth annual meeting of the American Association for the History of Medicine, Cincinnati, Ohio, May 3, 1973. The research for this paper was partially supported by the Penrose Fund of the American Philosophical Society.

[1] Excerpts from a letter drafted by William Cullen on November 12, 1786, to Henry Dundas proposing his son Henry Cullen as professor of botany at the University of Edinburgh. Cullen's letters are kept in the MSS Collection of the Royal College of Physicians of Edinburgh. They are contained in twenty quarto volumes extending from 1755 to 1790. Beginning in 1768, there are also letters from patients included in the correspondence. This collection will be referred to as *Cullen's Correspondence.* The quoted excerpts are from vol. XIII.

[2] Garrison's standard *An Introduction to the History of Medicine* (Philadelphia: Saunders, 1929), barely gives him half a page and repeats Sir William Hamilton's statement that "Cullen did not add a single new fact to medical science" (see pp. 357-8). Singer's and Underwood's *History of Medicine* (New York: Oxford Univ. Press, 1962), similarly describes him summarily in two short paragraphs (pp. 148 and 151).

[3] Thomas Parke, *Journal,* October 22, 1771; *Pemberton Papers* 57, pp. 93-98, Historical Society of Pennsylvania, as quoted in W. J. Bell, Jr., "Some American students of 'that shining oracle of Physic,' Dr. William Cullen of Edinburgh, 1755-1766," *Proceedings of the American Philosophical Society,* 1950, *94*: 275-281.

[4] Letter from a French physician appended to a 28-page consultation written in French and dated October 15, 1785. This and many other letters, classified as "Consultation letters to Dr. Cullen," are preserved in twenty-one leather bound folios in the MSS Collection of the Royal College of Physicians of Edinburgh and will be referred to as *Consultation Letters.* The above letter is contained in vol. XVIII.

acknowledged by his grateful patients, who saw him as the ultimate arbiter of their sufferings and the supreme referring physician. In short, William Cullen appeared to have been the epitome of a successful teacher and practitioner.

Shortly after his death in 1790, however, Cullen was quickly forgotten. His medical system rapidly became obsolete with the advent of chemical and electrical explanations of life, while his bedside teaching was soon eclipsed by the vastly more extensive clinical education available to students in cities such as London and Paris.[5] The lack of a biographical account, promised but never written by one of his sons, robbed posterity forever of a more intimate portrait of William Cullen.[6] Letters, lecture-notes taken by students, and his printed books have survived the vagaries of time, allowing at least a partial appraisal of his thought.[7]

As many prominent physicians before and after him, Cullen was a much sought-after medical consultant. Unlike the case of Boerhaave, most of Cullen's incoming letters and copies of his replies have been preserved.[8] Previous patients, their relatives and acquaintances, as well as a growing number of Cullen's students who had become active practitioners gradually swelled the ranks of those asking him for medical advice. This was especially true after 1773, the year in which Cullen assumed the chair of the practice of physic following the death of the previous holder of that position, John Gregory (1725-1773). His clinical skills, fair judgement, and a certain lack of therapeutical aggressiveness endeared him to most of the patients seeking his advice. In an era when travel was difficult, physical diagnosis extremely rudimentary, and laboratory medicine nonexistent, a fair description of somebody's complaints was deemed sufficient and appropriate to arrive at a tentative diagnosis and the institution of treatment.

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[5] An able summary of Cullen's ideas on fevers is L. S. King, *The Medical World of the Eighteenth Century* (Chicago: Univ. of Chicago Press, 1966), Chap. V, pp. 139-143. A more extensive analysis is contained in John Thomson, *An Account of the Life, Lectures, and Writings of William Cullen, M.D.*, 2 vols. (Edinburgh: W. Blackwood & Sons, 1859), especially vol. I, pp. 258-387. Cullen began to deliver clinical lectures at the Royal Infirmary of Edinburgh in 1757 together with Drs. Robert Whytt and Alexander Monro secundus.

[6] Cullen's later biographer, John Thomson, never met the physician personally, and only received the surviving papers from the family at the death of Lord Robert Cullen who had himself intended to write about his father and therefore declined various offers from friends and colleagues (see "Advertisement" in John Thomson, *op. cit.* (n.3 above), pp. vii-x).

[7] Several books with lecture notes on chemistry, physiology, clinical medicine, pathology, medical history, and materia medica in Cullen's own hand or written by his students are available at the Royal College of Physicians of Edinburgh, Glasgow University, and the American College of Physicians of Philadelphia. Cullen's physiology, nosology, and the *First Lines of the Practice of Physic* (published in 4 vols. between 1777-1784) were published by John Thomson, *The Works of William Cullen, M.D.*, 2 vols. (Edinburgh: W. Blackwood, 1827). See also Cullen's *A Treatise of the Materia Medica*, 2 vols. (Edinburgh: C. Elliot, 1789).

[8] See *Boerhaave's Correspondence*, ed. by G. A. Lindeboom, 2 vols. (Leiden: E. J. Brill, 1962-64). Only a few letters from consulting physicians have been found

Cullen's consultation practice by correspondence was carried out during the last twenty-five years of his life. The letters, addressed to "Doctor William Cullen, Physician, Edinburgh," and copies of his replies number almost 3,000.[9] Such an apparently burdensome task was handled by Cullen with an organization and efficiency unparalleled in the annals of medical consultation.[10] As soon as a letter demanding advice was received in Cullen's modest home in South Gray's Close, his aide carried out a careful registration procedure. The name of the patient, referring physician when applicable, at times the city of origin as well as month and year of mailing, were written on the outside of the communication, near the address. If the patient had previously corresponded with Cullen, the physician's latest letter was indicated by citing the volume and page number of his collected responses which were bound in chronologically arranged folios.

Cullen, in spite of his advanced age, usually rose every morning about a quarter to seven. With the help of an amanuensis, he would read the letters received the day prior, check his previous counsel, and then swiftly dictate his replies until about 9 a.m. before visiting his Edinburgh patients. Most of the inquiries were answered within a day, and Cullen was often praised for his prompt response. If hurried, he could work on various consultations simultaneously.[11]

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[9] The only author to mention the correspondence was J. D. Comrie, "An eighteenth century consultant," *Edinburgh Medical Journal,* 1925, *32*: pp. 17-30. Comrie's article is disappointing in that he merely presented a short biography of Cullen's and reproduced verbatim seven letters concerned with smallpox inoculation, hysteria, and gastric problems. One letter was from James Boswell and another from John Hunter.

[10] Even Benjamin Rush complained about his consultation practice: ". . . large deductions have been made from the *time* which might have been employed profitably in business in Philadelphia, by answering letters for advice for which I receive no compensation, that I have been obliged reluctantly to decline answering any more of them unless they are accompanied with a fee. I except from this rule of conduct letters of advice to persons who are objects of charity. My usual fee for my first letter of advice is ten dollars, and five for every subsequent one." Letter addressed to Dr. Charles Everett, Charlotteville, Virginia, and dated Philadelphia, July 25, 1809, and preserved in MSS Collection, National Library of Medicine.

[11] This information is contained in an anonymous letter found among the Cullen material deposited with the University of Glasgow, and used by John Thomson for his biography. Cullen was so organized that he would send out in advance (before 8 a.m.) cards to his

After completing his dictation, Cullen would sign the letters and allow the amanuensis to write out on a separate sheet of paper the prescriptions which were to accompany his advice. With Cullen's permission, the aide would place the initials W. C. below the recipes, and then proceed to copy the entire letter or its initial paragraphs for the physician's file.[12] Certain important consultations addressed to foreign dignitaries were amended by Cullen himself after dictation. Since we possess several of these letters with corrections, the amanuensis presumably rewrote the final version of the response before sending it.[13]

The last letter which Cullen's secretary copied by hand was dated March 21, 1781. Beginning with a reply of April 1 of the same year, Cullen employed a copying machine to make a duplicate of his letters for future reference. For this purpose, Cullen used a letter copying device recently invented by the great Scottish engineer, James Watt. The essential principle involved in Watt's method was to offset the original writing against a dampened sheet of thin, clear paper with the help of a press.[14] During the latter part of 1779, Watt described in some detail his copying principle to a mutual friend of his and Cullen, the chemist Joseph Black.[15] Cullen heard of the new invention through

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patients and people he wanted to meet indicating the exact hour of his visit. Since Thomson had strenuously solicited testimonials and recollections from individuals who knew Cullen personally, these accounts are probably correct.

[12] Cullen mentions this method in a letter written to a patient on July 4, 1789. The amanuensis wrote out the prescription and initialed it without leaving enough blank space for Cullen to dictate his recommendations, which had to be placed in part behind the recipe and the W. C. initials. The whole letter was eventually rewritten but the original and a copy of the corrected version are still available, in *Consultation Letters,* vol. XXI.

[13] These corrections in Cullen's own handwriting can be observed in several letters. Answering an inquiry of Dr. William Baylies, privy councellor and physician to his Majesty, the King of Prussia, dated September 13, 1784, Cullen writes: "After much thinking, what appears to me most probable is that Mr. Heynitz's disease is very much a case of gout. . . . I cannot now positively determine whether the bleedings practiced by the late Dr. Werlhoff whom I have always esteemed were not too much, but I am persuaded that the bleedings both spontaneous and artificial which have so often occurred have hurt his constitution. . . ." *Consultation Letters,* vol. XVII.

[14] Watt's frequent travels and the need to keep copies of his extensive business correspondence prompted him to develop a mechanical copying device. Watt added sugar or gum to tannin ink and obtained a clear impression of the original. The thin copy paper was chemically prepared by dipping it into a solution of vinegar or borax, thus turning black or dark brown when placed in contact with the ink. The ink and a wooden roller press were patented on February 14, 1780. See Henry W. Dickinson, *James Watt, Craftsman and Engineer* (Cambridge: University Press, 1935), pp. 115-118.

[15] Black received the first details from Watt in July 1779, and the copying machine is repeatedly mentioned in subsequent letters Watt formed a new company to exploit the discovery and launched an aggressive advertisement campaign involving demonstrations to

Black and seems to have immediately seized upon the usefulness of this contraption for his extensive correspondence.[16]

The bulk of Cullen's letters concerned patients far removed from Edinburgh who had never met him but were keenly aware of his clinical reputation and naturally viewed him as the final medical authority. Many letters begin with the phrase "in consequence of your well merited medical reputation I beg leave to lay before you the state of my health." [17] Most patients were anxious to render a complete picture of their illness to Cullen. Their belief in the "anomalous" or "very particular" nature of their affections often resulted in 8-10 page letters, frequently written with the aid of the attending physician. That method had its dangers as in the case of one lady who objected to her doctor's claim that she had a "scrophulous taint in her blood." The physician had to back down and wrote to Cullen: "P.S.—take no notice of this description." [18]

Some of Cullen's letters were directed to patients he planned to see personally in Edinburgh, while others clearly contained follow-up directions for people who had previously visited him at his home.[19] He also provided summary letters which certain patients took back to their own physicians. Often, Cullen would merely correspond with the referring physicians who submitted difficult cases for his evaluation. Among the consulting physicians were former students of Cullen [20] and others who had read his books and were impressed by his theory of neural patho-

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bankers and members of Parliament which resulted in the sale of more than 150 presses in the first year. Black received one of the first machines in October, 1780, but there were still some difficulties with the ink. See Eric Robinson and Douglas McKie, eds., *Partners in Science, Letters of James Watt and Joseph Black* (London: Constable, 1970), pp. 67-103.

[16] Black, writing to Watt from Edinburgh on January 7, 1780, about the copying machine, declares: "Since my last I have found two Gent[leme]n besides the former who wish to be Possessed of your secret for copying writing—Mr. Smith who is now a Commiss[ione]r of the Customs here and Doctor Cullen to whom it will be extreamly Usefull . . .," Eric Robinson and Douglas McKie, *op. cit.* (n.15 above), p. 75. The "Mr. Smith" alluded to was no other than Adam Smith, author of the *Wealth of Nations.*

[17] Another common expression with minor variations was "the fame of your superior skill in Physic gives me hope that from your assistance I may gain relief."

[18] Letter written in early 1776 with the help of a surgeon who had attended Cullen's lectures and accompanied by a fee of two guineas, *Cullen's Correspondence,* vol. III.

[19] "I send this letter today that you may have time to consider it before I visit upon you tomorrow morning to give what explanations you may happen to demand," letter dated July 7, 1785, in *Consultation Letters,* vol. XVIII.

[20] "Having had the honor of attending your lectures is all the apology I make in troubling you with this case," dated December 27, 1778, in *Cullen's Correspondence,* vol. V.

genesis.[21] At times, he was rewarded with autopsy reports of cases he had treated, such as a scirrhous cancer of the stomach,[22] another of the liver, and the submaxillary tumor of Governor Johnstone of Bristol. One surgeon provided him with a postmortem report of an enlarged spleen which had caused some bafflement.

Since this consultation service was carried out exclusively through the mails, it certainly reflected all the drawbacks inherent in such a form of medical practice. There were fateful mail delays and complaints that certain letters had miscarried.[23] Cullen was occasionally dissatisfied with the information he had received but unable to interrogate the patient himself to fill in the gaps.[24] Some patients stopped their medications before the next letter arrived, or at times objected to Cullen's rather "strong" expressions which could not be mitigated through personal contact.[25] One letter arrived from Belfast at Cullen's house together with a stool specimen containing "wonderful fragments with ears or horns, substances like portions of pickled cucumber." [26] In that particular instance, the patient's maid had spiked the specimen with bird bones in order to dramatize the case.

Cullen's standard charge for a written opinion was one guinea, although it appears that he often gave advice without being paid in advance, "leaving it to the circumstances and generosity of the patient." [27] Referring to payment, Cullen declared: "Nobody, however, shall suffer from want of my advice and therefore I shall give my best

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[21] "The well known reputation you so deservedly enjoy in the world as a physician as well as the great confidence and good opinion I ever had of your sagacity and penetration in discovering the most probable causes of affections of the nervous system . . . ," dated October 15, 1785, in *Consultation Letters*, vol. XVIII.

[22] "I'm advised to give you this information as all concerned were satisfied at his being opened, and indeed it was his particular desire to his wife not to mind it if the medical people choose to do it" (From Glasgow, dated May 8, 1782), in *Consultation Letters*, vol. XV.

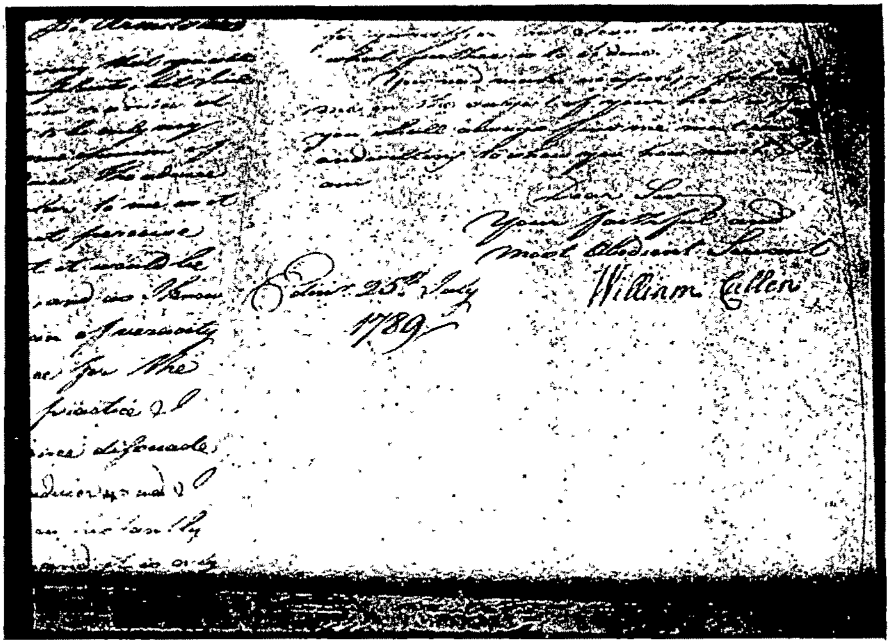
[23] One patient complained that the mail service between Edinburgh and Carlisle took 10-12 days.

[24] One referring physician defended himself: "I am not at all surprised to find (you), Dr. Cullen, dissatisfied with the information. . . I had only time to throw together a few hints of such things as Col. X. might not be supposed to know," dated February 20, 1789, in *Consultation Letters*, vol. XXI.

[25] "Dear Doctor, I read the whole of your letter to Mr. McDowal only softening some of the stronger expressions. Upon the case there is really nothing more to be said . . . ," from Glasgow, dated January 22, 1784, in *Consultation Letters*, vol. XVI.

[26] From Belfast, dated November 3, 1783, in *Consultation Letters*, vol. XVI.

[27] See letter to a Mr. Cowmeadow, dated September 16, 1789, *Consultation Letters*, vol. XXI.



Dear Sir
Your faithful &
most obedient Servant
William Cullen

Edinr 25th July
1789

FIG. 1. Letter dictated by William Cullen and written by his amanuensis. Notice Cullen's signature. (From the MSS Collection, Royal College of Physicians of Edinburgh.)

now tho' I expect that everybody that can afford it will pay me for it." [28] Literacy and finances restricted such consultations to the upper and middle classes of society, although at least one farmer and a weaver living in Edinburgh's Shoemakers Close were among those listed in the correspondence. Clergymen, army officers, merchants, a surveyor, and various "gentlemen" and minor nobility were the rule. Among the more important patients figured an Italian nobleman, Prince Rezzonico, residing in Pall Mall, London,[29] and the Prussian Director of Mines named Heynitz.[30]

The letters addressed to Cullen came primarily from Scottish cities and villages such as Aberdeen, Banff, Dumfries, Inverness, Elgin, Paisley, Inveraray, Irvine, Montrose, and others unknown today such as Tweedmouth, Kilpatrick, and Calderwood. Other correspondents lived in England, especially in Carlisle, Berwick-upon-Tweed, Chester, Man-

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[28] Dated August 7, 1785, in *Consultation Letters*, vol. XVIII.

[29] Rezzonico sent two letters in Latin requesting advice in October of 1786, *Consultation Letters*, vol. XIX.

[30] This correspondence was carried out with a British physician, William Baylies, living in Berlin, between September-November 1784, *Consultation Letters*, vol. XVII.

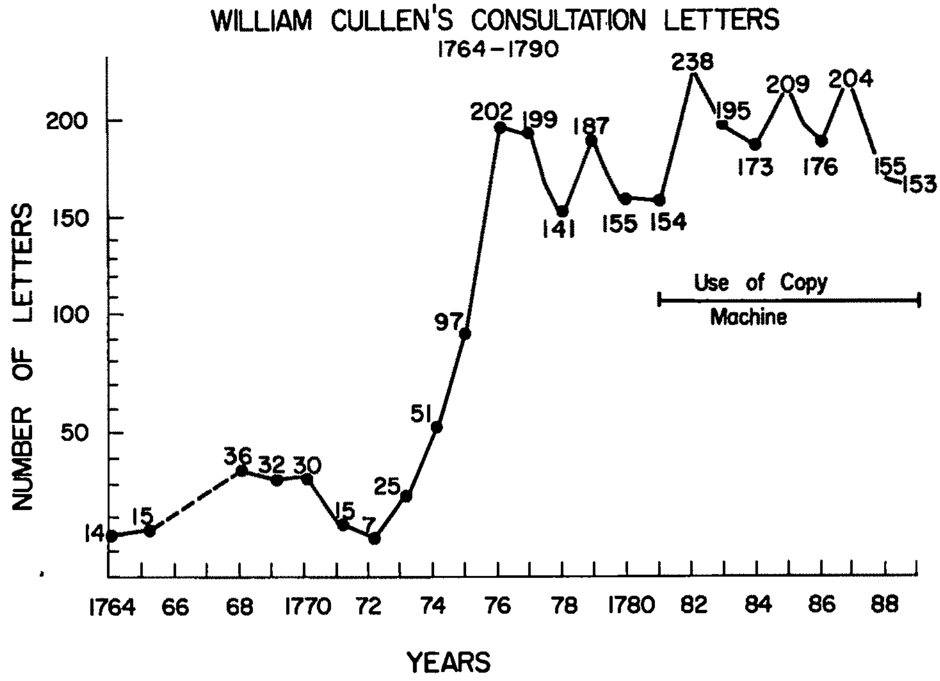

FIG. 2. From the available MSS material of the Royal College of Physicians of Edinburgh.

chester, Exeter, Liverpool, Newcastle-upon-Tyne, Bristol, Southhampton, and of course, London. Patients from Dublin, Belfast, and the Isle of Man were represented. From the Continent there were requests originating in Antwerp, Rouen, Genova, Berlin, Cadiz, and even Madeira, the latter from a physician travelling to India with a private patient. A patient, writing from Boulogne-sur-Mer on August 12, 1785, emphatically declared: "There is no distance that can prevent us from applying to you when any of us ails. . . . I cannot bring myself to place any confidence in the skills of the French physicians." [31] Even the New World consulted Cullen with letters arriving from New York and New Orleans.

Cullen emerges from his correspondence as a tactful and compassionate consultant. He never criticized his colleagues for their mistakes but smoothly suggested the necessary changes and, when possible, bolstered their own opinions and morale. In a letter to one of his patients, Cullen said: "You have taken a very prudent measure in consulting

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[31] See *Cullen's Correspondence,* vol. XII.

Dr. ——— whose abilities and judgement I have the highest opinion of and I am very clear that neither Dr. Monro nor I could have proposed anything better." [32] He showed a great deal of tolerance with demanding patients, addressing himself in detail to their multiple and recurrent complaints. "I cannot undertake to make her young again," he wrote to the attending physician of one such patient, "I think she speaks with too much despondency, and hope her complaints may still be very much relieved." [33] Like today's physicians, Cullen was sometimes caught in the dilemma of the so-called "functional" patient: "I am fully persuaded that Mr. X has no disease, but both because it may be difficult to persuade him of this, and because I cannot be sufficiently confident in a negative . . . [the] prudent measure [is] to give him a medicine." [34]

Although Cullen was kind and patiently interpreted the often strange information he received—in one case he had to review five daily pulse readings taken on a lady before and after spending some time on a swing [35]—he nevertheless remained firm in his recommendations. "I have again looked over the advice I gave you last week and really cannot find anything to add or alter," Cullen wrote to a dissatisfied patient, and to the referring physician of another, "I have thus offered the best advice I can to Mrs. X. But," he added with his usual humility, "it is possible that either you or she may think I have not attended to the variety of her particular complaints." [36] In truth, not even the greatest consultant is infallible. Wrote one patient, Mary Forsythe, to Cullen: "Agreeable to your direction, I used the nervous infusion for a fortnight which had not the least effect." [37]

Whenever possible, Cullen tried to cheer up his patients. In one letter he wrote: "I would willingly think that although you are not quite well, you are a good deal better than when I saw you. . . . I can only advise you to persist a little longer in the same remedies and regimen you have hitherto employed." [38] At times, Cullen would poignantly express his limitations, as in a long letter concerning various epileptic children in a Genoese family: "I have said all that I can recollect of what may be useful to this noble person, and have only to add my prayers that God may be pleased to bless my endeavours." [39]

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[32] Dated June 20, 1785, in *Consultation Letters,* vol. XVIII.

[33] Dated September 3, 1785, in *Consultation Letters,* vol. XVIII.

[34] Dated August 25, 1785, in *Consultation Letters,* vol. XVIII.

[35] The patient was a certain Mrs. Murray who had expressed some chest complaints. Using a stopwatch her physician counted her pulse daily before and after the lady had spent 5, 15, 20, and 25 minutes on a swing, March 1787, in *Consultation Letters,* vol. XX.

[36] Dated June 13, 1785, in *Consultation Letters,* vol. XVIII.

[37] Dated May 19, 1786, in *Consultation Letters,* vol. XIX.

[38] Dated August 1, 1785, in *Consultation Letters,* vol. XVIII.

Some patients expressed in glowing terms their recovery and gratefully acknowledged Cullen's help. Wrote one of them: "From my long silence you will no doubt have concluded that I had got the better of my complaints and when I tell you that I have lately taken unto myself a wife you will readily believe that I thought myself considerably better." [40] Another patient, Robert Fenwick, declared: "Sir, the benefit I have received from your judicious prescription will not permit me to defer my acknowledgement. . . . The complaints in my breast and bowels are totally removed, my appetite restored and that disagreeable sensation I had in my kidneys gives me now no further uneasiness. In short, I find myself quite a new Being and must ever entertain the highest Idea of that Medical Knowledge you are so justly renowned for." [41]

Himself a sufferer from gout, Cullen had a tendency to diagnose the same illness or a constitutional weakness leading to it, in many of his patients. This diagnosis gave him the opportunity to propose his favorite dietary regulations so frustrating to Cullen's disciple, John Brown. The regimen prescribed called for moderate intake of animal food once a day, abstention from tea, coffee, and tobacco, a ban on malt liquors of all kinds, avoidance of vegetables causing flatulence, and sparse consumption of strong-bodied wines. To supplement the restrictive diet, Cullen recommended gentle exercise in the fresh air, the daily application of a flesh brush to stimulate the skin of the back, and moderate living habits in general.[42] Most of these suggestions were based on Cullen's experience with his own gout. Writing to a friend a year before his death, he declared, "For many years past I have taken very little strong drink and always found myself hurt by the smallest excess in it." [43]

Cullen's treatment of scrofula and pulmonary tuberculosis was simple: "Remove her to the country to enjoy fresh air, frequent exercise, and proper diet," he declared to a consultant physician, "these I believe are almost the only remedies we can employ."[44] Several patients complaining of epileptic convulsions were urged to abstain from rich food and ingest a tonic, preferably Peruvian bark. Avoidance of sudden emotions and the pursuit of regular living habits were recommended by Cullen at a time when many of these fits were hysterical in nature.[45]

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[39] Letter directed in answer to an inquiry by William Batt, emeritus professor of chemistry, and dated July, 1788, in *Consultation Letters,* vol. XX.

[40] Dated June 9, 1781, in *Consultation Letters,* vol XIV.

[41] Letter dated October 31, 1779, in *Cullen's Correspondence,* vol. VI.

[42] For a detailed description of Cullen's treatment for gout see his *First Lines of the Practice of Physic,* 4 vols. (Edinburgh: W. Creech, 1777-84), vol I, chap. XII, pp. 352-417.

[43] Letter to Dr. Warren of Newcastle upon Tyne, dated October 1789, in *Consultation Letters,* vol. XXI.

On occasion, Cullen received letters from persons who sought a medical approach instead of submitting to a frightful surgical intervention, especially in the case of the so-called "gravel complaints" or passage of stones with the urine. One such individual from London, a patient of John Hunter, suffered from chronic urethritis and an urethral stricture. His remarks reflect the perennial diversity of approaches between surgeons and internists.[46] Another interesting approach was the use of electrical shocks for the treatment of paralysis or paresis, possibly secondary to a cerebrovascular disorder.[47]

Throughout the consultation letters Cullen advocated little bloodletting, usually only in the form of leeching or scarifications with wet cupping. Opiates were not to be given any longer than necessary in order to avoid habit formation.[48] The use of mild laxatives without purging was often indicated but no deliberate vomiting of the patient was allowed. Cullen frequently used the counter-irritation procedures in vogue during his time: cupping, moxibustion, and the establishment of setons, that is, suppurating skin sores kept open by the insertion of a cord. Cold bathing was viewed as one of the most powerful tonics and was to be employed with extreme caution, especially in children.[49]

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[44] Dated May 9, 1776, in *Cullen's Correspondence*, vol. III.

[45] Long consultation in French entitled "Mémoire a consulter," dated May 25, 1783 in New Orleans, concerning the 19-year old daughter of a captain of the Louisiana infantry, in *Cullen's Correspondence*, vol. VII.

[46] The patient wrote to Cullen: "I am therefore at all events determined to get rid of the running and I by no means despair of your medicines having that effect . . . Mr. Hunter in talking of my general complaint is quite the surgeon putting but little faith in the use of inward medicines. He talks of *knocking* off excrescences in the urethra as a mason would in ferming the stones to the building and if you would allow thus the urethral chiseler or in other words the caustick or Bougie he has but little idea of curing the disorder," dated June 6, 1782, in *Cullen's Correspondence*, vol. IX.

[47] ". . . the Seton in my back is doing very well. The pills have their proper effect and all my medicines agree with my stomach. But my left leg is still so insensible that at times I hardly feel when I touch any object with it and the right leg is fully as weak as ever I found it. The Electrifying machine I have got tho' it gives a severe shock to the arm when any person receives it at that part, is yet so weak as to be little felt at my knees. The instrument at St. Andrews gave me a shock vastly stronger . . .," dated September 21, 1777, in *Cullen's Correspondence*, vol. IV

[48] Cullen wrote: "(Opium) must be employed and used in the quantity that gentlemen upon the spot know to be requisite in and suited to his condition. I shall be sorry to induce a habit of opiates . . . ," July 4, 1789, in *Consultation Letters*, vol. XXI.

Among the drugs most frequently employed by Cullen were the astringents such as his cuprum ammoniacum—a copper preparation—prescribed in epilepsy. Realizing that the preparation was not always readily available abroad, Cullen carefully wrote out the prescription in Latin for a Parisian patient. The tonics were led by the Peruvian bark, used in intermittent as well as other fevers, respiratory ailments, dysentery, scarlatina, and even cases of smallpox. Moreover, Cullen prescribed a variety of botanical remedies as stimulants including cinnamon, nutmeg, gum guaiacum, juniper berries, all of them employed in the form of extracts and decoctions. His list of narcotics included camphor in addition to opium.[50]

Because of his emphasis on dietary manipulations in the treatment of disease, Cullen was extensively questioned by those consulting him, especially travelers, regarding the foodstuffs and wines available in foreign countries. To the Prussian official suspected of having gout, he wrote: "Avoid all Rhenish and other weak-bodied German wines. If French, Spanish, or Portuguese wines are used in Berlin, I take the two latter to be much safer than the former."[51] To another correspondent in Genova he declared: "The chicken or fish dishes in Italy are too rich." At times, even Cullen was at a loss since he had not traveled himself since his early experience as a ship surgeon to the West Indies. "I cannot properly direct her diet," he stated to one consultant, "because I don't know what the country affords during the winter season."

Some patients demanded a detailed knowledge of the European climate from Cullen. Although promoting Scotland—especially its summers—Cullen realized the nefarious effects of dampness on some of his correspondents suffering from rheumatic ailments. "Wear flannel everywhere next to the skin," he insisted. "Set off for some of the southern parts of Europe to pass the winter there." [52] But, by late April, the advantages of the milder climate seemed to fade, at least in Cullen's mind, for he strongly suggested that his patients sail back to Britain before the hot weather reappeared.

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[49] "I begin with a tempered water by putting to four parts of spring water of about 50° of Fahrenheit one part of boiling water, and with this giving hardly a feeling of cold I wash the whole body over. This I repeat every second day, but every second day I keep back one eight part of the boiling water, till I bring it to be of the temperature that it is at the spring, and if the child can be reconciled to it, having first washed his head and neck with it, I plunge the whole of his body in the water, and if he will not submit to the immersion, you must wash him all over by squeezing a sponge full of the water upon his head and shoulders, and following it over the rest of his body."

[50] For details consult Cullen's *A Treatise of the Materia Medica,* 2 vols. (Edinburgh: C. Elliot, 1789), especially vol. II, chaps I-VIII

[51] Letter to William Baylies in Berlin concerning the health of Mr. Heynitz, written in October 1784, in *Consultation Letters,* vol. XVII.

Closely related to his medical climatology was Cullen's knowledge of certain European spas, including the temperature and chemical composition of their waters. In one letter he was even asked about what portion of the northern French coast was ideal for sea bathing in the month of August but we do not have his reply.

Cullen's last consultation letter was dated December 26, 1789, and was addressed to a William Charters, Esq., who suffered from spasmodic asthma. A month earlier Cullen had already confided to an Irish colleague that he was not feeling well,[53] and subsequently he resigned his chair at the University. One of his sons, the physician, Henry Cullen, answered a few of the incoming letters in early January of 1790, intimating that it was extremely uncertain when the bedridden doctor would be able to resume his correspondence.[54] This, of course, never happened, since William Cullen died at Kirknewton, just outside of Edinburgh, on February 5, 1790, at the age of 79.[55]

Cullen's consultation practice sheds light on what a prominent university-based physician actually did during the last quarter of the eighteenth century. Contrary to expectations, Cullen's neural pathogenesis played only a minor role in his therapeutical efforts. Instead of attempting to make the cures entirely subservient to his theories, he wisely stressed diet, climate, living habits, and only used a few drugs which he had found empirically to be useful. Thus, like Boerhaave, Cullen was a physician attempting to systematize new explanations of physiology and pathogenesis without abandoning his conservative and eclectic methods of treatment. These forms of cure were naturally couched in terms related to Cullen's medical system, but the Scottish physician left no doubt about the bases for his assertions. "Upon the whole," he declared to his students, "we may depend upon Theory no more than is absolutely necessary. I shall endeavor to order my Plan (of lectures) so as to collect and communicate to you every Fact in Physic which Experience has afforded, and take care that no Conclusion I form shall be contradicted by any known Fact, or experienced Rule in Practice." [56]

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[52] Dated October, 1785, in *Consultation Letters,* vol. XVIII.

[53] Letter to a Dr. Smith in Dublin, dated November 11, 1789, in which he refers to the "unlucky time when I am just beginning my college under a very infirm state of health which my time of life necessarily brings on," in *Consultation Letters,* vol. XXI.

[54] Henry Cullen wrote to a Dr. Robertson on January 9, 1790: "Your letter of the 19th (December) was received by my father in course but he has been so much indisposed and being now confined to bed that he is unable to do any business," in *Consultation Letters,* vol. XXI.

[55] An unknown author composed the following excerpted poem following his death:

"Alas, great Master, Physic could not save
thy matchless worth from the devouring grave
thy healing hand off turn'd the darts of death
and kindled up the tyrant's fiercest wrath

. . . .

Thy stores of learning have enrich'd mankind
the purest treasures of the noblest mind
Ages to come shall feel thy healing pow'r
and Cullen live till Physic be no more."

Consultation Letters, vol. XXI.

Such an attitude laid Cullen open to the charge that his therapeutics were not "rational," or in other words, the various measures were not always logical and absolute consequences of the prevailing theoretical bases of health and disease. By contrast, it was Cullen's disciple, John Brown, and his adherents who claimed precisely such a single, rational, and logical method of treatment entirely based on a new theory of disease.[57] "To hold good in practice, is, in my opinion, the test of a theory . . . it astonishes me to hear a man daily labouring to instill into the minds of his pupils, a theory which he himself confesses is useless at the bedside," declared R. D. Willis, a Brownian medical student.[58]

Finally, Cullen's attitude, his devotion to the welfare of the sick, and the situations he encountered in practice did not essentially differ from our own. Thus, his consultation practice tends to support the notion that some of the basic tenets of the healing art have virtually remained unchanged through the ages.

[56] See Cullen's "History of the Practice of Physic," p. 73, MSS Collection, Royal College of Physicians of Edinburgh.

[57] See G. B. Risse, "The Brownian system of medicine: its theoretical and practical implications," *Clio Medica,* 1970, *5*: pp. 45-51.

[58] Paper delivered on February 8, 1783, at the Royal Medical Society in answer to the question "Do the inquiries after the modus operandi of causes improve the practice of physic?" in *Dissertations of the Royal Medical Society* XIV, pp. 70-77, MSS Collection at the University of Edinburgh. Willis, a follower of John Brown, had been admitted to the Society on December 15, 1781. He was matriculated at the University between 1781-1783 but did not graduate.

THE NOSE, AMBIENT AIR, AND AIRWAY MUCOSA. A PATHWAY IN PHYSIOLOGY

DONALD F. PROCTOR *

Pollution of our atmosphere, crowding of the increasing population into urban environments, and rapid travel from almost any part of our planet to another together have led on the one hand to an increase in airborne disease and on the other hand to the recognition of our need for better technological control of ambient air and more complete comprehension of the natural defences of our respiratory tracts. The current interest in air pollution has served to focus the attention of workers in hitherto separate fields of research upon a single goal, an understanding of the relation between man and his ambient air as it affects the individual's susceptibility to respiratory disease and the public health. The purpose of this paper is to survey a part of the story of how we reached our still imperfect state of understanding in this field of knowledge.

Our story has three major roots which reach far back into medical history. They involve our ambient air and the function of respiration, nasal anatomy and physiology, and respiratory mucus and mucous membrane physiology.

Progress in science seems to depend upon the conception of new ideas, new observations, methodology for their investigation, communication between workers in similar or widely disparate fields, and the development of unifying theses joining ideas and forming a foundation from which new ideas may spring or upon which new observations may be brought to bear. Communication between individual scientists is a vital part of this scheme. Today our methods of information retrieval and rapid, widespread communication are major factors in the growth of scientific knowledge. A troublesome and subtle obstacle to communication is the current high degree of specialization which has resulted in the formation of groups who attend their own meetings, read their own journals, speak their own jargon, and rarely turn to right or left from their central focus to examine related ideas. The subject of this paper involves rheologists and fluid flow engineers, respiratory physiologists

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and rhinologists, aerosol engineers and air pollution experts, industrial hygienists, students of infectious disease, immunologists and allergologists, architects and heating and ventilating engineers. Thus, the history of developments in this field of physiology is in part a story of communication between workers in these widely separated disciplines.

The Ambient Air and Respiration

As early as the Hippocratic writings[1] a relation between climate and disease was recognized; but the concepts described are difficult to fit into our contemporary knowledge because of the ancient lack of understanding of the very meaning of breathing or of infectious or noninfectious airborne disease. The variation of prevalent diseases with climate was self evident. What was not clear was the mechanism which related the ambient air to the body.

It is probable that Galen had the germ of an idea regarding the significance of breathing. He certainly saw the importance of the airways as bulwarks against ambient air although he had an exaggerated idea of the role of the uvula, and, of course, had no conception of mucociliary clearance of inhaled dust.

Galen said of the uvula, ". . . the incoming air is first divided by it, and the violent rush of the air and hence its coldness are broken up; . . ." He then goes on to say,

> . . . the apertures of the nose, how marvelously they come next after the spongelike [ethmoid] bone placed before the cavities of the encephalon, and how the connection was cut through into the mouth at the palate in order that inspiration may not begin in a straight line with the rough artery [trachea] and that the air entering it may first be bent and convoluted, so to speak. For I think this should be doubly advantageous: the parts of the lung will never be chilled when often times the air surrounding us is very cold, and the particles of dust, ashes, or anything else of the sort very frequently mixed with the air will not penetrate as far as the artery [trachea]. For the air can pass on its way in this winding path, but such particles are held back, encountering first the bodies [mucous membrane] which surround the bends and which are soft and moist, contain a sticky substance, and are capable for all these reasons of retaining what falls upon them. [Finally] those who wrestle in clouds of dust and . . . those who travel a very dusty road . . . presently blow their noses and cough up and spit out the dust. If, however, the channels in the nose did not first pass straight up toward the head and then turn obliquely back toward the palate, and if they did not find the uvula there to receive them, it is clear that

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[1] Hippocrates, *On Airs, Waters and Places,* transl. F. Adams (Classics of the St. Johns Program, 1941), p. 156.

> nothing would keep all such materials from falling into the artery [trachea]; for this is what happens if a person breathes through his mouth.[2]

Leonardo da Vinci[3] evidently observed something in lungs at post mortem which made him deduce that inspired dust was related to pulmonary disease. As shown in Figure 1, he noted (in his mirror writing) "polvere fa danno"—dust causes damage, evidently referring to the lungs shown just below. Leonardo failed to make the breakthrough which would permit an understanding of the meaning of respiration, evidently preferring to follow Galenic teachings in this regard.

Although in 1628 cardiovascular physiology was launched with Harvey's work,[4] the pitiful state of respiratory physiology 40 years later was expressed by Samuel Pepys when he noted on January 22, 1665, that at ". . . the first meeting of Gresham College since the plague . . . what, among other fine discourse pleased me most, was Sir. G. Ent about Respiration; that it is not to this day known . . . how the action is managed by nature, or for what use it is."[5] Ent, who had been a friend of William Harvey, enlarged upon these remarks and published them in 1679.[6]

Meanwhile Robert Hooke had laid one ancient belief to rest (the idea that the motion of the lungs in breathing served the sole but vital purpose of cooling the blood). He demonstrated before the Royal Society that motionless lungs kept aerated through bellows still sufficed to keep the animal alive.[7] In 1674 John Mayow published his *Tractatus Quinque*[8] which finally established the necessity for an exchange of gases in the lungs through breathing (Mayow's work remained relatively unknown until Priestley's discovery of oxygen in 1772).

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[2] Galen, *On the Usefulness of the Parts of the Body,* trans. M. T. May (Ithaca: Cornell Univ. Press, 1968), *II,* p. 525, Book XI, Section II, Paragraph 4.

[3] Leonardo da Vinci, *On the Human Body,* transl. etc., C. D. O'Malley, and J. B de C. M Saunders (New York: Henry Schuman, 1952), p. 172.

[4] William Harvey, *De motu cordis* (Springfield: Charles C Thomas, 1957, from the 1628 edition).

[5] *The Diary of Samuel Pepys* (New York: Limited Editions Club, 1942), vol. VII, p. 20 (Jan. 22, 1665).

[6] George Ent, *Animadversiones in Malachiae Thrustoni diatribum de respirationis usu primario* (Londini: Imp. Guil. Bromwich, 1679).

[7] Robert Hooke, "An Account of an Experiment, made by Mr Hook, of preserving Animals alive by blowing into their lungs with Bellows," *Philos. Trans.,* 1667, *2* (28)· 539-540.

[8] John Mayow, "Tractatus quinque medico-physici, Quorum primus agit de sal-nitro, et spiritu nitro aerae." *Medico-physical Works* (Alembic Club Reprint #17, Edinburgh, 1907, from the 1681 edition).

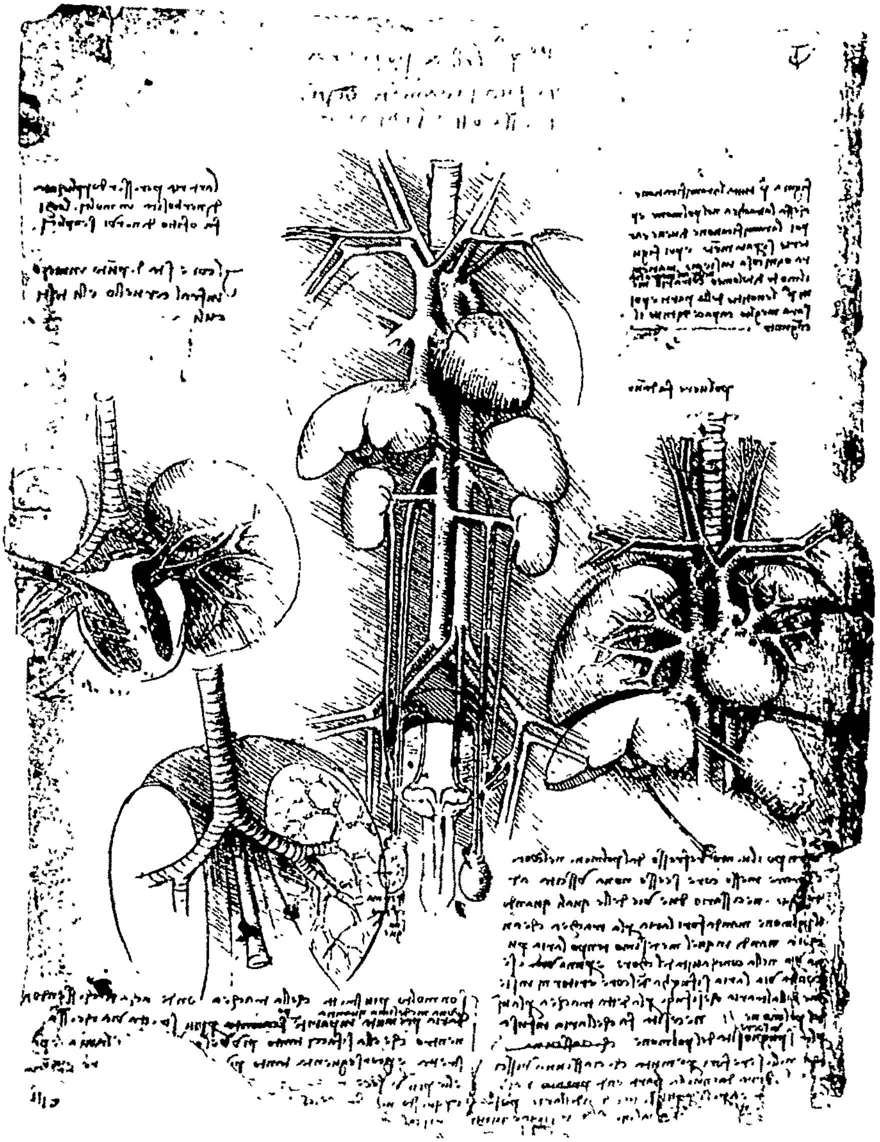

FIG. 1. In this Leonardo plate he has depicted cardio-respiratory anatomy in a manner to agree with his interpretation of the teachings of Galen. The three words above the drawing of the heart and lungs on the right are said to be "polvere fa danno" (in mirror writing), meaning "dust causes damage." I am grateful to Dr. David Bates for calling my attention to this illustration from Leonardo Da Vinci, *On the Human Body,* transl. C. D. O'Malley and J. B. de C. M. Saunders (New York: Henry Schuman, 1952), opp. p. 172.

Thus, at the beginning of the eighteenth century the reason for the necessity of breathing our ambient air was known although not generally recognized.

John Arbuthnot (1667-1735) has been given the dubious recognition of being included by LeFanu in the "lost half century in English Medicine,"[9] and condemned as a therapeutic nihilist whose writings contributed nothing which could not have come from an informed layman. Still, he did recognize that studies of the ambient air might cast useful light on human disease. Arbuthnot (who was the author of *The History of John Bull* and was physician to Queen Anne) in 1733 published his *Essay Concerning the Effects of Air on Human Bodies,*[10] in which he recommended ". . . the keeping a journal of the Weather and reigning Diseases, as a thing which might be of singular Use, especially to posterity. . . ." He also said, ". . . it seems preposterous that there should be so many minute Inquiries about the Qualities of every Drug which we take but seldom, and none into the Effects of a Substance that we take inwardly every moment"; and he noted that, "the Air of Cities is not so friendly to the Lungs as that of the Country, for it is replete with Sulphorous Steams of Fuel. . . ."

In the year 1829 James Clark, perhaps heeding the admonition of Arbuthnot, published *The Influence of Climate in the Prevention and Cure of Chronic Diseases,*[11] in which he gave extensive climate tables and offered a contemporary description of "consumption."

Meanwhile a concept of airborne disease had developed in the mind of young Jacob Henle (1809-1885) and in 1840 this culminated in the first paper in his *Pathologische Untersuchungen* entitled "Von den Miasmen und Kontagien."[12] This remarkable man (Professor of Anatomy of Zurich, and said to have been a skilful artist, poet, and musician) at the age of 31 years, through careful observation and deductive reasoning, was able to anticipate Pasteur and the coming age of microbiology with an hypothetical description of the spread of contagious disease through living microorganisms. Henle's treatise was an important step forward; but useful application had to await developments in microbiology, aerobiology, epidemiology, immunology, and nasal and mucosal physiology. The beginning of the latter had already occurred in 1830 with Sharpey to whom we shall shortly return.

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[9] W. R. LeFanu, "The lost half-century in English medicine 1700-1750," *Bull. Hist. Med.* 1972, *46*: 319-348.

[10] John Arbuthnot, *An Essay Concerning the Effects of Air on Human Bodies.* (London: J. Tonson, 1733).

[11] J. Clark, *The Influence of Climate in the Prevention and cure of Chronic Diseases more particularly of the Chest and Digestive Organs* 2nd. ed. (London, 1830).

[12] Jacob Henle, *Von den Miasmen und Kontagien* (On Miasmata and Contagia, 1840), transl. G. Rosen (Baltimore: Johns Hopkins Press, 1938).

The first American physician to enter our story is Morrill Wyman (1812-1903) who took his M.D. at Harvard in 1837 and practiced medicine in Cambridge. Evidently he divided his scientific endeavors between an interest in mechanics and the treatment of patients with respiratory disease. One major contribution to medicine was his development of a suction pump and its application, with Dr. H. I. Bowditch (not to be confused with H. P. Bowditch shortly to be mentioned), to thoracentesis in acute pleural effusion.[13] Of more interest to us is that he was perhaps the first to direct serious attention to the indoor climate,[14] a subject still relatively neglected today. Wyman also was a student of hay fever, numbered among his patients suffering from this malady Daniel Webster and Henry Ward Beecher, and, in 1876, published *Autumnal Catarrh (Hay Fever)*.[15]

In 1878 an architect, Robert Briggs, followed the lead of Wyman with a series of lectures, "On the relation of moisture in air to health and comfort."[16] He made the statement, the foundation for which is not made clear, that, "dry air, per se, of whatever temperature it may be found on the surface of the earth, is not unhealthy. . . ."

Shortly afterward, in 1884, John Shaw Billings (1838-1913) published *The Principles of Ventilation and Heating and their Practical Application*.[17] This remarkable man, who served as an army surgeon in the civil war (playing a major medical role at Gettysburg), is most famous today for initiating the *Index-Catalogue of the Surgeon General's Library* and the *Index Medicus*. In his day he was accepted as the leading authority on ventilation, especially in connection with hospital planning. He, like Briggs, stated that dry air in itself was innocuous. He served as the principle expert involved in the planning of the new Johns Hopkins Hospital and later designed the Peter Bent Brigham

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[13] E. C. Atwater, "Morrill Wyman and the aspiration of acute pleural effusions, 1850. A letter from New England," *Bull. Hist. Med.*, 1972, *46*: 235-256.

[14] Morrill Wyman, *A Practical Treatise on Ventilation* (Boston: James Munroe, 1846).

[15] Morrill Wyman, *Autumnal Catarrh (Hay Fever)* (New York, Hurd & Houghton, 1876).

[16] R. Briggs, "On the relation of moisture in air to health and comfort," *J. Franklin Institute,* 1878, 75: 10-20, 82-93, 168-179, 251-255.

[17] John Shaw Billings, *The Principles of Ventilation and Heating and their Practical Application* (New York: The Sanitary Engineer, 1884).

Hospital in Boston. He continued as a consultant to the Board of Trustees of the Hopkins Hospital for thirteen years.

Thus far we have seen little recognition of the role of the nose in the defence of human health. Among the earlier champions of this cause was a lawyer turned artist, explorer, and anthropologist. In 1832 George Catlin (1796-1872) abandoned a law practice, studied painting at the Peale Museum in Philadelphia, and left for the upper Missouri River country to begin a life of study of North and South American Indians. As far as possible Catlin sought contact with Indians who remained in a more or less primitive state, largely untouched as yet by the rapidly advancing white man's "civilization." He found himself enthralled by a sort of epidemiological study of these peoples.[18] Admittedly handicapped by the lack of written records, he used a variety of imaginative methods to ascertain the prevalence of disease, especially that causing infant and child mortality. He became convinced that, with the exception of accidental deaths, infant and child mortality was exceedingly low in comparison to that in civilized communities of the time. His studies involved 150 different tribes and two million "souls."

Not satisfied with a wealth of information on this general subject, nor with the hundreds of fine paintings he did of these people and their customs and habitats, he sought for some explanation of their relatively healthy condition. The result was a book published in 1861,[19] in which he preached the doctrine of nasal breathing as the way to health and happiness. He ends with the heartfelt exhortation to us all, "And if I were to endeavor to bequeathe to posterity the most important Motto which human language can convey, it should be in three words—Shut-your-mouth" (Figure 2). Only in the past decade have serious scientific studies been directed at an understanding of the wisdom of those words.

Nasal Anatomy, Physiology, and Mucous Membrane

The living anatomy of the nasal passage is difficult to study even today with our electrically lighted and radiographic devices. Although Leonardo [20] and Vesalius [21] provided illustrations of the bony anatomy of the

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[18] P. P. Thomas, "George Catlin: pictorial historian of aboriginal America," *Natural History,* Dec. 1972, *81* (10), 30-43.

[19] George Catlin, *The Breath of Life; or mal-Respiration. and its effects upon the enjoyments and life of man* (New York: J. Wiley, 1861).

[20] Leonardo da Vinci (note 3 above).

[21] A. Vesalius, *De humani corporis fabrica* (Basel, 1543).

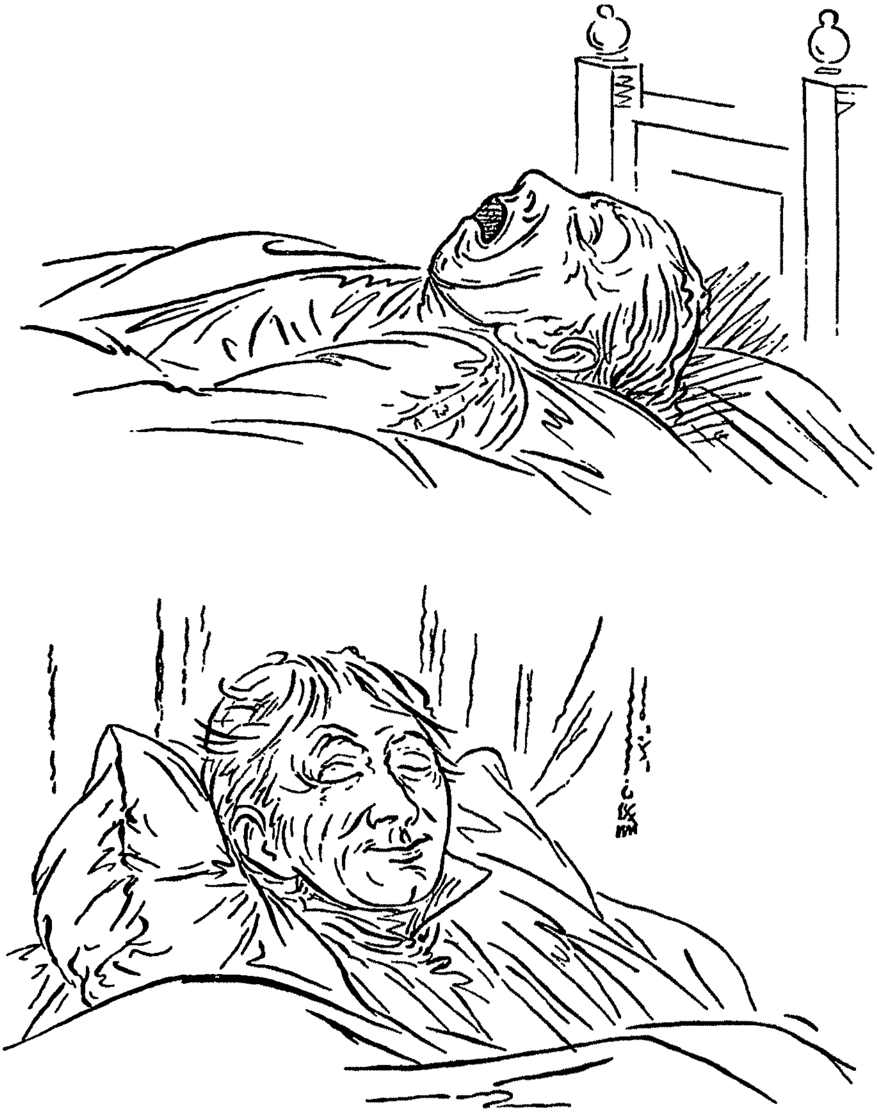

FIG. 2. Page 22 from Catlin's *The Breath of Life* (New York: J. Wiley, 1861). The book is replete with such vivid drawings showing the horrors of mouth breathing, and the joys of keeping one's mouth shut. Courtesy of the National Library of Medicine.

region, and Highmore[22] and Du Verney[23] and later still Hilton[24] cast further light on the subject, those who gave any consideration to nasal physiology seemed ignorant of the true nasal airway until well along into the nineteenth century. A partial exception appears to have been Du Verney, although he saw the nose as the "organ of smelling." According to the abridged version of his paper in the *Philosophical Transactions* he observed that "the cavities of the nose are filled with many cartilaginous laminae distinct from each other . . ." and that nature "has furnished the said tunic of the nose with a great many small glands, which open into it, and so moisten it with a thick and slimy exudation, the better to entangle the dry odorous particles."

Marie François Xavier Bichat (1771-1802), an army surgeon who died at the age of 31 years, found time to study and write extensively about mucous membranes. He was at least aware of the possible importance of the ambient air to the nasal mucosa.[25]

Among the contributions of François Magendie (founder of the *Journal de physiologie expérimentale,* the first journal to be devoted to physiology) was his recognition that the nose warmed and moistened the inspired air, a matter subjected to extensive investigation ever since.[26]

The recognition that the nasal mucosa was in large part erectile tissue, similar morphologically to that found in the genital tract, was an important advance. The story of the development of this concept is a complex one reviewed by Mackenzie[27] and by Bigelow.[28] It seems likely that the full recognition of the nature of nasal vasculature was a stepwise process with the credit going to no one person. In any event it was well recognized before 1885, and this made possible an understanding of the rapidly changing nasal airway and the ability of the nose to adapt, as an air conditioning system, to changing ambient conditions.

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[22] N. Highmore, *Corporis humani disquisitio anatomica* (The Hague, 1651)

[23] Joseph Guichard du Verney, Anatomical observations on the structure of the nose. *Philos. Trans.* (abridged), 1678, *2*: 432-433.

[24] John Hilton, *Notes on some of the Developmental and Functional Relations of Certain Portions of the Cranium* (London: Churchill, 1855).

[25] M. F. X. Bichat, *Traité des membranes en général et diverses membranes en particulier* (Paris: chez Mme. Veuve Richard, 1802), pp. 54-55.

[26] François Magendie, *Physiology,* transl. E. Milligan, 3rd ed. (Edinburgh, 1829), p. 387.

[27] J. N. Mackenzie, "Historical notes on the discovery of the nasal erectile tissue," *Boston Med. Surg. J.,* 1885, *112*: 1-2.

[28] H. J. Bigelow, Turbinated corpora cavernosa, *Boston Med. Surg. J.,* 1875, *92*: 489-492.

Thus, as the nineteenth century was entering its last quarter, investigations were being directed at many of the problems whose solution was essential to an understanding of man, his ambient air, and airborne disease. What was lacking was a binding together of the information available to form a unifying thesis on the basis of which new ideas could flourish. Between 1875 and 1900 some of this was to be accomplished.

Except for Catlin's admonition to stick to nasal breathing as a public health measure, scant and periodic attention had been devoted to the nose as a defence organ. With the latter part of the nineteenth century an increased interest in the anatomy, physiology, and disease of the nose was apparent. It is not clear to me what impetus produced this change.

In 1875 one of the leading Boston surgeons, Henry J. Bigelow (1816-1890) published a paper in which he linked the nature of nasal vasculature, environmental stimuli, and the symptoms of a cold. Dr. Bigelow (Professor of Surgery at Harvard and a witness to the first operation in which Morton administered ether for anesthesia) called attention to the function of the nasal turbinates and their erectile tissue.[29] He pointed out that an understanding of the "erectile tissue" of the nose could "elucidate, if it did not alleviate" the symptoms of a cold. He also noted that "a swallow of water, a pinch of snuff, a sudden start, mental or physical . . . often clears the passage, to be again filled up." Here at least was someone observing the nose as a physiological organ capable of responding to a variety of stimuli.

Devotees of P. G. Wodehouse may remember his description of Lord Emsworth's relief from symptoms of his cold. After a sudden severe mental shock "he was no longer sneezing. It is a remarkable fact, and one which will interest medical men, that of all the remedies for the common cold which had been suggested to him that night, the shock he had received from Gally's revelations was the only one that had done him any real good." A more recent and detailed listing of the respiratory symptoms arising from emotional stimuli appears in a song in the musical *Guys and Dolls*.

In 1888 Francke H. Bosworth (1843-1925), of the Bellevue Hospital in New York, drew together the observations of many writers into a surprisingly advanced view of nasal physiology.[30] He quoted Magendie's observation, "By successively passing the mouth, nasal cavities, pharynx, larynx, trachea and bronchi; the inspired air is charged with vapor which it carries away from the mucous membrane of the air-passages, and in this state always hot and humid, it arrives at the pulmonary lobules." Bosworth noted that the nose had three functions, olfaction, phonation, and respiration, and said that mucus is secreted "only in such quantities as are sufficient to keep the membrane in a soft and pliable condition." He estimated that mucus was 93% water and 7% solids (in good agreement with findings today). He calculated that "5,000 grains" of water were added to the inspired air each 24 hours and attributed this to a transudation from the erectile tissues. He quoted and agreed with Aschenbrandt's statement that "all mechanical dust" was "deposited on the moist surfaces of the nasal membrane," noting that Kayser had found the nose not a perfect filter for dust. These statements have only been documented in the past decade. Because the importance of the nasal filter was not appreciated he added that this was probably an adventitious function—"certainly it is a very unimportant one"—a statement only to be refuted in recent years.

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[29] *Ibid.*

[30] Francke Huntington Bosworth, "The physiology of the nose," *Medical News*, 1888, *53*: 117-124.

As far as I have been able to discover Bosworth contributed little in the way of new investigation or even new ideas; but he filled that role which science, and especially medicine, needs periodically—the admirable student who reads widely and thinks on a broad basis. He gathered together much of the then available information, organized it and presented it in a lucid and enjoyable format. One of his conclusions, quoted from an earlier lecture on allergic diseases given before the Climatological Association is particularly worth citing here, "This, then, is the great and prominent function of the nasal chambers, so to prepare the ingoing current of air that it shall exercise no injurious influence on the mucous membrane of the passages below."

Aschenbrandt[31] had noted the role of the nose in heat elimination, and in 1888 Greville MacDonald used ingenious methods to further elucidate the air conditioning function of the nose.[32] He noted that "air passing through the nose is always saturated with water as well as raised to the temperature of the blood" and that shrinking of the nasal mucosa reduced its efficiency in this regard. He also appears to have been the first to clearly recognize the difference between inspiratory and expiratory nasal airflow. At this time Zuckerkandl's (1849-1910) well illustrated monograph on normal and pathologic anatomy of the nose appeared.[33] In spite of this greatly improved anatomy as late as 1894, because of the difficulty of making the requisite measurements, Franke [34] was led to mistaken conclusions as to nasal air flow.

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[31] T. Aschenbrandt, *Die Bedeutung der Nase fur die Atmung* (Würzburg: Stahel'schen Univers-Buch und Kunstha, 1886).

[32] Greville MacDonald, "On the mechanism of the nose as regards respiration, taste, and smell," *Brit. Med. J.*, 1888, *2*: 1210.

A connection between the nose and sexual function was the subject for ancient speculation. Sterne's "Tale of Slawkenbergius" in his *Tristram Shandy* is an amusing example of this observation, and Rostand's *Cyrano de Bergerac* is filled with allusions to the many marvelous ancillary functions attributed to the nose. Sterne's Slawkenbergius tale ends with this sweeping statement, "Alas! alas! cries Slawkenbergius, making an exclamation—it is not the first—and I fear will not be the last fortress that has been either won—or lost by noses."

John N. Mackenzie (1853-1925) (Clinical Professor of Laryngology at the new Johns Hopkins Hospital) not only wrote on the history of the discovery of nasal erectile tissue,[35] but in 1898 published a delightful treatise on the "Physiological and Pathological Relations between the Nose and the Sexual Apparatus in Man." [36] In the same year Grayson [37] addressed himself to the same subject, perhaps one indication that the Victorian age was drawing to a close? It was not until the late nineteen forties that more serious attention and careful investigation was to be directed at this subject of emotional stimuli and nasal function.[38]

Respiratory Mucus and Mucociliary Function

The next chapter requires a step backward of several centuries. Possibly because of the association of headache with sinusitis, and perhaps enhanced by the occurrence of cerebrospinal rhinorrhea with head injuries, from ancient times nasal mucus was assumed to be a discharge of excess fluid from the brain, especially from the pituitary gland.

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[33] E. Zuckerkandl; *Normale und pathologische Anatomie der Nasenhöhle und ihren pneumatischen Anhange* (Wien: Wilhelm Braumuller, 1882).

[34] G. Franke, "Experimentelle Untersuchungen über Luftdruck, Luftbewegung und Luftwechsel in der Nase und ihren Nebenhöhlen," *Arch. für Laryngologie und Rhinologie*, 1894, *1*: 232-249.

[35] See n. 27 above.

[36] J. N. MacKenzie, "Physiological and pathological relations between the nose and the sexual apparatus in man," *Bull. Johns Hopkins Hosp.*, 1898, *9*: 10-17.

[37] C. P. Grayson, "Some notes concerning the influence of sexual excitement upon intranasal disease," *JAMA*, 1898, *30*: 404-406.

[38] T. H. Holmes, H. Goodell, S. Wolf, and H. G. Wolff, *The Nose: An experimental study of reactions within the nose in human subjects during varying life experiences* (Charles C Thomas: Springfield, 1950).

Galen did not question this belief; but evidently his remarkable sense of physiology instinctively told him something was missing, the something which turned out later to be mucociliary clearance. He says, "Now nature has made no organ for clearing away phlegm, this being cold and moist, and, as it were, half digested nutriment. . . . And perhaps one cannot properly give the name of phlegm to the surplus-substance which runs down from the brain, but one should call mucus or coryza—as, in fact, it is actually termed. . . ." [39]

Van Helmont belaboured with great ardor the beliefs that a wide variety of ills throughout the body were related to "catarrh" or the "excrement of the head." He begins his chapter [40] by saying, "It is now a seasonable time to show, that the great heap of Diseases which hath been dedicated to a Catarrhe or Rheume flowing down from the Head, even into the very tops of the Toes, without let or hindrance, is an old wives tale. . . ." He was especially interested in proving that lung and pleural disease did not so derive.

In 1655 Schneider [41] (1614-1680) (for whom the "Schneiderian membrane" term is applied to nasal mucosa) and later, in 1670, Lower [42] showed that fluids could not travel from within the cranium in that manner. Between them they put an end to recipes for "purging the brain." Schneider's three books on "catarrh" are, in their original Latin (which I make no claim to have translated), 257, 464, and 600 pages long. Indeed, this elucidation of the admittedly controversial and important point was so long winded that the historian Jonathan Wright [43] was to say later with some justification, "Never was the kernel of an important fact so wrapped up in the husks of verbosity." Even as late as 1751 Théophile de Bordeu (1722-1776) (a student of Descartes, Rabelais, and Montaigne) found it necessary to comment on this earlier belief in his *Recherches anatomiques sur la position des glandes et sur leur action.*[44]

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[39] Galen, *On the Natural Faculties,* transl. A. J. Brock (Harvard Univ. Press, 1947), p. 215, Book II, IX, paragraph 11.

[40] John Baptist Van Helmont, *Oriatrike or physick refined,* transl. J. C[handler] (London: Lodowick Loyd, 1662) pp. 429-450.

[41] C. V. Schneider. *Liber primus (secundus, tertius) de catarrhis* (Wittebergae: T. Mevii & E. Schumacheri, 1660).

[42] R. Lower. *Dissertatio de origine catarrhi in qua ostenditur illum non provenire a cerebro.* (Tractato de Corde) (Londini: typ J. Redmayne, 1670), pp. 221-239.

[43] J. Wright. *The Nose and Throat in Medical History* (St. Louis: Matthews, 1898).

[44] Théophile de Bordeu. *Recherches anatomiques sur la position des glandes et sur leur action* (Paris: G. F. Quillau, Père, 1751).

Perhaps the weighty character of these early deliberations on mucus accounts for the dearth of interest in the subject during the ensuing 250 years. Indeed, as already stated, as late as 1888 Bosworth[45] was denigrating the importance of mucus. In 1714 the great *Bibliotheca anatomica, medica, chirurgica,* etc.[46] contained a chapter with the following high sounding heading: "An entire History of all the fluids that are contained in the Humane Body, as Chyle, Milk, the Semen Virile, Pancreatick and Biliary Juices, the Lochia, and other Female Excretions; Serum, Lympha, the Humour of the Pericardium, Saliva, Tears or the Liquor flowing from the Eyes, the Mucus or Snot of the Nose, Ear wax, Urine, Sweat, the Matter of insensible Transpiration; of Spirits called Animal, Succus Nutritius, etc." The chapter contains nothing of interest to our history.

The major source of interest in mucus was the troublesome clinical problem of differentiating it from "purulent" secretions. In 1780 an essay was published which had been awarded a medal for work in that field. Its author was Charles Darwin, son of Erasmus Darwin and uncle of the Charles Darwin famed for his *Origin of the Species.*

Young Darwin had a short and tragic life. He is said to have inherited his father's stammer and was early sent to France in the hope that mastery of that tongue might cure him. Interestingly enough he never stammered when speaking in French, although this trouble in his native language continued. At 16 he attended Christ-Church College in Oxford, but there he "thought the vigour of the mind languished in the pursuit of classical elegance . . . and sigh'd to be removed to the robuster exercises of the medical school of Edinburgh."[47] This transfer was accomplished and it was there he did the interesting investigation reported in his prize winning essay.[48] Tragically, he cut himself performing an autopsy on a child dying of "hydrocephalus internus," that evening suffered severe headache followed by fever, delirium, and petechiae and he died on the 15th of May, 1778, not quite 20 years of age, two years before his essay would be published (Figure 3). At the end of this small volume is a touching letter of thanks from Erasmus Darwin

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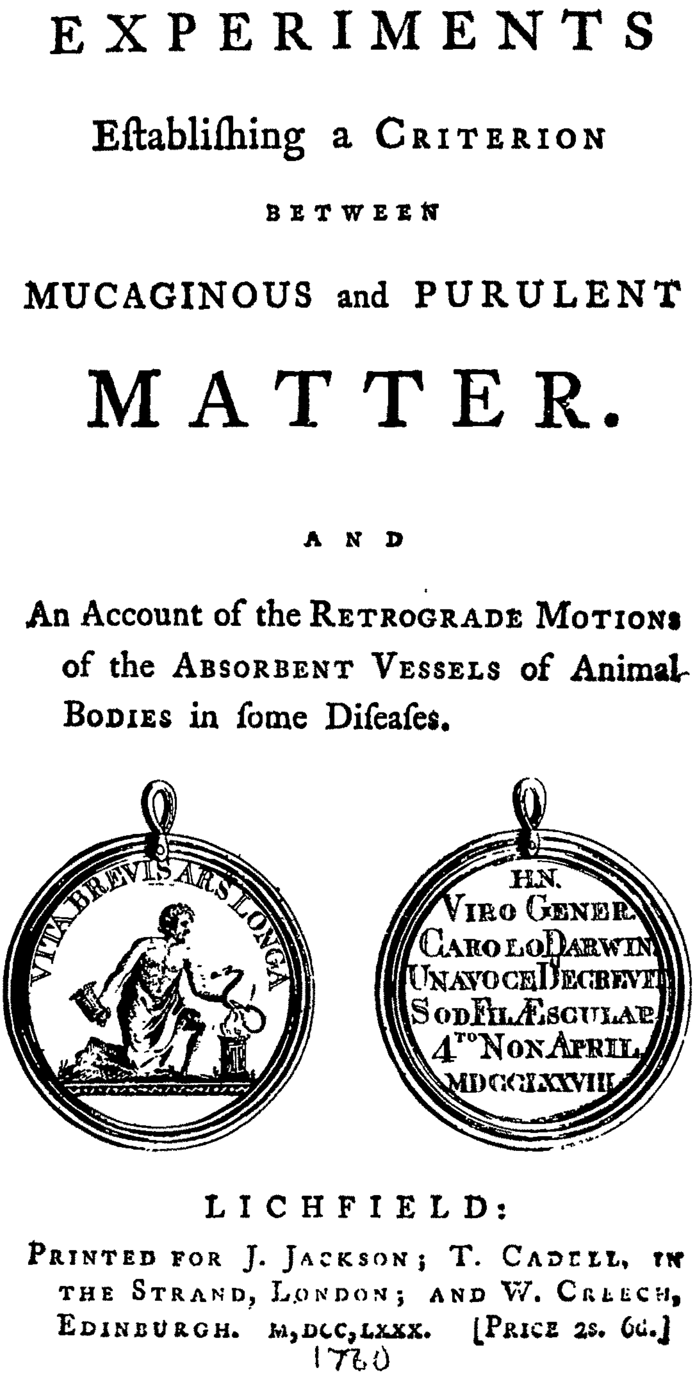
[45] See n.30 above.

[46] *Bibliotheca anatomica, medica, chirurgica, etc.* Containing a Description of the Several Parts of the Body (London: John Nutt, 1711), vol. 3: 242-258.

[47] E. Krause. *Erasmus Darwin.* Transl. W. S. Dallas (New York: D. Appleton, 1880).

[48] Charles Darwin. *Experiments Establishing a Criterion Between Mucaginous and Purulent Matter* (Lichfield, 1780), and *An Account of the Retrograde Motions of the Absorbent Vessels of Animal Bodies in Some Diseases,* with dedication to Dr. Andrew Duncan by E. Darwin, and Life of the author.



EXPERIMENTS

Eſtabliſhing a CRITERION

BETWEEN

MUCAGINOUS and PURULENT

MATTER.

AND

An Account of the RETROGRADE MOTIONS
of the ABSORBENT VESSELS of Animal
BODIES in ſome Diſeaſes.

VITA BREVIS ARS LONGA
H.N.
VIRO GENER.
CARO LO DARWIN
UNA VOCE DECREVIT
SOD FIL ÆSCULAP.
4TO NON APRIL.
MDCCLXXVIII

LICHFIELD:

PRINTED FOR J. JACKSON; T. CADELL, IN
THE STRAND, LONDON; AND W. CREECH,
EDINBURGH. M,DCC,LXXX. [PRICE 2s. 6d.]

1780

FIG. 3. The title page of Charles Darwin's essay.

to Dr. Andrew Duncan who was evidently young Charles' preceptor and friend.

The conclusions from Darwin's experiments were:

> From the above experiments it appears, that *strong vitriolic acid and water; diluted vitriolic acid;* and *caustic alkaline lixivium and water* will serve to distinguish Pus from Mucus: that the vitriolic acid can separate it from coagulable lymph, and alkaline lixivium from serum.
>
> And hence when a person has any expectorated material, the composition of which he wishes to ascertain, let him dissolve it in vitriolic acid, and in caustic alkaline lixivium; and then add pure water to both solutions; and if there is a fair precipitation in each, he may be assured that some Pus is present; if in neither a precipitation occurs, it is a certain test, that the material is entirely Mucus; if the material cannot be made to dissolve in alkaline lixivium by time and trituration, we have also reason to believe that it is Pus.

Darwin ends his essay, "Thus having finished the task I had undertaken, I hope, should I fail of obtaining the honour of your medal, these experiments may be of some service, as corroborating any better test, which may be presented to you; should I succeed, I need not say, that gratitude will always bind me to the Aesculapian Society, and that I shall be happy in having contributed my mite to ascertain an important circumstance in the practice of medicine."

Alas, the task he undertook is not finished even today as the chemical nature of mucus and its variants with disease are only now gradually being learned. Bostock[49] did further work along this line but also failed to make a major breakthrough in this diffcult problem.[50]

Now we take one more step backward to pick up the origins of our knowledge of mucociliary function. Thus far our tale has involved physicians and other scientists, two painters (one an explorer), an architect and a lexicographer. Here we add to this list the Dutch draper, janitor, and part time microscopist, Antony v. Leeuwenhoek.

In 1677 Leeuwenhoek sent the eighteenth of what were to total 375 letters to the Royal Society in London. Since this letter is usually quoted as the first description of cilia we might pause to consider the document. It is most commonly read in its abridged version.[51] When I tried to obtain a copy of the full version I learned that the original letter, written in Dutch, had been sent to Henry Oldenburg who, in translating it for the *Transactions*, took the liberty of condensing it to about half its original length. Those wishing to consult a translation of the entire original letter (a rewarding experience) must turn to Dobell's book on Leeuwenhoek.[52] The letter is entitled, "Observations on Animalcula seen in Rain, Well, Sea, and Snow water; as also in Pepper-water." It is clear that Leeuwenhoek really did observe cilia in a variety of forms and described them repeatedly with remarkable clarity.

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[49] J. Bostock. "Observations and experiments for the purpose of ascertaining the definite characters for the primary animal fluids and to indicate their presence by accurate chemical tests; and, Second essay on the analysis of animal fluids," *J. Nat. Phil. Chem. Arts,* 1805; *11*: 244 ff.; 1806, *14*: 140 ff.

[50] Dr. Edwin Keal, Brompton Hospital, London, has an excellent summary of the history of biochemical studies of mucus after Darwin in his University Thesis on "Neuraminic Acid in Chronic Bronchitic Sputum."

The following quotation may serve to whet the appetite of those who have not read his letter. "I also discovered a second sort of animalcules . . . provided with divers incredibly thin little feet, or little legs, which were moved very nimbly. . . . And albeit I could as yet distinguish no feet on the said animalcules . . . I imagined that they must be furnished with many very little ones; for the smallest animalcules were driven away from the bigger ones by the motion which these made in the water . . . just as if you were to blow a feather from your mouth." We hear nothing more of these "little feet" for 150 years.

Mucous membranes were the subject of at least two interesting books by Bordeu [53] and Bichat [54] in 1767 and 1802; but an appreciation of the role of respiratory mucosa in defence had to await recognition that ciliated mucous membrane could produce an organized motion in fluids contiguous to its surface.

Finally in 1830 William Sharpey began that phase of our story with his delightful little paper, "On a peculiar motion excited in fluids by the surfaces of certain animals." [55] Although this paper can only be considered a minor event in this remarkable man's career, it was indeed a monumental advance in the understanding of the role of mucociliary function in respiratory physiology and disease.

William Sharpey was born in Arbroath, Scotland, April 1st, 1802. He attended the University of Edinburgh and completed his medical studies there, being admitted to the Edinburgh College of Surgeons in 1821. He continued his studies in London and Paris and evidently practised medicine briefly in Arbroath; but, by the end of 1826 he became impatient to learn more pure science and spent most of the next three years traveling on the continent, frequently with his knapsack on his back. He went from France to Switzerland, Rome, Naples, Bologna, Padua, Venice, Innsbruck, Berlin, Heidelberg and Vienna. In 1829 he returned to Edinburgh where he taught physiology under Dr. Allen Thomson, and it was evidently while there that he made his initial observations on ciliary activity at the age of 28.

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[51] Antony V. Leeuwenhoek. *Observations on animalcula seen in rain,* well, sea, and snow water; as also in pepper-water," *Philos. Trans.*, 1677, *12*: 821-831 *Philos. Trans.* (abridged), 1809, *2*: 374-377.

[52] C. Dobell. *Antony van Leeuwenhoek and His "Little Animals"* (London, Baltimore: Harcourt, Brace, 1932), pp. 112-166.

[53] Théophile de Bordeu. *Recherches sur le tissu muqueux, ou l'organe cellulaire et sur quelques maladies de la poitrine* (Paris: Pierre France. Didot le Jeune, 1767).

[54] See n.25 above.

[55] William Sharpey. "On a peculiar motion excited in fluids by the surfaces of certain animals," *Edinb. Med. Surg. J.*, 1830, *34*: 113-122.

In 1836, while he was still only 34 years of age, he was appointed to the Chair of Anatomy and Physiology at the University of London (now University College) (Figure 4). This was the first recognition of physiology as a discipline in any English medical school. Sharpey continued in this capacity for 38 years during which time he was without doubt the major influence in physiology in Britain. In Geison's paper on the stagnancy of English physiology, 1840-1870 [56] he states, "During this same period, physiology was represented at University College by William Sharpey. . . . In the England of his day, Sharpey was the only teacher who did not simultaneously practice medicine or serve on a hospital staff. . . . When after 1870 English physiology enjoyed a great renaissance, it was led by men who had once studied or worked under Sharpey, Michael Foster, John Burden Sanderson, and Edward A. Schafer [Sharpey-Schafer]." At his death (from bronchitis in 1880) the obituaries spoke eloquently and warmly of Sharpey's capacity as a teacher, investigator, and valued friend of many.[57] In 1871, the Edward Albert Schafer (1850-1935) mentioned above in Geison became the first Sharpey Scholar at University College. An indication of the affection and respect which Sharpey engendered in his students is the action of this great physiologist when he prefixed Sharpey's name to his own and he, as well as his son E. P. Sharpey-Schafer, are known to this day under that name.

Because of the inadequacy of the magnification of his microscope Sharpey did not immediately recognize the role of cilia in the fluid motion he observed; but he pursued this observation and published

[56] G. L. Geison. "Social and institutional factors in the stagnancy of English physiology, 1840-1870," *Bull. Hist. Med.*, 1972, *46*: 30-58.

[57] Obituaries of W. Sharpey. *Nature,* 1880, *21*: 567-568; *Proc. Roy. Soc.* 1880, *31*: 10-19, 1880.

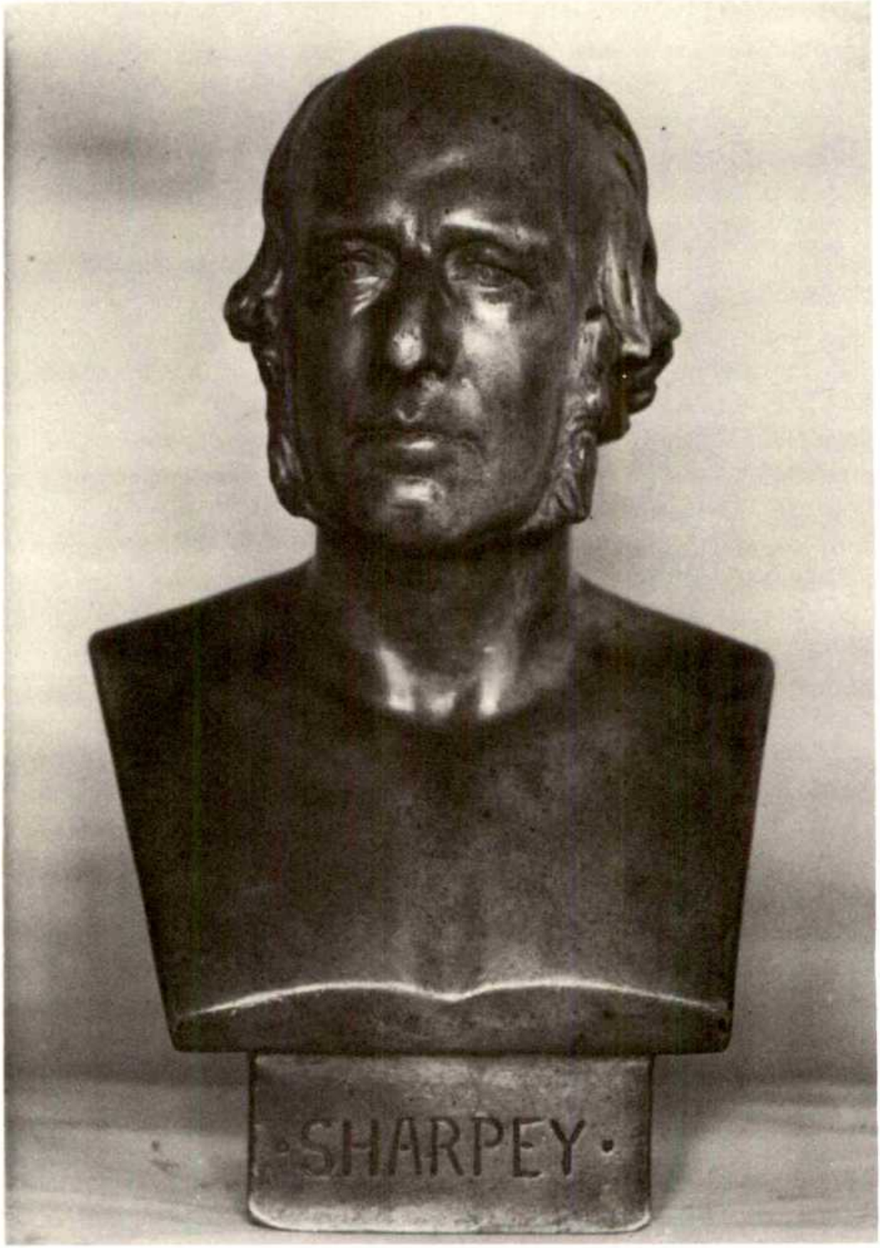

FIG. 4. Bust of William Sharpey in the entrance hall, Department of Physiology, University College London. This photograph was kindly sent to me by Professor D. R. Wilkie (Jodrell Professor of Physiology, University of London) who now holds the chair once occupied by William Sharpey.

further investigations along with a translation of the report of Purkinje and Valentin of their similar findings at about the same time.[58] In this 1835 paper [59] in reference to his studies in rabbits he notes, "The ciliary motion was particularly strong in the nasal cavities, and, as noticed by Purkinje, continued longer in this than in other situations." He noted anterior motion of particles in some parts of the nose and also observed motion toward the sinus orifice in the maxillary antrum.

One statement from the Purkinje paper as translated by Sharpey is especially interesting, "For, by its means, the secretions of those mucous membranes on which it occurs may be conveyed onwards, and many singular phenomena may perhaps be accounted for in this way. Thus, for instance, when the bronchial mucus accumulates during a long uninterrupted sleep, and is afterwards discharged, we do not bring it up from the interior of the lungs, but only from the larynx or top of the windpipe."

Sharpey's 1830 paper opens, "In the course of some investigations on the development of the tadpole in which I was lately engaged, I was accidentally led to observe, that the surface of that animal possessed the power of exciting currents in the water contiguous to it, in a constant and determinate direction."

"Accidentally led"—in research I suppose only those who have experienced the joy of pursuing an accidental observation to a fruitful goal can appreciate the meaning of serendipity. I sometimes wonder why people, especially physicians, choose to devote part or all of their lives to research. There must be many motivations, ambition for fame, attainment of a respected position, simple determination to find the answer to a question, money (although this can hardly be true of the physician abandoning a lucrative practice), and others. But, I feel that one motivation is probably more common and more forceful than the others, the joy of doing what one enjoys more than anything else; and a peculiar form of that joy is associated with serendipitous experience. Perhaps Sharpey's "accidental" discovery of ciliary activity at the age of 28 years was one of the factors which impelled him to devote the remainder of his life to physiology instead of returning to medical practice.

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[58] J. E. Purkinje and G. G. Valentin. "Entdeckung continuirlicher durch Wimperhaare erzeugter Flimmer bewegungen, als eines allgemeine Phänomens in der Klassen der Amphiben, Vögel and Saugethier." *Müller's Arch. für Anat., Physiol. u. wissensch. Med.,* 1834, 391-409.

[59] W. Sharpey. "Account of the discovery by Purkinje and Valentin of ciliary motions in reptiles and warm-blooded animals; with remarks and additional experiments." *Edinb. New Philos. J.,* 1835, *19*: 114-129.

Johann Evangeliste Purkinje (1787-1869) was also a most interesting man. Like Sharpey he was a Professor of Physiology (at Breslau and Prague). He was the first to use a microtome, the discoverer of the Purkinje cells (ganglionic cells in the cerebellum), the first to classify fingerprints, and a friend of Goethe who was apparently attracted to him after reading his graduation dissertation on subjective visual phenomena. Again, like Sharpey, his contribution to our field of interest must rank as a relatively minor part of his career.

Robert Edmond Grant (1793-1874) was Professor of Comparative Anatomy and Zoology at the same school as Sharpey from 1827 to 1874. This prolific scientist and lecturer also became interested in and wrote on the subject of cilia; [60] but I have been unable to uncover any evidence that the two were in close communication. Grant had observed and described the action of cilia in the Gasteropodus Mollusca in 1827; but Sharpey, who was still in Edinburgh at the time, was evidently unaware of this paper in 1830. Sharpey does refer to Dr. Grant in one of his later papers.

Interesting observations on cilia were reported in 1834 by Joseph J. Lister [61] (1786-1869) and Edward Forbes (1815-1854) in 1841.[62] Lister (a London wine merchant and father of the great Lord Lister of antiseptic surgery fame) noted that the surfaces which he studied were, ". . . lined with closely set ciliae, which by their motion, cause the current of water . . . the effect upon the eye is that of delicately-toothed oval wheels revolving continually . . . but the ciliae themselves are very much closer than the apparent teeth, and the illusion seems to be caused by a fanning motion given to them in regular and quick succession, which will produce the apparent waves. . . ." Who today could write a better description of this phenomenon? These observations of Mr. Lister are referred to in Sharpey's 1835 paper. Forbes finished his brief but interesting report by saying, "A minute inquiry into the nature of the involuntary vibratile cilia seen on mucous surfaces among the higher animals is most desirable."

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[60] R. E. Grant. "On the existence and uses of Ciliae in the young of the Gasteropodous Mollusca, and on the causes of the spiral turn of Univalve Shells," *Edinb. J. Sci.*, 1827, *7*: 121-125. Lecture 50, "On the respiratory organs of the invertebrated animals," *Lancet*, 1833-1834, *2*: 957-968

[61] J. J. Lister. XVIII. "Some observations on the structure and functions of tubular and cellular polypi and of ascidiae." *Philos. Trans.*, 1834, part 2, 365-388; see p. 378.

[62] E. Forbes. Note on the cause of ciliary motion. *London & Edinb. Month. J. Med. Sci.*, 1841, *1*: 27-28.

Although the work of Sharpey, and of Purkinje and Valentin, laid the groundwork for rapid advances, there appears to have been a period of about 40 years in which mucociliary function was considered more a subject for curiosity than for intensive study. Jeffries Wyman[63] (brother to the above mentioned Morrill Wyman, and Professor of Anatomy at Harvard) was among those who devised interesting techniques for demonstrating ciliary activity to students.

In the same year as Wyman's report an army staff surgeon in India, Henry Veale, sent a brief but fascinating report to *The Lancet.*[64] Only 16 years after Garcia's initial report to the Royal Society on the use of indirect laryngoscopy[65] this gentleman was using that tool most effectively. Evidently the descriptions of ciliary activity were unknown to him; but (with the aid of what must have been a most cooperative patient and marvelous vision) he described in beautiful detail movement of discrete particles of mucus from the bronchi ascending the trachea in a circuitous manner. The details of his brief report leave no doubt that he actually observed this phenomenon. He noted that "the back of the larynx is . . . the part from which" mucus was most easily cleared.

Other workers noted the fact of muciliary function and attempted to quantitate it. In 1876 Henry Pickering Bowditch (1840-1911) (founder at Harvard of the first laboratory of physiology in the United States) published his meticulous studies of the astounding work capacity of cilia.[66] Using the frog's palate for his investigations, he devised a vehicle to be carried by cilia up an inclined plane and showed that this mucosa was able to do work equivalent to 6.805 grammillimeters per square cm per min.

As we arrive at the twentieth century we appear to be on the doorstep of a broad understanding of man's relations to his ambient air. Fundamental studies of climate and indoor heating and ventilation have been published. We not only have Henle's hypothetical analysis of infectious airborne disease but the new science of microbiology has provided it with solid proof. The nature of respiratory mucus still escapes us but primitive attempts at its analysis have already been made, and Sharpey's simple observation has been followed by the development of many methods for observing mucociliary flow. Nasal anatomy and its effects upon nasal air flow have at least been touched upon. Still, another 30 years will pass before definitive studies in man will be done and yet another 30 before serious attempts to draw together all related fields of knowledge for a better understanding of atmospheric environmental threats and the nose as a defence organ.

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[63] Jeffries Wyman. "Experiments with vibrating cilia," *Am. Naturalist,* 1871, *5*: 611-616.

[64] H. Veale. "On the movement of mucus in the trachea and larynx," *Lancet,* 1871, *2*: 121-122.

[65] M. Garcia. "Observations on the human voice," *Proc. Roy. Soc., London,* 1854-1855, pp. 399-410.

[66] Henry Pickering Bowditch. "Force of ciliary motion," *Boston Med. Surg. J., 95*: 159-164.

An excellent attempt at integrating and analyzing the available information was made as early as 1880 by D. Bryson Delevan; [67] and in 1889 and 1896 Harrison Allen [68] and Thomas H. Manley [69] described nasal anatomy, gross and histological. Recognition of the potential for defence was there. Manley, speaking of mucous membranes in general says, "In the healthy state . . . they provide formidable barriers against germ invasion or against the introduction into the system of putrescent noxious elements. With their serried, close columns of epithelial cells, they resist the ingress of, or, at all events, neutralize or destroy in a large measure, lethal elements in the atmosphere and in the food we consume . . ."—this 79 years ago.

Among the most interesting papers at the end of the century was that which won the Boylston Medical Prize of Harvard University for Joseph Lincoln Goodale (1868-1957), a 28-year-old laryngologist at the Massachusetts General Hospital.[70] In addition to describing ingenious experiments on the nose as an air conditioner he demonstrated nasal air currents at least as they were reflected by the areas of deposition of particulates. He clearly identifies these areas as the "anterior end of the middle turbinate" and the "cartilaginous [anterior] portion of the nasal septum," and noted that ". . . no deposit is discoverable posteriorly in the nose or pharyngeal vault."

Goodale concluded that "the normal nose, in an atmosphere not excessively dust-laden, is a very nearly, if not wholly, complete filter for the inspired air," and he does not say that this is a purely "adventitious" function. Through the kindness of Dr. Goodale's son (also a laryngologist) I have learned that he pursued a long and successful career, continuing his interest in research and publishing numerous papers.

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[67] D. B. Delevan. "On some points in the anatomy of the nasal fossae," *New York Med. J.*, 1880, *32*: 376-381.

[68] H. Allen. "The anatomy of the nasal chambers," *Trans. Am. Laryngol. Assoc.*, 1889, *10*: 76-84.

[69] T. H. Manley. "A few notes on the general and special features of the anatomy and histology of mucous membranes," *Med. Surg. Reporter*, 1896, *75*: 735-737.

[70] J. L. Goodale. "An experimental study of the respiratory functions of the nose," *Boston Med. Surg. J.*, 1896, *135*: 457-460, 487-490.

Then, as today, the practicing surgeon evidently sometimes lagged well behind his physiological colleagues. At a time when the significance of the complex structure of the nasal passage was beginning to be recognized, in 1905 the same J. N. Mackenzie mentioned twice above delivered a paper on "some abuses in the intranasal surgery of today." In it he compares the ENT surgeon's ruthless attack on structures within the nose with the, "injunction of the Irishman at Donnybrook—If you see a head, hit it." [71]

With the beginning of the twentieth century we find more basic investigations of the nature of ciliary function. In 1903 R. S. Lillie, working at Woods Hole, studied the relation of various salts and ions to ciliary efficiency; [72] and, in 1905, Maxwell at Harvard [73] carefully repeated and confirmed the studies of Bowditch,[74] investigated the question of ciliary fatigue, and also examined the effects of numerous salt solutions on this function. In the same year W. E. Dixon and O. Inchley, working in pharmacology in Cambridge, England, devised a most ingenious "cilioscribe," making it possible to continuously record and quantitate the activity of cilia.[75]

Also in 1905, George H. Parker (1864-1955) (Professor of Zoology at Harvard) demonstrated a particularly interesting phenomenon.[76] Parker studied the action of the labial cilia of sea anemones, noted that they apparently were able to reverse the effective direction of their stroke, and suggested that they were able to discriminate between materials more or less nutritious. Using a minute piece of filter paper saturated with the test substance as bait, he showed that various organic substances (including meat, potassium, creatine but not creatinine, and glycogen) could all reverse the cilia beat to produce ingestion rather than expulsion. These findings have been both denied and supported; but studies designed to explore the possibility that disruption, incoordination, or reversal of ciliary beating may occur in respiratory disease in man remain to be done.

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[71] N. N. MacKenzie. "Remarks on some abuses in the intranasal surgery of today," *New York Med. J.*, Jan 28, 1905, *81*: 173-179.

[72] Lillie, R. S. "The relations of ions to ciliary movement," *Am. J. Physiol.*, 1903-1904, *10*: 419-443.

[73] S. S. Maxwell. "The effect of salt-solutions on ciliary activity," *Am. J. Physiol.*, 1905, *32*: 395-400.

[74] Bowditch (see n.66 above).

[75] W. E. Dixon and O. Inchley. "The cilioscribe, an instrument for recording the activity of cilia," *J. Physiol.*, 1905, *32*: 395-400.

[76] George H. Parker. "The reversal of the effective stroke of the labial cilia of sea-anemones by organic substances," *Am J. Physiol.*, 1905, *14*: 1-6.

Investigations of the mechanism of ciliary function continued to appear more or less regularly. But the desire to apply this accumulating knowledge to the problems of human health which was apparent in the latter part of the 19th century had waned. In retrospect one wonders if scientists were looking to the growing specialty of otolaryngology to take the lead.[77]

In 1916 Hill did consider the possible relation between the indoor climate, nasal mucous membrane, and the "common cold."[78] In discussing the ventilating system of the House of Commons (which evidently came from underfoot) he concluded the results must be, "Cold feet and stuffy heads . . . just the wrong conditions for legislators." Hill also did some studies of the nasal airway, and a study of in vitro tracheal ciliary function in relation to ambient temperature.[79]

I cannot draw this paper to a close without referring to the monumental contributions of Anderson C. Hilding whose work crowns the efforts of so many of his predecessors. His first paper came out in 1931.[80] A year later he wrote a paper which has served to stimulate the curiosity of four decades of students and investigators.[81] Hilding continues to introduce new and imaginative ideas into the field today.

Now that the population explosion, migration from country to town, rapid air travel, and increasing contamination of our atmosphere all daily add to the risk of airborne disease we can but hope that this long story will reach fruition at an early enough date to save mankind from his own atmosphere.

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[77] No one in that field rose to seriously accept the challenge until the advent of Arthur W. Proetz (1888-1968) whose numerous papers beginning as early as 1922 and whose book on the *Applied Physiology of the Nose* (St. Louis: Annals Publ. Co., 1953) were to exert a wide influence on otolaryngologists.

[78] L. Hill, "Colds and the influence of the atmosphere on the nasal mucous membrane," *Brit. Med. J.* 1916, *1*: 541-543.

[79] L. Hill, "The ciliary movement of the trachea studied in vitro," *Lancet*, 1928, *2*: 802-805.

[80] A. C. Hilding, "Ciliary activity and course of secretion currents of the nose," *Proc. Staff Meet. Mayo Clinic*, 1931, *6*: 285-287.

[81] A. C. Hilding, "Four physiologic defenses of the upper part of the respiratory tract: ciliary action, exchange of mucin, regeneration and adaptability," *Ann. Int. Med.* 1932, *6*: 227-234.

FREUD'S PAPER "ON MALE HYSTERIA" AND THE CONFLICT BETWEEN ANATOMICAL AND PHYSIOLOGICAL MODELS

KENNETH LEVIN

Using a postgraduate travelling grant which he had won the previous June, Freud attended J. M. Charcot's neuropathology lectures and teaching rounds at the Salpetrière in Paris from October, 1885, to the end of February, 1886. Shortly after his return to Vienna, Freud sent a report on his studies abroad to the Faculty of Medicine; and the following October he presented to the Vienna Medical Society a paper entitled "On Male Hysteria" in which he discussed Charcot's recent work.

In *An Autobiographical Study* (1925), Freud writes,

> The duty devolved upon me of giving a report before the "Gesellschaft der Aerzte" upon what I had seen and learnt with Charcot. But I met with a bad reception. Persons of authority, such as the chairman (Bamberger, the physician), declared that what I said was incredible. Meynert challenged me to find some cases in Vienna similar to those which I had described and to present them before the Society.[1]

He does not elaborate further on the material he presented or what aspects of it were challenged. But Henri Ellenberger, in *The Discovery of the Unconscious*,[2] attempts an analysis of Freud's paper and its reception. According to Ellenberger, Freud tried to present the concept of male hysteria, of which he had heard so much from Charcot, as a new discovery. But,

> Actually, things were quite otherwise. Freud had returned from Paris with an idealized picture of Charcot. Much of what he attributed to Charcot were the views of previous authors, and male hysteria was a well-known condition of which case histories had been published in Vienna long since by Benedikt, Rosenthal, and others.[3]

Ellenberger supports his interpretation by reference to the discussion that followed the paper, during which Leidesdorf, Rosenthal and Bamberger all noted their familiarity with cases such as Freud describes. Ellenberger acknowledges that there was, in fact, a dispute between the

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[1] *The Complete Psychological Works of Sigmund Freud*, ed. James Strachey (London: Hogarth Press, 1966 ff.), vol. 20, p. 15.

[2] Henri Ellenberger, *The Discovery of the Unconscious* (New York: Basic Books, 1970).

[3] *Ibid.*, p. 440.

Viennese and Charcot concerning male hysteria; but he argues that Freud "does not seem to have seized the complexity and the practical implications"[4] of the actual controversy. According to Ellenberger, this actual controversy had to do with "nervous shock," a term introduced by W. H. Page in England in 1882 to designate those neurological abnormalities which were often found to follow accidents, most notably railway accidents, but which could not be accounted for by the victim's physical injuries. Page and Charcot both insisted that nervous shock was merely a form of hysteria, whereas Robert Thomsen and Hermann Oppenheim, two Berlin neurologists, argued that nervous shock, or "traumatic neurosis" as they labelled it, was distinct from hysteria. Writes Ellenberger:

> There were now two kinds of male hysteria in Paris: the classical one (in which heredity was considered the main etiological factor), and the post-traumatic (in which heredity played a lesser role, if any). In Vienna the existence of classical male hysteria was no longer questioned, but the leading neurologists did not accept Charcot's identification of traumatic paralysis in men with male hysteria.[5]

One is immediately inclined to question Ellenberger's interpretation, because it reduces Freud to little more than a fool. But, beyond this, it fails to consider some basic and profound distinctions between Charcot's views and those of the Viennese, distinctions important both for an understanding of Freud's development and for a satisfactory analysis of changes in European psychiatry and neurology during this period.

Theodor Meynert and his associates, who at this time dominated not only Viennese psychiatry but German psychiatry generally, regarded hysteria, insofar as it was a distinct and delineable clinical entity, as due to an anatomical lesion of the brain. Disorders which displayed ephemeral and protean symptoms popularly labelled hysterical, but yielding no consistent clinical picture, were seen as probably due to simulation or some other equally intangible factor and as not properly within the purview of clinical psychiatry or neurology.

Charcot, on the other hand, had been arguing for several years that hysteria was not attributable to any anatomical lesion, but was nevertheless a distinct pathological entity which invariably displayed a consistent clinical picture. He viewed the underlying pathology as functional rather than organic; that is, as some physiological abnormality of nerve transmission for which no corresponding anatomical changes could be defined.

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[4] *Ibid.*, p. 441.

[5] *Ibid.*, p. 439.

Charcot's work was new to Freud, who had received his psychiatric training from Meynert and whose primary interest, prior to the Paris trip, was in neuroanatomy. In writing of his motive for going to Paris, Freud says,

> I was bound to reflect that I could not expect to learn anything essentially new in a German University, after having enjoyed direct and indirect instruction in Vienna from Professors T. Meynert and H. Nothnagel. The French School of neuropathology, on the other hand, seemed to me to promise something unfamiliar and characteristic in its mode of working, and moreover to have embarked on new fields of neuropathology, which have not been similarly approached by scientific workers in Germany and Austria.[6]

In Paris, Freud was impressed with Charcot's work on hysteria and was converted to his physiological model. When he later discusses hysteria in the paper presented to the Vienna Medical Society, he is perceiving it in the light of this new concept of the underlying pathology. Consequently, when his audience protests that it is quite familiar with the phenomenon of male hysteria it is not really talking to the point. Freud's hysteria is the functional illness described by Charcot; Rosenthal, Bamberger and Leidesdorf's hysteria is the clinical syndrome associated with Meynert's postulated anatomical lesions. In effect Freud and his audience are talking past each other. The real issues involved at the meeting are particularly noteworthy because this clash between anatomical and physiological models lies at the core of the vast changes undergone by continental psychiatry and neurology during the last two decades of the nineteenth century.

Charcot on Hysteria

Charcot's earliest views on hysteria, in the period before he developed his physiological model, display a belief in underlying anatomical lesions. Both Moriz Benedikt, in 1868, and Moriz Rosenthal, in 1870, cite Charcot as having demonstrated anatomical lesions in hysterical patients.[7] And in a collection of lectures published in 1877, Charcot argues that hysterical hemianaesthesia has the same etiology as other anaesthesias, that it is due to "a circumscribed lesion of the cerebral hemispheres."[8] He cites pathological evidence supporting this view.

This is consistent with Charcot's general anatomical bias. In the preface to an American edition of his *Lectures on Localization in Diseases of the Brain* (1875), he writes,

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[6] Freud, *Complete Psychological Works,* vol. 1, 1966, p. 5.

[7] Moriz Benedikt, *Elektrotherapie* (Vienna Tendler, 1868), p. 413; Moriz Rosenthal, *Klinik der Nervenhrankheiten,* 2 Aufl. (Stuttgart: Enke, 1875), p. 464.

[8] Jean Martin Charcot, *Lectures on Diseases of the Nervous System,* trans. George Sigerson (London: New Sydenham Society, 1877), p. 246f.

> In the lectures . . . I have selected, as occasion required, information furnished by normal anatomy, experimental physiology, and clinical observation, illustrated by minute and methodical examination of organic lesions.
>
> I have always given precedence, however, to the last-mentioned order of testimony, convinced that, although normal anatomy and experimental physiology may serve to indicate the true direction; still, clinical and pathological research is necessary (in the case of the human subject) to a final judgment and the furnishing of proof.[9]

In 1882 he again asserts his faith in the primacy of pathological anatomy, noting that, "It is often said that the progress of medicine and of pathological anatomy go side by side. This is specially true in diseases of the nervous system." [10] But he now modifies his position with respect to hysteria:

> From what has been said it will be understood how much importance we ought to accord in our studies to the anatomo-pathological method of research. But you are aware, gentlemen, that there still eixsts at the present time a great number of morbid states, evidently having their seat in the nervous system, which leave in the dead body no material trace that can be discovered. Epilepsy, hysteria, even the most inveterate cases, chorea, and many other morbid states which would take us too long to enumerate, come to us like so many Sphynx, which deny the most penetrating anatomical investigations. These symptomatic combinations deprived of anatomical substratum, do not present themselves to the mind of the physician with that appearance of solidity, of objectivity, which belong to affections connected with an appreciable organic lesion.
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> There are even some who see in several of these affections only an assemblage of odd incoherent phenomena inacessible to analysis, and which had better, perhaps, be banished to the category of the unknown. It is hysteria which especially comes under this sort of proscription. . . . [But] much credit is due to Bricquet for having established in his excellent book, in a manner beyond dispute, that hysteria is governed, the same way as other morbid conditions, by rules and laws, which attentive and sufficiently numerous observations always permit us to establish.[11]

Charcot, in the balance of the lecture, pursues this thesis that hysteria is a clear-cut entity which follows definite rules; and he offers proofs that hysterical symptoms cannot be attributed to intentional deception by the patient. On the latter point he concludes:

> A hundred other examples might be invoked which would only show that the simula-

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[9] Charcot, *Lectures on Localization in Diseases of the Brain,* trans. E. P. Fowler (New York: William Wood, 1878), preface.

[10] Charcot, *Clinical Lectures on Diseases of the Nervous System,* trans. Thomas Savill (London: New Sydenham Society, 1889), p. 9.

[11] *Ibid.,* pp. 12-13.

> tion, which is talked about so much when hysteria and allied affections are under consideration, is, in the actual state of our knowledge only a bugbear, before which the fearful and novice alone are stopped.[12]

Hysteria, for Charcot, is now labelled a neurosis, which his translator defines as a disease "of the nervous system apparently due to functional or dynamic causes; which [is] not, so far as we know, attended by any organic lesion." [13]

Charcot had argued several years earlier that minor physical trauma can induce contractures in people with hysterical dispositions. He develops this theme further in an 1882 lecture on "Two Cases of Hysterical Contracture of Traumatic Origin." [14] In this lecture, as in all his case presentations of hysterics, Charcot is primarily concerned with showing that the patients' symptoms do not resemble symptoms caused by anatomical lesions, but do fit the "rules" of hysteria. He thereby not only establishes his diagnosis but reinforces his argument that hysteria is a bona fide pathological entity and presents a consistent picture. His first patient is a female. The demonstration that her contracture is hysterical involves: (1) noting hereditary indications of possible hysterical diathesis [15] (her father died in an insane asylum and her brother is an idiot); (2) displaying the characteristic nature of the contracture (e.g., hysterical contractures are more intense and more consistent—the intensity does not vary between sleep and waking—than are those contractures due to anatomical lesions); (3) pointing out other hysterical symptoms in the patient (in this case, hemianaesthesia, hemianalgesia and "ovarian phenomena"). The second case, that of a man, is presented in the same manner. These cases show clearly that by 1882 Charcot had a unified concept of hysteria, covering the disease's appearance in men and women and including symptoms induced by trauma as well as other factors.

Examples of hysteria in males were of interest to Charcot insofar as they demonstrated the universality of the disease and helped establish its credentials as a legitimate pathological entity. It is for this reason that he is happy to note, in an 1885 lecture:

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[12] *Ibid.*, p. 18.

[13] *Ibid.*, p. 13.

[14] *Ibid.*, p. 85.

[15] The role attributed to hereditary factors by Charcot in all his case presentations of hysteria precipitated by trauma argues against Ellenberger's distinction between "two kinds of male hysteria in Paris," one of hereditary and the other of traumatic origin. Further, some of Charcot's patients had histories of hospitalization for hysteria prior to developing symptoms secondary to trauma. Charcot in fact never gives any indication of a distinction such as that cited by Ellenberger.

> Quite recently male hysteria has been studied by Messrs. Putnam and Walton in America, principally as it occurs after injuries, and especially after railway accidents. They have recognised, like Mr. Page, who in England has also paid attention to this subject, that many of those nervous accidents described under the name of Railway-brain, are in fact, whether occurring in man or woman, simply manifestations of hysteria.[16]

Page's work is particularly interesting to Charcot because,

> One can conceive that it may be possible for a young effeminate man, after excesses, disappointments, profound emotions, to present hysterical phenomena, but that a vigorous artisan, well built, not enervated by high culture, the stoker of an engine for example, not previously emotional, at least to all appearance, should, after an accident to the train, by a collision or running off the rails, become hysterical for the same reason as a woman, is what surpasses our imagination. Yet nothing can be more clearly proved, and it is a fact which will have to be accepted.[17]

Charcot regards the clinical histories of these men as further evidence of both the universal occurrence of the disease and the universal uniformity of its symptoms. In the same context he rebuts the arguments of Hermann Oppenheim and Robert Thomsen, who claim that nervous shock is a separate disease characterized by more severe symptoms, by showing that symptoms of equal severity may be found in the most pedestrian cases of hysteria.

In another lecture on hysteria given in 1885, Charcot reasserts that,

> The collection of symptoms which have been unfolded, so inexplicable on the hypothesis of an organic lesion of the brain, spinal cord, or peripheral nerves, admits of a very simple interpretation on the supposition of a dynamic lesion.[18]

And he remarks,

> The prognosis naturally follows from the diagnosis, and it is clear that it will be infinitely less grave in cases of this nature than in such as supervene in a destructive organic lesion.[19]

Charcot then considers the question of what sort of intervention might best help to bring this hopeful prognosis to fruition. He suggests that intervention would be most effective if it were "founded on a physiological basis" and were based on a recognition of the mechanism responsible for the patient's difficulties. This leads him into a consideration of hypnosis.

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[16] Charcot, *Clinical Lectures,* p. 221.

[17] *Ibid.,* pp. 222-223.

[18] *Ibid.,* pp. 281.

[19] *Ibid.,* p. 288.

Charcot had been discussing hypnosis in the context of the pathology of hysteria for several years. He believed that hypnosis itself involved physiological changes in the nervous system; or, according to a discussion of Charcot's views by Freud,

> that the mechanism of some at least of the manifestations of hypnotism is based upon physiological changes—that is, upon displacements of excitability in the nervous system, occurring without the participation of those parts of it which operate with consciousness; they [i.e., Charcot's school] speak therefore, of the physical or physiological phenomena of hypnosis.[20]

Charcot further argued that hypnosis had peculiar manifestations in a person suffering from hysteria, manifestations presumably related to the basic dynamic abnormalities of the nervous system in these people. To quote again from Freud:

> "grand hypnotisme"—the phenomena described by Charcot in the case of hypnotized hysterical patients. Unlike normal hypnotized subjects, these hysterical patients are said to exhibit three stages of hypnosis, each of which is distinguished by special physical signs of a most remarkable kind (such as enormous neuro-muscular hyperexcitability, somnambulistic contractures, etc.).[21]

The three stages of "grand hypnotisme" are the lethargic, the cataleptic and the somnambulic. In a series of lectures given in 1885 and beginning with the one cited above, Charcot demonstrated how, by means of verbal suggestions to a patient in the somnambulic state, he could artificially produce, "a perfect imitation of the monoplesia caused in our two other patients by a process apparently very different, the action of traumatism." [22] After awakening the patient, so that power was restored to her paralyzed arm, he once again hypnotized her; and, by this time striking her on the shoulder, he again induced a monoplegia exactly duplicating that of his traumatized patients. He concluded:

> It may be inquired whether the mental condition occasioned by the emotion, by the Nervous Shock experienced at the moment of the accident and for some time after, is not equivalent in a certain measure, in subjects predisposed, . . . to the cerebral condition which is determined in "hysterics" by hypnotism. Upon the assumption of this hypothesis, the peculiar sensation felt by our hysterical female in the member submitted to shock, and which we may suppose to have been produced in

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[20] Freud, "Preface to the translation of Bernheim's *Suggestion*," *Complete Psychological Works*, vol. 1, p. 77.

[21] *Ibid.*, p. 77.

[22] Charcot, *Clinical Lectures*, p. 302

> the same degree and with the same character in our two male patients by a fall on the shoulder, that sensation, I say, may be considered as having originated, in the former as in the latter, the idea of motor paralysis of the member. But because of the annihilation of the ego produced by the hypnotism in the one case, and, as one may suppose, by the nervous shock in the other, that idea once installed in the brain takes sole possession and acquires sufficient domination to realise itself objectively in the form of paralysis. The sensation in question, therefore, in both the cases plays the part of a veritable suggestion.[23]

Charcot thus develops a model for the etiology of hysteria of traumatic origin. In a nervous system conditioned by an hysterical diathesis, nervous shock or fear induces an hypnotic state which renders the victim susceptible to suggestion. The trauma then provides the suggestion of paralysis. But beyond an etiological explanation, Charcot also derives from this model a therapeutic program. He begins regularly to use suggestion, both under hypnosis and without it, as an instrument for resolving hysterical symptoms.

This is, in essence, the new world revealed to Freud during his stay in Paris.

Hysteria as a Dynamic Lesion

The concept of hysteria as due to a physiological abnormality, but involving no anatomical lesion, is not original with Charcot. The physiological emphasis can be traced at least in part to William Cullen who coined the term "neuroses" and defined it as:

> all those preternatural affections of sense and motion, which are without pyrexia as a part of the primary disease; and all those which do not depend upon a topical affection of the organs, but upon a more general affection of the nervous system, and of those powers of the system upon which sense and motion more especially depend. . . .[24]

Cullen's text, *First Lines in the Practice of Physic,* was translated into French by Pinel, and both the term "neurosis" and the concept thereby entered French psychiatry. Although Cullen's original "neuroses" cover a large area, including all forms of insanity, the definition was soon narrowed to encompass a small body of diseases among which hysteria held a prominent position.

In Germany, M. H. Romberg, Professor of Medicine at the University of Berlin, published in 1840 a text entitled *Diseases of the Nervous*

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[23] *Ibid.,* p. 305.

[24] William Cullen, *First Lines in the Practice of Physic* (Edinburgh: Reid and Scott, 1802), vol. 2, pp. 65-66.

System. In the preface Romberg defines his position on the general debate between physiologists and anatomists over which discipline represents the royal road of medicine. He asserts that his book is based upon "the physiological principle" and bemoans the fact that, "the majority of students [are] attracted by the school which seeks to base the science of medicine exclusively upon pathological anatomy and chemistry." [25] Romberg divides all neurological diseases, including hysteria, into four categories: neuroses of sensibility, neuroses of motion, logo-neuroses, and tropho-neuroses.

Numerous other practitioners, in Germany, France, and England, argued similar positions. While thus emphasizing the physiological aspects of hysteria and supposedly related diseases, these writers were often either silent or ambiguous on the question of whether any anatomical lesions accompanied these ailments. But examples can be found where anatomical lesions were explicitly excluded. In addition examples can also be cited of authors who attributed a therapeutic role to hypnotism. A single reference will suffice to illustrate both positions. In 1843, James Braid, an English surgeon, wrote in his text *Neurypnology:*

> By the impression which hypnotism induces on the nervous system, we acquire a power of rapidly curing many functional disorders, most intractable, or altogether incurable, by ordinary remedies, and also many of those distressing affections which, as in most cases they evince no pathological change of structure, have been presumed to depend on some peculiar condition of the nervous system, and have therefore, by universal consent, been denominated "nervous complaints." [26]

Braid includes hysteria among the latter diseases, and asserts in a later work on *Hypnotic Therapeutics* that, "the most striking . . . cases for illustrating the value of the hypnotic mode of treatment, are cases of hysteric paralysis." [27]

But while the concept of hysteria as due to a physiological abnormality, with no accompanying anatomical lesion, and of hypnotism as a therapeutic device, is not new in the history of medicine, it is new and revolutionary in the context of the medical training of Charcot, Meynert, Freud, and most of the other physicians discussed in this paper.

Charcot's basic orientation towards patho-anatomy has already been noted. His nosological method consisted of careful clinical observation followed by post-mortem examination to discover correlative anatomical changes. This approach is consistent with that emphasized by the Paris Faculty of Medicine, where Charcot received his medical education and where he became Professor of Pathology and Anatomy in 1872. Pathological anatomy dominated the Paris school from the time of Corvisart and Laennec, and efforts by physiologists such as Claude Bernard to counter this influence had only limited impact. In view of this background, Charcot's anatomical bias is understandable. Nor does it come as any surprise that his reputation as a neurologist was based on his work in elucidating the anatomical lesions associated with tabes dorsalis, multiple sclerosis, amyotropic lateral sclerosis, and other clinical entities; [28] and that he published several volumes of lectures on the localization of diseases of the brain and spinal cord.[29] On the other hand, development of a purely physiological model of hysteria clearly reflects a profound shift in his basic notion of admissible pathological models.

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[25] M. H. Romberg, *Diseases of the Nervous System,* trans. Edward Sieveking (London: Sydenham Society, 1853), p. xiii.

[26] James Braid, *Neurypnology* (London: John Churchill, 1843), pp 15-16

[27] Braid, *Hypnotic Therapeutics* (Edinburgh: Murray & Gibb, 1853).

Meynert's training, and his work as a psychiatrist, involve an even greater emphasis on pathological anatomy. Among the factors impelling German psychiatry in that direction around mid-century were: (1) the anatomical bias of influential French psychiatrists (Jean Pierre Falret, for example, asserted that, "I believe firmly that in all cases, without exception, one could find in the mentally sick appreciable lesions in the brain or its membranes; these lesions are sufficiently constant to account satisfactorily for all the various intellectual and affective disturbances in insanity."); [30] (2) the high percentage of psychiatric patients—Ackerknecht estimates up to thirty percent [31]—who were suffering from paralyses primarily due to tertiary syphilis, and would therefore yield clear-cut lesions on autopsy; and (3) the influence of Wilhelm Griesinger whose text emphasized organic lesions. But in Meynert's case there was also the major factor of his training under Carl Rokitansky.

In a period when German medicine was largely dominated by physiologists, particularly Johannes Müller and his students, Rokitansky, a pathologist, founded with Josef Skoda the "new School of Vienna." According to Knud Faber,

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[28] Erwin Ackerknecht, *A Short History of Psychiatry,* trans. Sulammith Wolff (New York: Hafner, 1959), p. 73.

[29] *E.g., Lectures on Localization in Diseases of the Brain* (1878); *Lectures on the Pathological Anatomy of the Nervous System,* trans. Cornelius Comegys (Cincinnati: Peter G. Thomson, 1881); *Lectures on the Localization of Cerebral and Spinal Diseases,* trans W. B. Hadden (London: New Sydenham Society, 1883).

[30] Quoted in G. Zilboorg, *History of Medical Psychology* (New York: W. W. Norton, 1941).

[31] Ackerknecht, *Short History of Psychiatry,* p. 66.

> They followed largely along the lines laid down by the Paris school. Rokitansky, drawing on an immense body of material, most industriously and systematically continued the study of the pathologico-anatomical changes in the organs which had been undertaken by the French pathological anatomists.[32]

Contemporary accounts confirm Faber's comparison between Paris and Vienna. In *Cellular Pathology* (1858), Virchow speaks of "the Vienna School, for they too, like the French school, ground their system of pathology upon pathological anatomy." [33] If there is any difference between Rokitansky and the Parisians, it is that he, unlike the French,

> placed anatomical alterations first in the classification of morbid processes, and held that the task of clinical medicine was confined to demonstrating these anatomical changes in patients. He thus entirely subordinated clinical medicine to pathological anatomy and made anatomical diagnosis in the literal sense of the word the end and object of his efforts.[34]

Meynert, while still a student, made histological preparations for Carl Wedl, and later did similar work for Rokitansky's assistant, Gustav Scheuthauer. In the course of this work he became interested in brain anatomy, and began doing research in Rokitansky's Patho-anatomical Institute. In 1865, Rokitansky procured for him a lectureship in "the structure and function of the brain and spinal cord with relation to their pathology." And in 1870, after Meynert completed his psychiatric training, Rokitansky was instrumental in gaining him a post as director of a psychiatric clinic. In 1875, again with Rokitansky's help, Meynert became director of the newly founded psychiatric clinic at the Vienna General Hospital.[35]

Meynert's approach to psychiatry was thoroughly pathoanatomical; and neuroanatomical investigations did in fact enjoy extensive success between 1865 and 1885 in elucidating the pathology of numerous psychiatric and neurological syndromes, with much of the work being done by Meynert and his students (who included Carl Wernicke, Auguste Forel, and Johann Fritsch). This constant reinforcement helped to establish pathological anatomy as the dominant force in German psychiatry, with Vienna and Meynert at its head.

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[32] Knud Faber, *Nosography* (New York: Paul B. Hoeber, 1930), pp. 55-56.

[33] Rudolph Virchow, *Cellular Pathology,* trans. Frank Chance (Philadelphia: J. B. Lippincott, 1863), p. 428.

[34] Faber, *Nosography,* pp. 56-57.

[35] Erna Lesky, *Die Wiener medizinische Schule* (Koln: Verlag Hermann Bohlaus, 1965), pp. 373-374, 380.

Freud's Response to Charcot

Freud was a student of Meynert in medical school, and later served as Secundararzt in Meynert's Psychiatric Clinic (May to October, 1883). He also worked for two years (1883-1885) in Meynert's laboratory. Freud writes in his *Autobiographical Study:*

> One day Meynert, who had given me access to the laboratory even during the times when I was not actually working under him, proposed that I should definitely devote myself to the anatomy of the brain, and promised to hand over his lecturing work to me, as he felt he was too old to manage the newer methods.[36]

Freud's own orientation towards pathoanatomical psychiatry is revealed in the following anecdote, also found in *An Autobiographical Study:*

> The fame of my diagnoses and of their post-mortem confirmation brought me an influx of American physicians, to whom I lectured upon the patients in my department in a sort of pidgin-English. About the neuroses I understood nothing. On one occasion I introduced to my audience a neurotic suffering from a persistent headache as a case of chronic localized meningitis; they all quite rightly rose in revolt and deserted me, and my premature activities as a teacher came to an end. By way of excuse I may add that this happened at a time when greater authorities than myself in Vienna were in the habit of diagnosing hysteria as cerebral tumour.[37]

Virtually all of Freud's research work, that done in Brucke's laboratory as well as the work under Meynert, was in neuroanatomy. A curriculum vitae, submitted by Freud in 1897 as part of his application for professorship, lists fifteen papers written before the one on "Male Hysteria." Of these, nine concern neuroanatomical research and three others are case histories of patients with well-defined nervous system lesions. (His first paper was a histological study of eel testes; and the remaining two papers are on cocaine).[38]

In his "Report on My Studies in Paris and Berlin" (he had spent March, 1886, in Berlin), written two months after leaving Paris, Freud notes:

> My work in the Salpêtrière itself took on a different shape from what I had originally laid down for myself. I had arrived with the intention of making one single question the subject of a thorough investigation; and since in Vienna my chosen concern had been with anatomical problems, I had selected the study of the secondary atrophies and degenerations that follow on affections of the brain in children. Some extremely valuable pathological material was put at my disposal, but I found that the conditions for making use of it were most unfavorable. The laboratory

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[36] Freud, *Complete Psychological Works,* vol. 20, p. 11.

[37] *Ibid.,* p. 12.

[38] *Ibid.,* vol. 3, p. 225 f.

> was not at all adapted to the reception of an extraneous worker, and such space and resources as existed were made inaccessible owing to lack of any kind of organization. I thus found myself obliged to give up anatomical work and rest content with a discovery concerned with the relations of the nuclei of the posterior column in the medulla oblongata. Later, however, I had an opportunity of resuming some similar investigations with Dr. von Darkewitsch (of Moscow); and our collaboration led to a publication in the *Neurologisches Centralblatt,* bearing the title "On the relation of the restiform body to the posterior column and its nucleus, with some remarks on two fields of the medulla oblongata." [39]

The Report just cited is, however, devoted for the most part to Charcot; and Freud writes that he "left the Salpêtrière as Charcot's unqualified admirer." Of Charcot's work he writes:

> Charcot used to say that, broadly speaking, the work of anatomy was finished and that the theory of the organic diseases of the nervous system might be said to be complete: what had next to be dealt with was the neuroses. This pronouncement may, no doubt, be regarded as no more than an expression of the turn which his own activities had taken. For many years now his work has been centered almost entirely on the neuroses, and above all on hysteria.[40]

Freud then proceeds to relate how up until recently hysteria had not been well-defined. Its symptoms were thought extremely variable, and exaggerated importance was attributed to simulation. Charcot imposed order on this chaos and recognized patterns in the clinical picture of hysteria. Furthermore:

> By making a scientific study of hypnotism—a region of neuropathology which has to be wrung on the one side from scepticism and on the other from fraud—he himself arrived at a kind of theory of hysterical symptomatology. These symptoms he had the courage to recognize as for the most part real.[41]

Freud was fascinated by these revelations, and he relates that his exchanges with Charcot, "led to my preparing a paper which is to appear in the *Archives de Neurologie* and is entitled 'A Comparison between Hysterical and Organic Symptomatology' " [42] He was, of course, well aware that hysteria was often recognized as a legitimate disease, but regarded as due to an anatomical lesion; and he enthusiastically undertook to point out signs which distinguish it from disorders caused by such lesions.

The report clearly emphasizes Charcot's position that hysteria is a clinically well-defined syndrome which is nevertheless distinguishable from illnesses due to organic lesions. Freud's enthusiasm was such that he asked Charcot's permission to translate a collection of lectures he was preparing for publication. The task was carried out so industriously that the German translation was actually published before the French edition.[43]

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[39] *Ibid.,* vol. 1, pp. 8-9.

[40] *Ibid.,* p. 10.

[41] *Ibid.,* p. 11.

[42] *Ibid.,* p. 12.

The text of the paper delivered by Freud before the Medical Society has not survived, but there are several reviews of it in the medical journals.[44] All concur that Freud emphasized three points made by Charcot: that hysteria is a well-defined disease with predictable symptoms, that simulation plays no significant role, and that hysteria in males has the same clinical manifestations as in females. None of the reviews suggests that either Freud or Charcot was presenting male hysteria as a new concept. The paper appears to have been mostly concerned with establishing the standard clinical picture of hysteria, with considerably less space devoted to the third point, that male hysteria presents the same picture as female hysteria. That Freud, through his title, chose to emphasize this last point, can be attributed to his sharing Charcot's belief that cases of male hysteria provided some of the strongest support for the argument that hysteria is a clinically uniform disease.

The journals also indicate that Freud explicitly associated Charcot's success in his investigations of hysteria with his move away from pathological anatomy. The *Wiener medizinische Blätter* writes:

> He stressed to begin with the great advances made by Charcot in the theory of hysteria, while he emphasized as significant Charcot's altered direction, his shift from pathological anatomist to clinician.[45]

Freud adopted fully Charcot's physiological interpretation of hysteria and held this view in subsequent years. This is clear from an article on hysteria written in 1888:

> Hysteria is a neurosis in the strictest sense of the word—that is to say, not only have no perceptible changes in the nervous system been found in this illness, but it is not to be expected that any refinement of anatomical techniques would reveal any

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[43] Ernest Jones, *The Life and Work of Sigmund Freud* (New York. Basic Books, 1953), vol. 1, p. 209.

[44] 1) *Wiener medizinische Presse,* vol. 27, 1886, pp. 1407-1409. 2) *Wiener medizinische Wochenschrift,* vol. 36, 1886, pp. 1445-1447. 3) *Münchener medizinische Wochenschrift,* vol. 33, 1886, p. 768. 4) *Wiener medizinische Blätter,* vol. 9, 1886, pp. 1292-1293. 5) *Allgemeine Wiener medizinische Zeitung,* vol. 31, 1886, pp. 506-507 6) *Anzeiger der k. k. Gesellschaft der Aerzte in Wien,* no. 25, 1886, pp. 149-151.

[45] *Wiener medizinische Blätter,* vol. 9, p. 1293.

> such changes. Hysteria is based wholly and entirely on physiological modifications of the nervous system and its essence should be expressed in a formula which took account of the conditions of excitability in the different parts of the nervous system.[46]

In the years following his Paris visit, Freud was active in the debate over anatomical and physiological models even beyond its significance for hysteria, or the neuroses. Most notable is his book *On Aphasia* (1891) in which he attacks the theories of brain localization of Broca, Wernicke, and Lichtheim. E. Stengel, in the preface of his translation of the book, notes that, "Freud was the first in the German speaking world to subject the current theory of localization to a systematic critical analysis." [47] In his lengthy rebuttal of anatomical models, Freud directly attacks Meynert's approach:

> We remember that, under the influence of Meynert's teaching, the theory has been evolved that the speech apparatus consists of distinct cortical centers. . . . One may first of all raise the question as to whether such an assumption is at all correct, and even permissible. I do not believe it to be.[48]

And in the Conclusion, he writes,

> After Wernicke had discovered the relationship of the area called after him to sensory aphasia, the hope was bound to arise that this variety could be fully understood from the circumstances of localization. It appears to us, however, that the significance of the factor of localization for aphasia has been overrated, and that we should be well advised once again to concern ourselves with the functional states of the apparatus of speech.[49]

With regard to the neuroses, Freud subsequently directed his own efforts towards developing psychological explanations of hysterical and related phenomena. But he continued to believe, in accordance with Charcot's views, that many aspects of the neuroses could only be explained in neurophysiological terms.

Meynert on Hysteria

The reviews of the meeting at which Freud presented his paper "On Male Hysteria" all concur that Meynert spoke of having seen cases of nervous shock following trauma comparable to those described by Freud. One review suggests that Meynert fully subscribed to Freud's description of these cases and offered Freud patients from his clinic with whom he could demonstrate the symptoms he had described.[50] Four others indicate, instead, that Meynert merely expressed curiosity whether all these cases do indeed follow the rules established by Charcot.[51] A sixth review states, in contrast to all the others, that Meynert questioned whether these cases ought to be labelled hysteria.[52]

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[46] Freud, *Complete Psychological Works*, vol. 1, p. 41.

[47] Freud, *On Aphasia*, trans. E. Stengel (New York: International Universities Press, 1953), p. x.

[48] *Ibid.*, p. 54.

[49] *Ibid.*, pp 104-105

The second account of Meynert's remarks, on which four of the reports agree, is undoubtedly closest to the truth. On November 26, 1886, Freud did in fact present a patient to the Medical Society, and he began his presentation by noting,

> When, on October 15, I had the honor of claiming your attention to a short report on Charcot's recent work in the field of male hysteria, I was challenged by my respected teacher, Hofrat Meynert, to present before the society some cases in which the somatic indications of hysteria—the "hysterical stigmata" by which Charcot characterizes this neurosis—could be observed in a clearly marked form.[53]

This would seem to contradict the first review, and to corroborate the second account. Freud is here trying to prove that Charcot's rules, his lists of "stigmata," are indeed comprehensive. With regard to the last review, there is no indication in the November 26 paper of Freud's having been challenged by Meynert on whether nervous shock can properly be called hysteria. More importantly, Meynert's own writing on the subject makes no distinction between nervous shock and hysteria. His point of disagreement with Charcot, and with Freud, is, rather, his belief that hysteria is due to an anatomical lesion.

In 1889, largely in response to the publication of Freud's translation of Charcot's lectures, Meynert submitted a paper to the Medical Society entitled "A Contribution to the Understanding of Traumatic Neuroses." The paper was published in three parts in the *Wiener klinische Wochenschrift* during June, 1889.

Although Meynert writes favorably of Oppenheim's work, he does not endorse the latter's distinction between traumatic neurosis (Thomsen and Oppenheim's term for nervous shock) and hysteria. On the contrary, Meynert explicitly accepts Charcot's view that they represent a single syndrome.[54] The important distinction for Meynert is between hysteria, or traumatic neurosis, on the one hand, and organic paralysis on the other; and he writes of the "effort to establish the differences between the paralysis of traumatic neurosis, or Charcot's hystero-traumatic neurosis, and the paralysis of anatomical contusions of the brain." [55] Meynert, in fact, says that he prefers to speak simply of "functional" versus anatomical paralysis; [56] and he later refers to the "functional disorder . . . which [Charcot] calls hysterical, and which among men is usually of traumatic origin." [57]

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[50] *Wiener medizinische Presse,* vol. 27, p. 1409.

[51] *Wiener medizinische Wochenschrift,* vol. 36, p. 1446. *Münchener medizinische Wochenschrift,* vol. 33, p. 768. *Allgemeine Wiener medizinische Zeitung,* vol. 31, p. 507. *Anzeiger der k. k. Gesellschaft der Aerzte in Wien,* no. 25, p. 150.

[52] *Wiener medizinische Blätter,* vol. 9, p. 1293.

[53] Freud, *Complete Psychological Works,* vol. 1, p. 25.

Meynert's principal aim is to establish the pathology of the "functional" illness. His thesis is that this pathology involves an anatomical lesion of the brain, and he argues that

> Charcot . . . completely ignores the relation which the arteries of the Circle of Willis might have to hysterical, or, using the term in a purely descriptive sense, functional paralysis. While he places himself at the height of explanatory knowledge in relation to organic diseases of the brain, the sharp-minded author relinquishes this standpoint in relation to the nevertheless clearly equally localized functional disorders, and allows purely nosological descriptions to suffice with respect to these.[58]

Meynert accounts for Charcot's failure to recognize the anatomical basis of hysteria by referring to Charcot's scheme of the vasculature of the brain in his *Lectures* of 1875. He notes that Charcot

> overlooks, however, in [his] scheme of the basilar arteries, a branch of the internal carotid. . . . This branch, which leaves the carotid below the posterior communicating artery, is the choroid artery.[59]

Meynert then gives an account of the areas of the brain supplied by this choroid artery, and what symptoms might be expected if it were occluded; and he argues that these symptoms correspond perfectly to those found in hysterical paralysis:

> I think I have now shown that Charcot's very meritorious and rational attempts to establish, on the basis of arterial distribution in the area, a localization of functions

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[54] Theodor Meynert, "Beitrag zum Verstandniss der traumatischen Neurose," in *Wiener klinische Wochenschrift*, 1889, *2*: p. 475.

[55] *Ibid.*, p. 475. (All translations mine unless otherwise specified—K. L.)

[56] *Ibid.*, p. 475. Meynert's use of "functional" is rather peculiar, because he still envisions an underlying anatomical lesion. He says in a later quotation that he is using the term "in a purely descriptive sense"; that is, not in the sense of a particular type of pathology. This usage illustrates his position; he accepts the clinical distinction between anatomical and hysterical syndromes, but does not accept Charcot's pathological distinction.

[57] *Ibid.*, p. 500.

[58] *Ibid.*, p. 498.

[59] *Ibid.*, p. 498.

> of the brain stem, must remain incomplete because of the omission of the choroid artery from his scheme; as it is this very artery which controls the nutrition of the posterior part of the internal capsule, the area whose function is lost in functional hemiparalysis.[60]

He concludes that cerebral ischemia secondary to occlusion of the choroid artery, which in turn is probably due to arteriospasm, is the pathological basis for this major hysterical symptomatology.

Having established to his satisfaction the anatomical lesion underlying this syndrome, Meynert next attempts to disprove Charcot's thesis that hysterical paralysis is identical to that induced by suggestion. He concludes with a condemnation of hypnotic therapy and some observations on the potential dangers of hypnotism. In the course of this discussion Meynert notes that he has used Freud's translation of Charcot's lectures. He remarks that Freud is now active in Vienna as a trained practitioner in hypnosis, and adds dryly that Freud is pursuing suggestion-therapy despite his excellent Viennese training.[61]

Other Views

In his discussion of the meeting at which Freud presented his "Male Hysteria" paper, Ellenberger notes that Bamberger, Rosenthal and Leidesdorf all asserted their familiarity with cases such as Freud described. But, again, it is Charcot's concept of the underlying pathology that is the primary innovation, not his formulation of the clinical picture; and rebuttals that merely profess familiarity with the clinical picture are not to the point. Similarly, Bamberger and Leidesdorf's argument that traumatic neurosis ought not to be viewed as hysteria (an argument with which Freud was well acquainted both from Charcot's rebuttal of Thomsen and Oppenheim and from his own meeting with Thomsen and Oppenheim while in Berlin) really deals with a matter peripheral to the thesis of Charcot and Freud rather than with the central issue.

Moriz Rosenthal, a neurologist, noted that he had written up two cases of male hysteria twenty years before, and added that it had been known for a long time that psychic shock or trauma could induce hysteria. But Rosenthal's *Klinik der Nervenkrankheiten* (1870), to which he was referring, contains a section headed "The Anatomical Findings in Hysteria," which states:

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[60] *Ibid.*, p. 501.
[61] *Ibid.*, p. 501.

> In cases of hysteria, very few people regarded post-mortem examination of the central nervous system as worthwhile; and where it was done (as in the cases of Ollivier, Brodie and Bricquet), one was satisfied with a macroscopic examination, and from the negative results concluded somewhat hastily the absence of material changes in the nerve centers. . . . [However] a gratifying proof that whoever knows how to look will find something here, is provided in a finding discovered by Charcot (*Gazette hebdom.*, Nr. 7, 1865). In a woman, who from the age of fourteen suffered hysterical attacks with later motor disorders, there evolved in the last nine years of her life permanent contractures of all extremities, involving also the muscles of the trunk. Her intelligence remained undisturbed. The patient died of an intercurrent illness, and the autopsy and microscopic investigation revealed sclerosis of the lateral vessels from the bulbus to the lumbar enlargement, together with partial atrophy of the anterior roots.[62]

There is no evidence to suggest that Rosenthal, like Charcot, later repudiated the search for anatomical lesions in hysteria. On the contrary, his comments at the Medical Society meeting indicate that he still held his earlier views.

Bamberger, whose field was internal medicine, never wrote on hysteria and it is impossible to judge what his views of the underlying pathology were. But Leidesdorf, a psychiatrist, shared Meynert's pathoanatomical orientation and stated in his *Lehrbuch der psychischen Krankheiten* that, "every psychic disorder is based on an anatomical change in the brain."[63] It seems safe to assume that his notion of the pathology of hysteria differed little in principle from that of Meynert or Rosenthal.

Meynert, Bamberger, Rosenthal and Leidesdorf are the only people whose comments are cited in the reviews of the meeting. But Ellenberger, in speaking of male hysteria as a phenomenon well-known in Vienna, also mentions the work of Moriz Benedikt. Benedikt's work is noteworthy because he had argued some years earlier for a neurophysiological interpretation of hysteria. In his *Elektrotherapie* (1868) he wrote:

> Hysteria is thus a modification of physiological being—a diathesis—which is characterized by an excessively labile irritability of the nervous system and by an abnormal influence of the various parts of the nervous system on one another; the latter abnormality, following the former, merely reflects an alteration in normal irritability and conductivity. The abnormalities in hysteria manifest themselves primarily in dynamic changes of the nervous system; rarely there appears secondarily an anatomical lesion, such as those Charcot has confirmed for hysterical convulsions.

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[62] Rosenthal, *Klinik der Nervenkrankheiten*, p. 464.

[63] Max Leidesdorf, *Lehrbuch der psychischen Krankheiten* (Erlangen: Enke, 1865), p. 239. See also Lesky, *Die Wiener medizinische Schule*, p. 184 f

> Many apparently anatomical alterations, for example contractions, can be simulated in the living through hysterical disturbances of innervation.[64]

This would seem to contradict the thesis that patho-anatomical models totally prevailed in Vienna in the years preceding 1886. But Benedikt is in fact an exception and an outsider, and Erna Lesky writes of him: "Such avant-garde independence was not received without opposition by the Vienna School of Medicine." [65] Benedikt was a student and protege of Johann Oppolzer, who, while paying lip-service to the primacy of pathological anatomy, emphasized physiology in his own work. Of Oppolzer's position in Vienna Lesky observes:

> If Oppolzer clearly places the physiological standpoint above the pathological-anatomical, and does this especially in the establishment of his therapeutic principles, there lies in this a clear deviation from the direction of the Vienna School—indeed, an open criticism of it.[66]

Oppolzer procured a position for Benedikt in the Vienna General Hospital; but when his mentor died in 1871 Benedikt, who remained a maverick throughout his medical career, moved to the "secessionist" General Polyclinic. His status in Vienna is indicated by the fact that although he had been named Professor-extraordinarius in 1869, while still working for Oppolzer, he was not granted the title of full professor until 1899. Finally, the discussion of Freud's training at the Vienna Medical School and his subsequent research orientation is indicative of what little impact Benedikt's views had on the education of neurologists and psychiatrists in Vienna.

Benedikt was most likely the only man in Vienna who was enthusiastic about the new directions which Charcot's work had taken; and it is no doubt significant that, when Freud left for Paris in 1885, it was Benedikt who gave him a letter of introduction to Charcot.[67]

Charcot and Freud were not alone in their disenchantment with the emphasis on anatomical lesions or in their search for alternate models. As the successes of the anatomical approach continually removed syndromes from the list of pathological problems and left behind disease entities less amenable to that approach, more and more neurologists and psychiatrists in what might be called the patho-anatomical sphere of influence recognized a need for developing alternate formulas. Thus, for example, Auguste Forel, who did research on brain anatomy under Meynert's guidance and earned his Chair as Professor of Psychiatry at the University of Zurich on the basis of his neuroanatomical work, travelled to Nancy in 1887 to study hypnotism under Bernheim and began to formulate dynamic psychiatric models. Others could also be cited. But it was Freud's trip to Paris which marked the first step in the evolution of the most fruitful of the new pathopsychologies generated in the last two decades of the nineteenth century.

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[64] Benedikt, *Elektrotherapie,* p. 413

[65] Lesky, *Die Wiener medizinische Schule,* p. 393.

[66] *Ibid.,* p. 150.

[67] Jones, *Life and Work of Freud,* vol. 1, p. 208.

CHASING THE CURE: IRVING FISHER'S EXPERIENCE AS A TUBERCULOSIS PATIENT

CHRISTINE R. WHITTAKER

Tuberculosis underwent a significant change in image during the nineteenth century. At the beginning of the century, the term "consumption" evoked images of frail women gradually sinking under its grasp, of young poets struggling to express their genius before their premature death. The romantic picture of consumption was reinforced by the belief that it was a hereditary disease. It was almost as though sufferers were exhorted to accept the decree of fate. The change in this concept of the disease can be traced to several factors. The romanticism of tuberculosis began to fade when it was seen in the squalid living and working conditions of the working classes. Koch's discovery in 1882 of the tubercle bacillus demonstrated that tuberculosis was a contagious disease. Gradually people realized that it was the result not of hereditary fate but of poor hygiene and inadequate nutrition. The most important factor, however, in changing the public attitude to tuberculosis was the realization of the economic cost to the nation. In 1909, Edward Otis reckoned the country's annual loss from tuberculosis to be $330,000,000. He estimated that there was a yearly mortality from tuberculosis of 150,000 in the United States. This was equivalent to the total dead, Union and Confederate, during the Civil War. Probably 1,250,000 persons in the United States suffered annually from tuberculosis. The enormous cost was the result of these working lives being cut short by death or hindered by illness, especially since tuberculosis attacked in the prime of life and the majority of its victims were aged between fifteen and thirty-five.[1]

This economic motivation for a higher level of community health was expressed by public health specialists like Hermann Biggs who, in 1911, while chief medical officer of the Health Department of New York City, wrote, "Public health is purchasable. Within natural limitations a community can determine its own death-rate."[2] The cost of public health reforms would be more than off-set by the benefits of longer and more productive working lives. This desire for better methods of prevention and cure of "the great white plague," as tuberculosis was known, coincided with increased medical knowledge of the causes of the disease and the way of life which could effect a cure. An excellent illustration of these factors is found in the case of Irving Fisher (1867-1947), Professor of Political Economy at Yale University, who himself became a tuberculosis victim in 1898. Fisher was to become an ardent crusader in health matters, founding, with another Yale economist, J. Pease Norton, the Committee of One Hundred on National Health. He became convinced that tuberculosis was a preventable disease which could be eradicated to the same extent that smallpox had been wiped out as an epidemic disease. Fisher's own experience with the disease is characteristic of medical knowledge and therapeutic methods at the turn of the century and was to prove the catalyst for his later campaigns for greater attention to national health problems.

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[1] Edward O. Otis, *The Great White Plague* (New York: Thomas Y. Crowell, 1909), pp. 6-19.

[2] The desire for improved community health was not merely an economic concern. Biggs wanted also to reduce the suffering of the working classes: "The reduction of the death rate is the principal statistical expression and index of human and social progress. It means the saving and lengthening of the lives of thousands of citizens, the extension of the vigorous

Fisher was thirty-one when he was diagnosed as suffering from "consumption" or "phthisis," as pulmonary tuberculosis was then known. The symptoms of consumption had been recognized since classical times. Hippocrates had cataloged the dry cough, thick sputum, chest pains, fever, wasting, possible enteritis, clubbed fingers, curved nails, blood-spitting, raucous voice and flushed cheeks of the consumptive.[3] Although diagnosis was straightforward when the majority of these symptoms were present, it was also usually too late to offer much hope of recovery to the patient. The advantage of early diagnosis was gradually realized from the early nineteenth century onwards. The state of medical knowledge at the end of the nineteenth century is illustrated by the section on tuberculosis in *Modern Medicine,* the multi-volume text edited by William Osler, and by such classic textbooks on the disease as Klebs's *Tuberculosis.*[4] The physician was advised in a possible case

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working period well into old age, and the prevention of inefficiency, misery and suffering. These advances can be made by organized social effort. Public health is purchasable." C.-E. A. Winslow, *The Life of Hermann M. Biggs* (Philadelphia: Lea and Febiger, 1929), pp. 230-31.

[3] Gerald B. Webb, *Tuberculosis* (New York· Paul B. Hoeber, 1936), p. 117.

[4] William Osler (ed.), *Modern Medicine, Its Theory and Practice* (Philadelphia and New York: Lea Brothers, 1907); Arnold C. Klebs, *Tuberculosis* (New York: D. Appleton, 1909). Major advances in early diagnosis had resulted from Auenbrugger's announcement in 1761 of his discovery of percussion, which was publicized by Corvisart, and Laennec's discovery of auscultation in 1819. The râles which were so typical of the consumptive could be clearly heard with the aid of the stethoscope even though other symptoms might not yet

of tuberculosis to consider first whether there had been predisposing factors, such as physical strain, poor nutrition, close proximity to another tuberculosis patient. Then he was to look for the following symptoms: loss of weight, fever, loss of strength, cough, haemoptysis, dyspnea, pleurisy (which, it was emphasized, was a symptom of many diseases and not a disease in itself), physical signs from auscultation and the presence of tubercle bacilli in the sputum.[5]

No letters have been preserved giving details of the diagnosis of Fisher's illness, but he described his condition in a letter to his good friend Will Elliot in December 1898:

> The truth is I have a threatening of tuberculosis. It has been caught in its early stages and, as you know, is in such cases curable and generally cured. I have been here [Saranac Lake] three weeks now and have regained my normal strength and weight. The Doctors fully expect me to get well but it takes time, especially to be sure the cicatrix is hardened enough not to allow a relapse.[6]

Fisher had several of the classic symptoms, such as fever and loss of weight. There had also been pre-disposing factors; physicians at this period emphasized the importance of psychological as well as physiological factors both in susceptibility to tuberculosis and in the attempt to achieve a cure. Fisher, in this vein, wrote in 1903 to his brother, Herbert, about the circumstances preceding his attack:

> I will make another secret confession to you. Dr. Russell in '96 frightened me about my heart. It had been strained by hill-climbing on the bicycle but was not wrong in any other way. He said as much, in fact, but the grave way in which he told me . . . frightened me. I feared sudden death, and I feared to hear my heart beat on my pillow. I might have learned that the former fear was groundless had I enquired. The latter interfered with sleep and I ran down steadily for two years and then, as you know, broke down. So that fear was a contributory cause of my tuberculosis as well as a very serious drag on its cure.[7]

Fisher's illness seems, however, to have been diagnosed without the need of the somewhat controversial use of tuberculin to confirm the diagnosis. Tuberculin was the discovery of Robert Koch, one of the major figures in the history of tuberculosis, who in 1882 had proved that a specific bacterium was the cause of tuberculosis. He was able to cultivate this organism from infected tissues; when it was inoculated

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be apparent. Lawrason Brown, *The Story of Clinical Pulmonary Tuberculosis* (Baltimore: Williams and Wilkins, 1941), pp. 62-66; 112-14.

[5] Osler, *Modern Medicine*, pp. 327-31

[6] Letter of Irving Fisher to William Greenleaf Elliot, December 11, 1898, in Irving Fisher Papers (Yale University Library, New Haven). Cited hereafter as Fisher Papers.

[7] Letter of Irving Fisher to Herbert Fisher, January 1, 1903, in Fisher Papers.

into an animal, it caused the specific disease and could again be recovered.[8] In 1890, he announced that he had discovered in tuberculin a cure for tuberculosis. Tuberculin was made from a culture of living human tubercle bacilli. Koch advised its use only in early cases, with a 1 milligram dose which was to be repeated until there was no longer any reaction, then a 2 milligram dose given in the same fashion and so on. He believed that a failure of reaction was due to the destruction and lack of tuberculous tissue, which meant that healing was being accomplished. Tuberculin was, however, unfortunately hailed as a miracle cure. Patients traveled to Germany from all over Europe and the United States in an effort to obtain the treatment. Tuberculin was administered to all sorts of cases and the results were often disastrous. It caused fever and often revived old tuberculous patches in the body. Brown described the resulting panic as the age of "tuberculin terror." It was not until 1900-1901 that the use of tuberculin was revived on any widespread scale. It was used in smaller doses with a far more cautious selection of patients. Its real value, however, was later realized to be its diagnostic ability. Infection with bacilli produced an allergic reaction to tuberculin, which was to be developed by Mantoux into a standardized tuberculin sensitivity test.[9] When Fisher developed tuberculosis, however, the use of tuberculin as a diagnostic test was regarded with extreme caution; Brown advised, "Tuberculin should be used diagnostically only as a last resort. When a patient with suspicious symptoms, with indefinite physical signs and with no tubercle bacilli in the sputum on repeated examinations, wishes a positive diagnosis, tuberculin should be administered." [10]

Fisher had succumbed to tuberculosis at a time when the sanatorium movement was at its height and his case is a classic example of this form of therapy. Open-air treatment for tuberculosis had been advocated in ancient times; sea voyages were often recommended to consumptives by Roman physicians.[11] In the seventeenth century, Thomas Sydenham advocated fresh air and horseback riding.[12] Such methods were, however, often more dependent upon climate and exercise than the cardinal principles of the sanatorium, fresh air and rest. These were first advocated by George Bodington (1799-1881), who drew attention to the fact that there were few deaths from consumption in the country, in contrast with the high urban death rate from tuberculosis. He pleaded for an end to the practice of confining patients to a sick room and advocated instead constant fresh air:

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[8] Webb, *Tuberculosis,* p. 97; Brown, *Clinical Pulmonary Tuberculosis,* p. 101.

[9] Webb, *Tuberculosis,* p. 111.

[10] Osler, *Modern Medicine,* p. 341.

[11] Webb, *Tuberculosis,* pp. 168ff.

[12] "Then ye Doctor sent him into ye County on Horseback (thoe he was soe weak yt he could hardly walk) & ordered him to ride 6 or 7 miles ye first day, (which he did) & to encrease dayly his journey as he shd be able until he had rid 150 miles. . ." John Locke, in *Anecdota Sydenhamia,* quoted by Brown, *Clinical Pulmonary Tuberculosis,* p. 16.

> Consumptive patients are considered hopeless and desperate cases by most practitioners, and the treatment commonly rendered is conducted upon such an inefficient plan as scarcely to retard the fatal catastrophe. One mode of treatment prevailing, consists in shutting the patients up in a close room, to exclude as far as possible the access of atmospheric air, and thus forcing them to breathe over and over again the same foul air contaminated with the diseased effluvia of their own persons . . . . To live in and breathe freely the open air, without being deterred by the wind or weather, is one important and essential [sic] in arresting its progress . . . .[13]

Bodington suggested the creation of special tuberculosis hospitals in the countryside surrounding large cities. His views, however, met with little favor from the medical establishment of his time. The sanatorium movement was to begin not in England but in Germany.

No connection has been traced but the theories of Brehmer were very close to those of Bodington. Hermann Brehmer in his graduation thesis in 1853 had contended that in the early stages pulmonary tuberculosis was always curable. Although his contention was hotly disputed, he started to treat tuberculosis patients with some success and in 1859 he took over a hydrotherapeutic institution at Goerbersdorf in Silesia and founded the first sanatorium. His treatment was based on his theory that tuberculosis was caused by a relatively small heart, insufficient pulmonary blood supply and poor general nutrition. His therapeutic measures were based on a special diet, fresh air, hydrotherapy and graded exercise. Sanatorium treatment, in the form which was to be universally adopted, was, however, devised by one of Brehmer's pupils. Peter Dettweiler had been a patient and then assistant at Goerbersdorf; he had been unable through weakness to exercise much and had discovered almost by accident the therapeutic value of rest. In 1876, he opened his own sanatorium at Falkenstein which was based on the twin principles of fresh air and rest. The sanatorium treatment had been born and was soon to become the most widely accepted method.[14] The principle involved in such treatment had been foreseen in the 1860s by an Edinburgh physician who wrote, "Although it was generally considered by the profession that no remedy and no plan of treatment yet proposed could be depended on in cases of consumption, *it was obvious to the author, that if the process employed by nature could be discovered, and then imitated by art, we might ultimately arrive at the true principle of cure.*"[15] When tuberculosis was healed by nature, it was through the presence of fresh air, adequate nutrition and ample rest, all of which were to be embodied in the sanatorium. Gradually this method was to replace the wide variety of remedies previously employed for consumption, which ranged from fusel oil, alcohol, inhalations and narcotics to cold water, milk, malt or meat extracts and the juice of various herbs.[16]

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[13] *Ibid.*, pp. 90-91.

[14] *Ibid.*, pp. 94-96.

When Fisher's tuberculosis was diagnosed, he left New Haven for the sanatorium at Saranac Lake, which was already well established. Its founder, Dr. Edward Livingston Trudeau, was himself a tuberculosis sufferer who, in 1872, had expected to die within a year or two. He tried a period of horseback riding in the South but there was no improvement and in May 1873 he went to a sporting inn, Paul Smith's, in the Adirondacks, where it was expected that he would die. Instead, he gradually became better, gaining weight and losing his fever, and returned to New York City for the winter. Here, however, he relapsed and so he returned to Paul Smith's in 1874 and stayed there through the following winter. He discovered for himself the value of rest:

> I found, however, I could not walk enough to stand much chance for a shot without feeling sick and feverish the next day, and this was the first intimation I had as to the value of the rest cure, which in after years I applied so thoroughly and rigidly to my patients. I walked very little after this, and my faith in the value of the rest cure became more and more fully established.[17]

Trudeau settled in Saranac Lake and, as he gradually recovered, started to build up his own practice. Other tuberculosis patients followed him there and Trudeau started to consider founding an institution for them: "The idea of building the sanitarium originated on my reading, in 1882, in Anstie's *English Practitioner* an account of a visit to Brehmer's sanitarium in Silesia and a discussion of Brehmer's views as to the value of sanatorium treatment in pulmonary tuberculosis."[18]

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[15] Professor I. H. Bennett, quoted in Carl Both, *Consumption: and Its Treatment in All Its Forms* (Boston: Alexander Moore, 1873), p. 30.

[16] *Ibid.*, pp. 24-26.

[17] Edward Livingston Trudeau, *An Autobiography* (Philadelphia and New York: Lea and Febiger, 1916), p. 107.

[18] *Ibid.*, p. 154. *The Practitioner* was a monthly journal of therapeutics, first published in London in 1868 and called after Dr. Francis Edmund Anstie, who edited it until his death in 1874

Trudeau wanted to do something to help tuberculosis patients of only moderate means and decided to found a sanatorium at Saranac Lake which would charge patients less than the economic rate for their treatment. He succeeded in obtaining funds from several sources and the sanatorium accepted its first patients in 1884. It charged patients $5 a week in 1885, which had risen to $8 a week by 1915; the weekly deficit per patient was $2, rising to $4 by 1915.

Fisher, when he went to Saranac Lake in 1898, probably stayed in a hotel or boarding house, since he did not qualify financially for the sanatorium. In other respects, however, he was a typical patient. Emphasis on incipient cases was employed at the majority of sanatoria in the United States. Trudeau selected patients for the sanatorium carefully:

> I realized at once that if I was to try to obtain curative results I must confine the admission of patients to incipient and favorable cases as much as possible, and refuse to take the acute and far-advanced ones. This brought me any amount of criticism from physicians who thought I was trying to make a personal reputation for myself for cures by treating only selected cases.[19]

The careful selection, however, was essential if the resources of the sanatorium were not to be wasted on patients whose tuberculosis was too far advanced to admit any hope of arresting or curing the disease.

There had been a fairly rapid development of sanatoria in the United States in the years immediately following Trudeau's founding of the sanatorium at Saranac Lake. Many were built on similar lines to Trudeau's; the Adirondack Cottage Sanitarium, as he called it, comprised a central hospital, a laboratory and administration buildings, as well as housing for the patients in the form of many small cottages. Such an arrangement both gave the patients, whose stay was usually lengthy, greater independence and privacy and also minimized the risk of infection. Other sanatoria, however, were built in the more traditional institutional style, such as the first state sanatorium at Rutland, Massachusetts, which was founded in 1898 by Dr. Vincent Y. Bowditch.[20] When *The Journal of the Outdoor Life* conducted a survey in 1905, it found the following numbers of sanatoria: [21]

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[19] *Ibid.*, p. 244.

[20] Brown, *Clinical Pulmonary Tuberculosis*, p. 99, Lawrence F. Flick, *Development of Our Knowledge of Tuberculosis* (738 Pine Street, Philadelphia, 1925), p. 708.

[21] "What is being done in the United States and Canada to stamp out tuberculosis," *The Journal of the Outdoor Life*, II, No. 1 (February, 1905), pp. 18-23.

	Free	Under $10 per week	Over $10 per week
Sanatoria accepting all cases	37	8	14
Sanatoria " early cases only	13	27	30
Sanatoria " advanced cases only	7	3	1

Like many tuberculosis patients, Fisher was not really prepared for the length of time his cure would take. Shortly after he arrived in Saranac Lake, he wrote to his friend Will Elliot:

> I have given up my work till next September and may even take another year off. The cure will probably take place in the first year but a second is considered advisable in most cases. I have thought of going to Colorado Springs but am getting along so well here at present that I shall probably stay till March and then decide on the futura.[22]

This initial optimism was somewhat qualified only a month later in a letter from Fisher's wife, Margaret:

> As soon as this season is over we are seriously thinking of going to Colorado for a year at least, and for longer if necessary—The Doctors say that a year is a *very* short time in which to get perfectly well even from a slight case of tuberculosis.[23]

In February, Fisher caught a slight cold and had to return to bed for several days; he was experiencing one of the many setbacks which were typical of the pattern of recovery and which taken together increased greatly the time needed for a cure.[24]

Time was an essential ingredient in the cure by sanatorium methods; the others were rest, fresh air and an enriched diet. The place of climate and exercise was somewhat more controversial. Fisher became a complete convert to the sanatorium method. By 1902 he was sending to Will Elliot his advice on how to seek a cure:

> Your tuberculosis friend should receive an answer at once. *Time* is everything in a parasitic disease.
>
> T. is cured by persistent faithful *patient* hygiene. This consists of
>
> 1. Air cure
> 2. Food cure
> 3. Rest "
> 4. Mind "
> 5. Avoid reinfection
> 6. Climatic reinfection (i.e. cure)
> 7. Medication very occasionally
>
> The last two are not essential.[25]

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[22] Fisher to Elliot, December 11, 1898, Fisher Papers.

[23] Letter of Margaret Fisher to William Greenleaf Elliot, January 19, 1899, Fisher Papers.

[24] Letter of Irving Fisher to William Greenleaf Elliot, February 6, 1899, Fisher Papers.

[25] Letter of Irving Fisher to William Greenleaf Elliot, March 24, 1902, Fisher Papers.

The sanatorium regime was devised in large measure by Trudeau, adapting Brehmer's and Dettweiler's methods. Its practice spread from Saranac Lake, where Fisher learned it, throughout the country and was adopted, with variations, at every sanatorium and health resort.

The most important ingredient was rest. Together with the breathing of pure, uncontaminated fresh air and ample nutrition, rest was the healing agent in the cure. The importance of rest was not always fully appreciated at first. Brehmer had believed that tuberculosis was caused partly by a small heart and therefore had prescribed exercise for his patients in an attempt to strengthen their hearts. Dettweiler, however, found that rest was far more effective and his methods were adopted by Trudeau who, as mentioned above, discovered for himself the value of rest. Webb and Ryder, in their manual for the tuberculosis sufferer, exhorted, "Do not forget that *of all the countless remedies proposed, Rest alone has stood the test of time.*"[26] The patient was never to hurry, to attempt strenuous exercise or to neglect any of the numerous rest periods advised in the daily timetable, and was to have at least nine hours sleep each night. The philosophy, in Trudeau's words, was to "conquer fate by acquiescence." The patient had to win his way back slowly to a full and active life or, if that proved impossible, build for himself a passive life that was worth living.[27] Rest took over the dominant position previously held by fresh air. In 1915, Lawrason Brown, one of Trudeau's associates at Saranac Lake, published a small handbook on the *Rules of Recovery from Tuberculosis,* in which he told sufferers:

> There are three great medicines in the treatment of tuberculosis: medicines which when rightly used are far superior to those found in any pharmacopoea, Rest, Food and Fresh Air. Formerly this order of their importance was reversed and the value of fresh air and food so emphasized that rest was placed last, hyphenated often to exercise and modified by "properly regulated" . . . The value of rest at the outset of treatment cannot be over-emphasized.[28]

Brown justified this overriding insistence on rest by explaining that complete rest caused the least irritation to the pleura and thus permitted better and faster healing. Latham and Garland, however, gave a more complex explanation of the function of rest, which had been hinted at by Klebs.[29] The cure was, they claimed, brought about by autovaccination, that is, vaccination with the patient's own poison. Rest allowed only very small portions of the poison to be absorbed into the general circulation. The production of the antidote was stimulated and the white phagocytic cells could destroy the small number of invading bacilli. Very carefully regulated amounts of exercise allowed gradually increasing quantities of bacilli into the system without overcoming the resistance of the white phagocytic cells and thus in the course of time the disease was arrested.[30]

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[26] Gerald B. Webb and Charles T. Ryder, *Recovery Record for Use in Tuberculosis* (New York: Paul B. Hoeber, 1925), p. 17.

[27] *Ibid.*, p. 2.

[28] Lawrason Brown, *Rules for Recovery from Tuberculosis* (Saranac Lake, New York: By the author, 1915), p. 22.

Fisher's treatment took place when the importance of rest, especially at the outset of the cure, had been recognized. It was not, however, until a few years later that the extreme emphasis on rest developed and Fisher, from fairly early on in his illness, began to exercise. In his advice to a fellow tuberculosis sufferer, he expressed this balance between rest and exercise:

> *Rest. Keep* rested. Learn to relax and make mind a blank. Exercise but be careful to stop short of fatigue. Then rest thoroughly and exercise again. Alternate rest and exercise is the rule. I got to walk five miles a day when I could not walk over half a mile at a time by walking ten times with perfect rest between.[31]

In subsequent years, rest was given much greater importance. Fisher's amount of exercise was far in excess of that suggested by Webb and Ryder in 1925; the patient was to rest in bed if his temperature was 98.8 or over, or if his pulse was over 80 after 15 minutes in bed. They advised that initially one should, "Be lazy. Be selfish." [32] They also warned the patient that he should expect the cure to require at least three to four years. Fisher was to discover this fact and accept it, in spite of his initial optimism about a relatively quick cure. In January 1900, he wrote to Will Elliot, "The disease is absolutely arrested and . . . barring accidents, it is only a question of time until I shall be entirely cured." [33] He also realized that he would be able only gradually to resume the duties of his Yale professorship, probably at first living in New Haven for only the fall months.

The other major characteristic of sanatorium treatment was initially even more apparent than rest; residents were invariably to be seen resting outdoors, even when the temperature was below zero and they had to be wrapped in several layers of blankets and furs. Fisher became an ardent exponent of the virtues of fresh air, and indeed accorded it first place in his list of essential components of the cure. His suggestions were:

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[29] Klebs, *Tuberculosis,* pp. 679-80.

[30] Arthur Latham and Charles H. Garland, *The Conquest of Consumption* (London: T. Fisher Unwin, 1910), pp. 39-40.

[31] Fisher to Elliot, March 24, 1902, Fisher Papers.

[32] Webb and Ryder, *Recovery Record,* p. 23.

[33] Letter of Irving Fisher to William Greenleaf Elliot, January 2, 1900, Fisher Papers.

> Be out of doors all day
> Sleep out by all means
> Keep windows open when indoors & air move at all times
> Avoid as much as possible contam. of pure air by gas jets lamps, etc. These sources vitiate air much more than occupants. Electric light is the only hygienic light. Average 15 to 20 hours a day out of doors.[34]

In accordance with the majority opinion at that time, Fisher ascribed the most important place in regaining health to fresh air:

> I agree with you fully in regard to the importance of many other things besides fresh air in the matter of preservation and regaining of health: nevertheless, by almost every test by which we can make a comparison, I believe air is of the most importance. . . . As measured by the results obtained in the treatment of tuberculosis, air is much more important than any other hygienic element.[35]

The sanatorium was a monument to this belief. Its patients would sit out during the day, resting on such reclining chairs as the patented "Adirondack Recliner," regardless of temperature, in order to breathe the pure outdoor air. This air was not contaminated with the bacilli which they might emit while coughing or spitting. They were free from the danger of reinfection or infection by other germs. If the temperature was low, as it was during the winter at Saranac Lake and in Colorado, where Fisher went in the spring of 1900, the patient would wear several layers of clothing and then wrap himself in furs and blankets so that he was impervious to the cold. Publications for the tuberculosis sufferer, such as *The Journal of the Outdoor Life,* founded at Saranac Lake by Trudeau, were full of small items advising one how to dress for the cold and which gloves allowed one to write and advertisements describing different styles of balaclava helmet. Fisher adopted such practices wholeheartedly; he wrote from Saranac Lake, "I am sitting out in the porch, thermometer 20° and snow two feet deep! I find ink freezes and so use pencil." [36]

Sleeping out had been part of the cure for the previous half-century.

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[34] Fisher to Elliot, March 24, 1902, Fisher Papers.

[35] Letter of Irving Fisher to William Greenleaf Elliot, April 11, 1901, Fisher Papers.

[36] Fisher to Elliot, December 11, 1898, Fisher Papers.

In a letter to *The Journal of the Outdoor Life,* Trudeau mentioned early advocates of sleeping out. The practice had started in the 1860s when consumptives used to travel west by prairie schooner; sleeping out during the two months or so on the trail often proved one of the most effective methods of cure. After that, several western physicians had recommended to their patients camping out on the plains. The practice was adopted by sanatoria and also by physicians in areas previously thought unfavorable to tuberculosis.

Sleeping out became one of the key features in what was known as the "home sanatorium treatment." When one compares the numbers of tuberculosis sufferers known in the United States with the number of places available at sanatoria, the need for a method of treatment which could take place at home is immediately apparent. The need was even greater when one took into account the poverty of the majority of tuberculosis sufferers and the length of time needed for a cure. One of the first physicians to recommend such home treatment was Dr. C. S. Millet of Brockton, Massachusetts, who in the 1890s advised tuberculosis patients under his care to sleep out at nights. His patients were mainly mill workers who had to work indoors during the day.[37] Similar methods were adopted at sanatoria and were also recommended to patients on their return home, as a means to ensure the permanence of their apparent cure. Fisher considered the effect significant:

> Did I ever tell you of the decided leap forward I took in health as soon as I began to sleep out of doors, although before making this change, I had been sleeping in a room with the door wide open to the East and two windows wide open to the South! . . . the very night that I slept out, I noticed a decided difference and within a very few days my symptoms abated very markedly. The change was so striking as to make it extremely probable, on the basis of one fact alone, that the slight difference between the purity of nature's air and the air in my bed chamber had produced this difference in my health.[38]

This experience made Fisher almost fanatical on the subject of fresh air. He described fifteen different degrees of purity of air and claimed that if only the human race would live in the top half, "consumption would become in ten years as rare as leprosy." [39] After his return to New Haven in the fall of 1901, he continued to keep up "the practice of out-of-door life in accordance with the principles which I have learned." He wrote to Will Elliot:

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[37] Edward Livingston Trudeau, "First advocates of outdoor sleeping," *The Journal of the Outdoor Life,* II, No. 6 (July, 1905), p. 137.

[38] Fisher to Elliot, April 11, 1901, Fisher Papers

[39] *Ibid.*

> I see that Mrs. Fisher and the children are not far behind [in the practice of open-air living] I have an out-of-door study in which I am now writing, and have arranged my bedroom, which is a corner room, with both windows wide open at the top and the head of the bed pushed up against one of them, with a screen on the side to prevent a cross-draught.[40]

The majority of handbooks written to aid the tuberculosis patient contained plentiful advice on how to adapt the sanatorium method to treatment at home. The key factor was outdoor living. The patient was advised to build a porch or verandah to his house, in direct imitation of the porch on sanatorium cottages or the verandahs at large institutional buildings. If this were not possible, there were other possibilities. Galbreath described the sleeping shacks which he had built in Colorado and Maryland. Knopf's handbook was full of advice, down to measurements and building instructions for the suggested devices. The most popular recommendation for patients in cities was the window tent. This contraption worked on the same principles as Fisher's sleeping arrangement but shut the patient off completely from the rest of the room, thus making it safer for others, if need be, to sleep in the same room. A miniature tent, covering the upper half of the bed, was open on one side to the window, thus ensuring that the patient had constant fresh air.[41] Another advantage to treatment at home was that it prepared him for living and working in that climate when recovered, rather than requiring an adjustment to a completely different climate after several months in a resort climate. The whole philosophy of fresh air was contained in Brown's advice: "One should be so 'faithful to the cure' that his conscience will trouble him when he remains inside; in other words, he should develop an 'outdoor conscience.' "[42]

The importance of climate was still a controversial subject at this time. Fisher followed the typical pattern of financially able consumptives in going to climates which supposedly were most beneficial. Many authorities advised a cool, dry, sunny climate, with reasonable extremes of temperature, as the most beneficial.[43] The most popular areas were the Adirondacks and the states of Colorado, Arizona and New Mexico. When Fisher was first diagnosed as having tuberculosis in November 1898, he went to Saranac Lake. He left the following March for Colorado Springs. He remained there until May 1900 before going to Santa Barbara, California, which was also recommended for its climate and which Fisher wanted to try since, like New Haven, it was at sea level.[44] Fisher himself was aware of the changing emphasis in the ingredients of the cure. He wrote to Will Elliot from Santa Barbara, "Of course it is something of an experiment for me but the Doctors think that the risk in coming was extremely small, and as you know the modern idea is that it is not the climate but the life that cures." [45] By 1902, he was telling his wife, "I don't fear any climate as to any effect on my lungs and I don't like to think that you have any of that old fear. It doesn't matter whether the weather is dry or not either. But I think our summers ought to be inland because our winters are not." [46]

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[40] Letter of Irving Fisher to William Greenleaf Elliot, November 5, 1901, Fisher Papers.

[41] Thomas Crawford Galbreath, *T B. Playing the Lone Game Consumption* (New York: Journal of the Outdoor Life Publishing Company, 1915), pp. 27-31; S. Adolphus Knopf, *Tuberculosis, A Preventable and Curable Disease* (New York: Moffat, Yard and Co., 1910), pp. 60ff; Joseph H. Pratt, "The home sanatorium—a successful method of treating pulmonary tuberculosis," *The Journal of the Outdoor Life,* III, No. 1 (February, 1906), pp 8-9.

[42] Brown, *Rules for Recovery,* p. 65.

The older remedy was to prescribe the climate out west for every consumptive. Former sufferers, such as George Price, wrote handbooks like *Gaining Health in the West: Being Impressions of a Layman, Based on Seven Years' Personal Experience with "Climate."* Price warned patients not to expect a quick cure; they must learn to accept the fact that they had tuberculosis and settle down to a lengthy cure rather than fool themselves imagining that their attack was not serious and would soon be cured. He described in some detail the resorts and small climate differences in Colorado, Arizona and New Mexico.[47] There was, however, one major obstacle in prescribing climate as a remedy—the expense. Comparatively few patients could afford the cost of transportation to the west and accommodation there for at least a year. Possibly it was this economic factor which brought about a change in medical theory. Patients who could not afford the trip west started to follow an open air life at home and the results demonstrated that climate was only a minor factor in the cure. Handbooks started to advise patients to consider carefully whether they could really afford a lengthy stay in the west; if not, they would be far more successful following sanatorium methods at home then spending all they had on a short stay in Colorado, only to return while still seriously ill. Galbreath reminded tuberculosis patients of Dettweiler's opinion that, "elements like temperature, humidity and atmospheric pressure are really of slight consequence; pure air and proper nutrition are the main things." [48] He also cited the similar conclusions of the Committee on the Influence of Climate in Pulmonary Tuberculosis which had been appointed by the National Association for the Study and Prevention of Tuberculosis. Ward's major treatise on climate concluded that it was, at most, an important help in the treatment of tuberculosis, in so far as it afforded clean and pure air.[49]

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[43] Cf. Brown's advice "that coolness, dryness and active motion of the air are factors to be sought after and further, as James puts it, that 'many atmospheres are good enough to be breathed which are not good enough to be lived in.' " *Rules for Recovery,* p. 56. Webb and Ryder thought that most patients did better in varied, semi-severe climates than in very mild and equable ones, *Recovery Record,* p. 39.

[44] Letter of Irving Fisher to Margaret Fisher, January 28, 1901, Fisher Papers.

[45] Letter of Irving Fisher to William Greenleaf Elliot, June 18, 1900, Fisher Papers.

[46] Letter of Irving Fisher to Margaret Fisher, September 2, 1902, Fisher Papers.

[47] George B. Price, *Gaining Health in the West: Being Impressions of a Layman, Based on Seven Years' Personal Experience with "Climate."* (New York: B. W. Huebsch, 1907), pp 13-14, 26-40.

Climate was still a vested interest in some quarters. Sanatoria and physicians in climatic resorts such as the Adirondacks and the Rockies were dependent upon continued belief in at least some climatic benefits. The American Climatological Association represented this interest and argued that there was a real, if not indispensable, benefit from a "healthy" climate. In May 1906, *The Journal of the Outdoor Life,* which as a publication of the Saranac Lake sanatorium had an interest in maintaining the claims of climate, contained an article which contended that: "It is now a well-established fact that climate is an unimportant factor in the treatment of tuberculosis. It cannot be gainsaid that treatment can be conducted in certain climates more comfortably than in others, but tuberculosis can be cured in all climates." [50] The author attributed much of the supposed beneficial effect of climate to the incentives for living out of doors in a good climate. While not disagreeing with the major premise that tuberculosis could be cured in all climates, the editors of *The Journal* nevertheless went on record to state the value of climate in providing change, refreshment and pure air.[51] In his handbook on tuberculosis, Flick brought a common sense verdict to several of the debates over treatment. His comment on climate is perhaps the best, for it encompasses psychological and environmental factors: "New York was at one time a health resort for consumptives, so was Pennsylvania and so was every State in the Union in turn and in order going west. The reputation earned in the primaeval forest was lost in the cultivated fields. When consumption became prevalent, it was said that the climates had changed." [52]

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[48] Galbreath, *T.B.* p. 16 One wonders also whether patients found Denver an exceedingly inhospitable place if the "phthisiophobia" was such as described. Cf. Galbreath, *Chasing the Cure in Colorado* (Denver, Col : by the Author, 1907), pp. 27-33.

[49] Robert DeCourcy Ward, *Climate* (New York: G. P. Putnam's Sons, 1908), p. 208.

[50] J. W. Pettit, "The open air treatment of tuberculosis," *The Journal of the Outdoor Life,* III, No. 4 (May, 1906), p. 146.

[51] *"In re* climate," *The Journal of the Outdoor Life,* III, No. 10 (November, 1906), p. 389.

The remaining ingredient of the cure which all authorities agreed upon was nutrition. Consumptives usually lost weight quite severely and an enriched diet was essential if they were to regain strength and energy. Sanatorium patients were usually given three meals a day, with extra lunches of milk and raw eggs between meals. Milk and eggs were thought particularly valuable in building up strength. Handbooks are full of advice on the easiest way to learn to enjoy raw eggs. The success of the diet was judged by the amount of weight gained. Webb and Ryder's *Recovery Record* contained detailed charts to record weight change and *The Journal of the Outdoor Life* advertized similar charts for sale.[53] The diet was also to be fairly bland to avoid digestive difficulties which tended to be common in tuberculosis sufferers. Rest before and after meals was strongly recommended. There is no evidence to show when Fisher first became interested in diet problems, but he became a zealot on the subject. In his advice to a fellow sufferer, he wrote:

> Rest on back with eyes closed and body *relaxed* an hour before meals. The stomach needs rest before its work. After meals rest ½ hour in semi-*sitting* posture. Eat *slowly*. Masticate until all taste is gone and food disappears without voluntary effort to swallow. Any residue in mouth reject. Eat *heartily very*. Eat a *variety*. Eat what agrees with you. *Milk* & *meat* & other nitrogenous foods are best in general. Raw eggs are excellent. I have known of a person eating a *dozen* at end of each *meal!* [54]

Later, however, Fisher was to change his opinion on the value of meat. He gradually gave up eating meat and considered his new diet far superior. His stay at Battle Creek Sanitarium confirmed his belief in a predominantly vegetarian, "aseptic" (i.e. pure or poison free) diet.[55] He later conducted various diet experiments, using his Yale students as subjects in order to try to determine the most important elements of diet to ensure good health.[56]

[52] Lawrence F. Flick, *Consumption: A Curable and Preventable Disease* (Philadelphia: David McKay, 1903), p. 96.

[53] Webb and Ryder, *Recovery Record,* pp 7-8, 80ff.

[54] Fisher to Elliot, March 24, 1902, Fisher Papers.

[55] Letter of Irving Fisher to Margaret Fisher, January 7, 1905, Fisher Papers.

[56] Letter of Irving Fisher to William Greenleaf Elliot, April 20, 1906, Fisher Papers.

The cure for tuberculosis at this period was a total way of life; Fisher was a good candidate for such a regime. His personal psychological traits, including a somewhat compulsive nature and a strong sense of duty, made him an excellent patient, once convinced of the necessity of following such a regime. Timetables intended to guide the patient, such as Brown's, left him little or no free time to himself.[57] Compulsory rest, short exercise periods, frequent meals and long periods of sleep filled the patient's day as effectively at home as if he had been in a sanatorium. Fisher responded to this prescription with complete faith. Flick had written that "Every action of the patient must be brought under control." [58] For many physicians, this included the state of the patient's mind. Tuberculosis sufferers were constantly exhorted to accept their fate, give up fighting the disease mentally and instead put their mental energy into maintaining a healthy cheerful disposition. This aspect of treatment was a direct reflection of late nineteenth century preoccupation with the practice of mind cure.[59] It allowed the patient to feel that he was constructively aiding his cure. Fisher applied the principles of mind cure to himself and then, in the usual pattern, tried to convert others. He wrote to his brother that he had managed to conquer "the blue devils," by which he meant the depression lasting for almost three years after he knew he had tuberculosis, by "hard work and the application of auto-suggestion." The discovery that he could control his nerves was, in his opinion, the most important lesson he had ever learned. He wanted to help others to see, "that fear is at the bottom of most diseases, especially 'chronic diseases'—fear and foul air." [60]

The distinction was frequently made in works on tuberculosis between an "arrested" case and a "cured" case. Although the disease might be arrested and the patient apparently be in good health, one could not be certain of a cure until several years later, when there was no further danger of a relapse and when the lung tissue had healed. Fisher's case was fairly quickly arrested and he could safely be pronounced "cured" a few years later. His recovery from tuberculosis is a classic example of the therapeutic methods employed at the turn of the century. This period marked a breakthrough, when it was recognized that tuberculosis was indeed, as many authors on the subject proclaimed, a preventable and curable disease. The sanatorium method, although lengthy, offered a good chance of recovery provided that the disease had been diagnosed at a fairly early stage. The means of cure was not dramatically changed until the discovery of effective drugs a half century later. Fisher's case was typical in his adoption of the "cure" as a total way of life. The cardinal principles of rest, fresh air and nutrition ruled all his actions. It was also typical that the convert became a zealot; like many other former tuberculosis patients, Fisher advocated a similar regime as the means to a healthy life. He became an ardent crusader for greater attention to health and hygiene issues throughout the country. His voice was added to the growing chorus of physicians and others calling for prophylactic measures.[61] His most important work was in the foundation of the Committee of One Hundred, a national campaign whose aim was to secure the establishment of a federal department of health.[62]

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[57] Brown, *Rules for Recovery,* p. 63.

[58] Flick, *Consumption,* p. 212.

[59] For a brilliant description of such movements, see Donald B. Meyer, *The Positive Thinkers* (Garden City, N.Y.: Doubleday, 1965).

[60] Irving Fisher to Herbert Fisher, January 1, 1903, Fisher Papers.

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[61] The emphasis on prevention was the result of Koch's discovery of the tubercle bacillus. As soon as it was generally realized that tuberculosis was a contagious disease, the cry was raised for greatly improved personal and public hygiene. Health education programs were instituted to teach people how to avoid spreading the bacillus (especially by taking care of sputum). The campaign had some comical side-lights, such as a call for shorter women's skirts because trailing skirts might pick up bacilli from the street.

[62] George Rosen, "The Committee of One Hundred on National Health and the Campaign for a National Health Department, 1906-1912," *American Journal of Public Health,* LXII, No 2 (February 1972), pp. 261-63.

INFLUENZA 1918-1919: A STUDY IN MORTALITY

ROBERT S. KATZ

In 1918 and 1919, most of the world was affected by a devastating epidemic of influenza. One author has classed this pandemic, along with the plague of Justinian and the Black Death, as one of the three most destructive outbreaks of disease the human race has ever known.[1] Coming at the close of the First World War, influenza was responsible for more deaths than four years of fighting. In the United States alone, approximately 500,000 deaths were recorded.[2]

Perhaps the most puzzling aspect of the great epidemic is the widely observed fact that mortality was highest in the age group that should be most resistant to disease. Young, vigorous adults, those between the ages of twenty and forty, were most susceptible to the ravages of the disease and suffered approximately half the deaths, as shown in regional and national studies,[3] with older age groups only slightly affected.[4] In Chicago, for example, most of those dying at Cook County Hospital were between the ages of twenty-five and thirty.[5] This mortality distribution is both unusual and difficult to understand. Why those traditionally in robust physical condition were so devastated by influenza is not an easy question to answer. However, in approaching a possible explanation, it must be kept in mind that epidemics, like all other historical phenomena, should be viewed in the context of their times. Thus, the 1918-1919 influenza epidemic was just as subject to sociological, political, and geographical considerations as was any other event of its era.

Studies of even the most recent writings on influenza pandemics show that a workable explanation of the unusual mortality distribution has not yet been offered despite the amount of statistical material available.[6] However, since morbidity and mortality were broken down and analyzed in almost every conceivable fashion, by age groups, sex, economic brackets, and localities, the qualitative results of these statistical studies will be used in this analysis.

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[1] Geddes Smith, *Plague on Us* (New York: Commonwealth Fund, 1941), p. 25.

[2] Selwyn D. Collins, "The influenza epidemic of 1928-1929 with comparative data for 1918-1919," *Am. J. Publ. Hlth,* 1930, *20:* 119-129.

[3] W. H. Frost, "The epidemiology of influenza," *Publ. Hlth Rep., Wash.,* 1919, *34:* 1823-1836. E. O. Jordan, *et al.,* "Influenza in three Chicago groups," *J. Infect. Dis.,* 1919, *25:* 74-95. C. H. Stuart-Harris, "Influenza," in *The History and Conquest of Common Diseases,* ed. by Walter R. Bett (Norman: Univ. of Oklahoma Press, 1954), p. 76. Bernard Davis, *et al., Microbiology* (New York: Hoeber, 1969), p. 1331.

[4] Rollo Britten, "The incidence of epidemic influenza," *Publ. Hlth Rep., Wash.,* 1932, *47:* 303-339.

[5] A. A. Hoehling, *The Great Epidemic* (Boston: Little, Brown & Co., 1961), pp. 40 & 147.

An unusual and fruitful attempt to understand the epidemic was made in the state of Connecticut. During the most serious outbreak of communicable disease in the history of the state, 56% of all deaths from influenza fell in the twenty to forty age group, while only 9% came at ages over forty-nine. The group between twenty and twenty-nine accounted for 30% of all deaths while the next age group was a close second with 26%.[7] The authors of the study, Winslow and Rogers, were naturally puzzled by the mortality distribution and discussed what would seem to have been the most popular explanation of the phenomenon available at that time, that exposure to previous influenza outbreaks in the United States, the last one referred to was that of 1892, granted an immunity to subsequent attacks. This would explain why the death rate was quite low in the over-forty-nine group. However, as the authors pointed out, this immunity should also extend to the thirty-to-forty age bracket.[8] Yet, this age group suffered just as heavily as the division below it.

Before considering their interesting study any further, some additional comments about influenza and its effects in 1918-1919 should be added. One analysis of the epidemic showed that if those affected were divided on the basis of income into four groups, ranging from well-to-do to very poor, the case fatality was almost twice as high in the very poor group as it was in the well-to-do division.[9] As income went down, incidence and mortality went up.

Another contemporary study indicated that while the morbidity of influenza in the young adult group was approximately equal on a sex basis, or perhaps even slightly higher for females, the mortality in the group was higher in men.[10] The study also indicated that the epidemic was generally milder in the South and Middle West than in the Northeast and Far West.[11]

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[6] Jay P. Sanford, "Influenza: consideration of pandemics," *Adv. Internal Med.*, 1969, *15:* 419-453. Hoehling, (n. 5), *passim.*

[7] C. E. Winslow and J. F. Rogers, "Statistics of the 1918 epidemic of influenza in Connecticut," *J. Infect. Dis.*, 1920, *26:* 185-216

[8] *Ibid.*

[9] Edgar Sydenstricker, "The incidence of influenza among persons of different economic status," *Publ. Hlth Rep., Wash.*, 1931, *46:* 154-170.

[10] W. H. Frost, "Statistics of influenza morbidity," *Publ. Hlth Rep., Wash.*, 1920, *35:* 584-598.

Finally, some considerations as to the medical nature of influenza should be made. Influenza is a respiratory infection transmitted from man to man by means of infective droplets. Its diagnosis, especially in the early stages, seems somewhat vague and difficult, and its treatment, even now in the age of antibiotics, is still largely composed of bed rest and gradual return to full activity. Studies made in 1957 indicated that crowding is the major disposing factor to epidemics, and that epidemics may be avoided or mitigated in communities where there are large numbers of immune individuals.[12] The immunity acquired in a community is apparently of some durability.[13]

Serious epidemics of influenza had, of course, occurred before 1918. A major world-wide epidemic took place in 1889-1890,[14] with the outbreak of 1892, as was mentioned earlier by Winslow and Rogers, affecting primarily the United States.

Keeping this information in mind, some answers to the puzzling mortality distribution of 1918-1919 can be evolved. Although reasons for rejecting the effects of immunity were discussed earlier, this explanation should be reconsidered. Can the existence of a large group of people in the young adult group who were not immune to influenza be postulated? Winslow and Rogers seem to be operating on the assumption that everyone alive in 1892, or for that matter in 1890, would have had the opportunity to develop immunity. Influenza, however, would seem to be primarily a disease of cities. Contemporary evidence indicates that the epidemic of 1889-1890 primarily affected urban areas.[15] Crowding and man-to-man contact, typical conditions in the urban environment, were already listed as prime factors in the spread of epidemics.

The answer to the problem lies, as was mentioned earlier, in the concept that epidemics must be evaluated in the context of the sociological, political, and geographical factors of their times. One of the most important phenomena in late nineteenth and early twentieth century America was, undoubtedly, that of immigration.[16] Vast numbers of foreign-born came to this country to take up residence. However, as statistics indicate, this immigration was of a very particular type. From the end of the century, until the outbreak of the First World War, the number of immigrants of Italian, Russian, Austrian, Polish, and French-Canadian stock increased vastly.[17] In addition, most of those entering the country were between the ages of fourteen and forty-four, an economically productive group that rapidly sought employment.[18] A specific example can be made of the Polish immigrants of the time, of whom 82% were in this age bracket.[19] It should be noted that more than three-quarters of all the new immigrants were males, as opposed to a more even sex distribution in previous waves of immigration.[20]

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[11] *Ibid.*

[12] Robert R. Wagner, "Influenza," in *Harrison's Principles of Internal Medicine,* ed. M. W. Wintrobe, *et al.,* (New York: McGraw Hill, 1970), pp. 956, 957, 959.

[13] Edwin Kilbourne, "Influenza," in *Textbook of Medicine,* ed. Paul Beeson and Walsh McDermott (Philadelphia: Saunders, 1971), p. 370.

[14] John F. Townsend, "History of influenza epidemics," *Ann. Med. Hist.,* 1933, n.s. *5:* 533-547.

[15] "The history of influenza," *Practitioner,* 1919, *102:* 104-108. C. O. Stollybrass, "The periodicity of influenza," *Lancet,* 1920, *198:* 372-373.

Of prime importance is the fact that these new immigrants predominantly came from rural areas.[21] This generality can be supported by references to the backgrounds of each of the major ethnic groups that comprised the "new immigration." The Italians came, in particular, from southern Italy and Sicily, agricultural regions of low population density.[22] They were, for the most part, peasant farmers from isolated, self-contained villages.[23] Polish immigrants, as well, came from a similar rural, agricultural, peasant background.[24] The immigrants from the Austrian domains were really a combination of Slavs from various parts of the old Austro-Hungarian empire.[25] By and large, the bulk of the Slavs came from agricultural, non-industrial areas.[26] Most of the Russians who entered the country at the turn of the century had been land-poor peasants or farmers.[27] The large contingent of Jews from Russia and Poland came from agricultural sections or small villages.[28] Finally, the French-Canadians, who also came in large numbers shortly before the First World War, were mostly peasants or children of peasants who left the farms to come to America.[29] Thus, it is evident that most of the immigrants came from backgrounds where the chances of exposure to influenza would be small. Living in isolated, rural communities where contact with outsiders was rare,[30] they would not have been exposed to the man-to-man droplet contact, the crowding, and the constant influx of new infective vectors vital to the spread of influenza. Consequently, they would not have had the chance to develop immunity.

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[16] Samuel Eliot Morison, Henry Steele Commager, and William E. Leuchtenburg, *The Growth of the American Republic* (New York: Oxford University Press, 1969), II, pp. 103-106.

[17] Imre Ferenczi, *International Migrations, Statistics* (New York: National Bureau of Economic Research, 1929), pp. 389-392, 444-448. Oscar Handlin, *The American People in the Twentieth Century* (Cambridge: Harvard Univ. Press, 1954), p. 49. Oscar Handlin, *Immigration as a Factor in American History* (Englewood Cliffs, N.J.: Prentice-Hall, 1959), p. 26. Morison, Commager and Leuchtenburg (n. 16), p. 108.

[18] Ferenczi (n. 17), pp. 444-448.

[19] Joseph Swastek, "Polish Americans," in *One America*, ed. Francis J. Brown and Joseph S. Roucek (New York: Prentice-Hall, 1952), p. 155.

[20] Donald R. Taft and Richard Robbins, *International Migrations* (New York: Ronald Press Company, 1955), p. 375.

[21] Handlin, *The American People in the Twentieth Century* (n. 17), pp. 49-50, 57, 68-69. Taft and Robbins (n. 20), pp 483-484. George Stephenson, *A History of American Immigration* 1820-1924 (New York: Russell and Russell, 1964), pp. 68, 87, 89, 91. Morison, Commager and Leuchtenburg (n. 16), p 109.

[22] A. M. Ratti, "Italian Migration Movements from 1876 to 1926," in *International Migrations, Interpretations*, ed. Walter Wilcox (New York: National Bureau of Economic Research, 1931), p. 448 Stephenson (n. 21), p. 68.

[23] Taft and Robbins (n. 20), pp 483-484. Francis J. Brown, "Italian Americans," in *One America* (n. 19), p 246. Handlin, *The American People in the Twentieth Century* (n. 17), p. 57.

With these facts in mind, the activities of the immigrants after their arrival in the United States might also be considered. The new immigrants congregated in the cities, both large and small, especially in the Northeast, and for the most part worked as unskilled industrial laborers.[31] Italians often worked in the construction fields,[32] Russian and Polish Jews in factories,[33] and French-Canadians in the mills.[34]

It was shown earlier that the preponderance of the new immigrants entering the country in such large numbers prior to the 1918 influenza epidemic were males between the ages of fourteen and forty-four. In considering the situation in 1918, then, the recent immigrants, many now young adults, the age bracket hardest hit by influenza, not having been exposed to previous influenza outbreaks, settled in urban areas and found unskilled industrial employment. They lived in crowded tenement conditions aptly described by one author as "breeding places for vice, crime, and epidemics." [35] All these factors combined to make them the ideal victims for the devastating effects of influenza in 1918.

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[24] Swastek (n. 19), p. 155. Taft and Robbins (n. 20), pp. 499, 505

[25] Stephenson (n. 21), p. 87.

[26] Taft and Robbins (n. 20), p. 506. Stephenson (n. 21), pp. 89, 91.

[27] Joseph L. Lichten, "Russian Americans," in *One America* (n. 19), p. 129.

[28] Morison, Commager, and Leuchtenburg (n. 16), p 108. Taft and Robbins (n. 20), p. 499.

[29] Handlin, *The American People in the Twentieth Century* (n. 17), p. 49-50.

[30] Oscar Handlin, *The Uprooted* (Boston: Little, Brown, 1951), p 9.

[31] Taft and Robbins (n. 20), p. 375. Morison, Commager, and Leuchtenburg (n. 16), p. 111.

[32] Robert Foerster, *The Italian Immigration of Our Times* (Cambridge: Harvard Univ. Press, 1919), p. 353.

[33] S. Dingol, "The Jews," in *Immigrant Backgrounds,* ed. Henry Fairchild (New York· John Wiley, 1927), p. 129.

[34] Handlin, *The American People in the Twentieth Century* (n. 17), pp. 49-50.

Little needs to be said about the social conditions of industrial work in this period. Laboring for extremely low wages, for very long hours, in poor and often unhealthy circumstances, the employee barely stayed at the subsistence level.[36] The immigrant workers of 1918, so many of whom were in the twenty to forty age group, lacking immunity, might have contracted influenza without recognizing its vague early symptoms and gone off to work at what must have been physically demanding jobs in industry. Not only did the unhealthy factory conditions give them the opportunity to infect fellow workers, but the strenuous nature of the jobs probably made the infection much worse, with pneumonia a possible complication,[37] and death following shortly afterwards.

It must now be asked whether this hypothesis can be verified. Unfortunately, mortality statistics were not broken down on a national scale on the basis of ethnic groupings. However, the study in Connecticut, mentioned earlier, does give an ethnic analysis on a statewide level. The evidence from Connecticut is probably of general applicability since Connecticut was, like many other Eastern states, highly urbanized and industrialized. The authors of the work, after presenting their data, concluded that race was a factor in the incidence of mortality. The proportion of influenza deaths is lower than would be expected in the case of persons of native, Irish, English, and German stock, and higher than would be expected in the case of Russian, Austrian, Polish, and French-Canadian stock, with an enormously high death rate among Italians.[38] Since most Irish, English, and Germans had come to this country well before 1890, as can be seen from immigration statistics,[39] it can be concluded that they would have been in the United States long enough to have been affected by previous outbreaks of influenza and to have developed immunity. Winslow and Rogers observe that the high death rate among the new immigrants might be attributed to the fact that many immigrants were young adults.[40] The opposite hypothesis is offered here—that the young adults died because so many of them were immigrants who lacked the protection of previously-gained immunity. To reinforce this concept, it should be noted that the old, on a nationwide basis, seem to have had some lasting immunity from previous epidemics, as was shown by their low incidence of infection.[41] In spite of equal morbidity, as was mentioned earlier, mortality was much lower among women than men, since almost all the new immigrants were male. High mortality was present in the one-to-five age group, probably because of their lack of immunity and natural frailty. Finally, the large number of new immigrants in the cities of the Northeast could explain the relative severity of the epidemic in this area and its lesser severity elsewhere.

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[35] Morison, Commager, and Leuchtenburg (n. 16), pp. 110-111.

[36] F. R. Dulles, *Labor In America* (New York: Crowell, 1949), pp. 98-99.

[37] Frost (n 3).

[38] Winslow and Rogers (n. 7).

[39] Ferenczi (n. 17), pp. 380, 384-388 Morison, Commager, and Leuchtenburg (n. 16), pp. 106-108.

In this paper, thus, an explanation of the great mortality of young adults during the 1918-1919 epidemic influenza has been proposed. Immunity gained during previous exposure to influenza epidemics is, in fact, the key concept. The new immigrants of the post-1890 period came predominantly from rural backgrounds and were consequently not exposed to infection, as were those who had lived in cities, foci of influenza epidemics. When the immigrants arrived in the United States, during the late nineteenth or early twentieth century, they lived in very poor, crowded conditions in cities and took unskilled employment. Large numbers of male immigrants would have been in the young adult group in 1918 and, lacking immunity, would have been unprotected against the scourge. As a result, they died of the disease and its complications and produced the unusual mortality distribution that is so often noted in discussions of the great epidemic in America.

A viable explanation of the young adult mortality in 1918-1919 has, then, been evolved. The answer developed shows that a proper understanding of epidemics and their effects must be based on the premise that an epidemic is as much the product of the sociological, geographical, and political factors of its time as is any other historical event.

Acknowledgment

The interest and advice of the late Professor Charles Culotta, Bryn Mawr College, is gratefully acknowledged.

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[40] Winslow and Rogers (n. 7).

[41] *Ibid.*

NOTES AND COMMENTS

THE CONGRESSIONAL DEBATE OVER THE SEAMAN'S SICKNESS AND DISABILITY ACT OF 1798:

THE ORIGINS OF THE CONTINUING DEBATE ON THE SOCIALIZATION OF AMERICAN MEDICINE

HARRY J. CAMPBELL

Our political forebears were concerned about health problems, just as we are today. The U.S. Congressional debate over the Seaman's Sickness and Disability Act of 1798 is a good example of their concern.

The health of the merchant marine sailor of the late 1700s was important to American politicians. Already there had been a long heritage among the English and their American colonists in the provision of medical care for sailors. Since 1588, the year of the success of the English over the Spanish Armada, English sailors were cared for in many ways, beginning with direct medical services provided at a hospital in Greenwich, England. This tradition was transplanted to America, for in 1780, the Commonwealth of Virginia began to tax seamen to establish a fund for their medical care. After the American Revolution, trade with Europe expanded. The United States Congress began to consider the merchant marine as part of its responsibility.[1]

In 1798, Representative J. Parker stated his position:

> It would be wise and politic . . . to begin measures which must place these persons (seamen) in a better situation than at present. The British sailor looked up to Greenwich Hospital as an asylum when all his toils are over; and perhaps it may be owing, in some measure, to this circumstance, that the British sailors are so valiant a set of men.[2]

In the course of their work, the sailors contracted serious infections, nutritional disorders, and injuries. Their acute illnesses were treated in the nearest American ports by whatever resources the communities had at their disposal. Most of these communities were reluctant to extend medical services to the merchant marines because of their low income and social status. When the seamen were given the opportunity to receive medical care, they were cared for in the local poorhouses. Apparently the seamen did not consider the charity medical care that they received in the poorhouses to be satisfactory. This feeling can be seen in the report of the Committee on Commerce before the 29th Congress in 1846, a report which advocated the establishment of a new marine hospital in New Bedford, Massachusetts:

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[1] Ralph C. Williams. *The United States Public Health Service, 1798-1950* (Washington: Commissioned Officers Association of the United States Public Health Service, 1951), pp. 23-29.

[2] *History of Congress.* "Sick and Disabled Seamen," House of Representatives, April, 1798, p. 1391.

> The town poorhouses are at present the only tolerable places where they can be provided for. It is well known that sailors have great repugnance to becoming inmates of an almshouse: this feeling is so strong that they will resort to small and badly ventilated apartments in their boarding houses, rather than be seen amongst the paupers.[3]

In order to encourage seamen to remain at their posts, the Congress passed the Seaman's Sickness and Disability Act in 1798.[4] This law levied a twenty cents a month tax on each seaman for the purpose of providing temporary relief of sickness to those who needed it. The money was supposed to be spent in the city that collected it and the surplus used for purchasing separate hospitals for use by the sick seamen.[5] A year later, the program was extended to the navy. In 1837, Congress acted to include the river-boatmen of the western waterways. Later these marine hospitals were to form the bulwark of the personal medical care system of the U.S. Public Health Service.

Before the 1798 act was passed, a Congressional debate was held in which several issues were aired; two of them are still with us today. The first was whether the seamen's medical care should be provided by public charity or by a tax on their income. Congressman Sewall of Massachusetts, the chief opponent of the act, spoke first in the debate. Here are his views, as recorded by the Congressional scribe:

> Mr. Sewall said he was unwilling to say anything against this bill; but the passing of it would be attended with many inconveniences and objections, which ought to be mentioned. He allowed the objections principally applied to the part of the Union from whence he came, where provision is already made for sick and disabled persons of every description, sailors as well as others, with which every person in the community is charged. Seamen in that quarter, therefore, now pay their full proportion of money for public charity; and if they were again to be called upon by this act,

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[3] *Committee on Commerce,* "Disabled Seamen," 29th Congress, First session, House of Representatives, April 14, 1846, Report #569.

[4] *Op. cit.* (n. 1 above), full text of the Act, pp. 29-31.

[5] *Report of the Supervising Surgeon of the Marine Hospital Service of the United States.* Executive Documents, No. 131, 1872, p. 7.

> they would be made to pay in two different ways for the same object, without any advantage to themselves, because they are already entitled to relief in case of sickness or disability. But if this was not the case, he doubted the propriety of taxing seamen only for the support of what ought to be considered as a public charity. He thought the laws of reason and charity called upon the public at large in support of unfortunate men of this description, and that the burden ought not exclusively to be laid upon them.[6]

Sewall saw no reason to establish a second mechanism to obtain care for seamen and advised that local charity should provide for their care.

Representative Pinckney of South Carolina favored the act. He was not convinced of the efficacy of the "laws of reason" cited by Sewall. He thought that the federal government should be involved in the financing of personal medical services. Pinckney

> . . . thought it only reasonable and equitable that these persons (the seamen) should pay for the benefit which they were themselves to receive, and that it would be neither just nor fair for other persons to pay it.[7]

Analysis of Sewall's and Pinckney's arguments demonstrates the origin of a dilemma that still plagues us today—should medical services be financed by charity or by a tax mechanism? Part of the current debate over the national health insurance centers around whether the financing of a health insurance plan for citizens should be a function of the federal government or of a private sector.

The second issue in the 1798 congressional debate was the consumer's voice in policy-making. What did the seamen themselves have to say about the proposed act, and what attitudes did their political representatives have toward them? The views of the act's sponsor, Congressman Edward Livingston from New York, can be seen below:

> . . . Livingston did not believe the people themselves would object to the tax in question. If he knew anything of the careless, honest nature of this class of men, they would not object to so trifling a sum as twenty cents a month for so valuable an object. A sailor, said he, is concerned only for the present, and is incapable of thinking of, or inattentive to, future welfare; he is, therefore, a proper object for the care of government, and whilst he can provide an asylum for infirmity or old age, by the sacrifices of a few gills of rum, he will not scruple to do it.[8]

Livingston's paternalistic attitude was not held by all the congressmen. Representative Gallatin had the following to say:

> This bill assumed a principle for its foundation, the truth of which (Gallatin) was

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[6] *Op. cit.* (n. 2 above), p. 1386.

[7] *Op. cit.* (n. 2 above), p. 1387.

[8] *Op. cit.* (n. 2 above), p. 1388.

> not acquainted with, viz: that the seamen of the United States are not able to provide for themselves, and therefore, that it is necessary to provide a sum for their relief, in cases of sickness or disability. He had not seen this distinction between them and other classes of citizens, and therefore, believed them to be capable of taking care of themselves. He knew there were, in all communities, a number of men who were not sufficiently provident to lay by money to afford relief in sickness, or to make old age comfortable; but he did not know that there was a greater number of this description of persons among seamen than among others. Institutions of the kind recommended in this bill might be used in other countries where there was a distinction between sailors and other citizens, but in the United States, he had not been able to discover any material distinction between them and other classes of men. (Gallatin) thought it better to leave the business as at present, and suffer this class of people to provide for themselves, or to be provided for in the same way in which other poor and sick or disabled are supported.[9]

Whereas Livingston considered the seamen to be a helpless lot and therefore eligible for special attention by federal legislation, Gallatin considered them to be like everyone else, claiming that they should fend for themselves under existing institutions. It is difficult to find evidence of congressmen consulting the seamen to determine their wishes. The major criteria used by the congressmen were whether the legislation would be beneficial to the Union, whether it was their duty to provide care to the seamen, or whether it was the moral thing to do. Wishes of the seamen were apparently unimportant.

Certainly no discussion in Congress today is complete until the opinions of several consumer groups are heard, such as labor, senior citizens, minority groups and the poor.

In summary, debate over the Seaman's Sickness and Disability Act of 1798 demonstrates an early expression of one of today's dilemmas in health care—charity medicine versus a federally sponsored tax program. One also sees that the role of the consumer was still undefined at that time. In many respects, the debate over personal medical services has not changed since 1798, but the attitudes of the involved citizens have changed a great deal.

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[9] *Op. cit.* (n. 2 above), p. 1392.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

APPOINTMENTS IN THE HISTORY OF MEDICINE

Six appointments in the history of medicine have been announced recently—several beginning in September 1974, or soon afterwards, the remainder early in 1975. Princeton University and the Universities of Minnesota, Pennsylvania, and Wisconsin are joined in this by two Canadian schools. Most of these appointments are at the level of assistant professor; on 1 November 1974, however, Dr. Ruth G. Hodgkinson becomes the Jason A. Hannah Professor of the History of Medicine and Related Sciences at Queen's University, Kingston, Ontario. This chair is supported by an endowment from the Associated Medical Services, Inc. Dr. Jason A. Hannah, who has long been President and Managing Director of Associated Medical Services in Ontario (what were once the functions of A.M.S. have been assumed by the government as Provincial responsibilities), has established chairs for medical history in the five Ontario universities which have medical faculties. The only other one to make an appointment at this time is the University of Western Ontario, London, Ontario, where Dr. Paul Potter has just been named Assistant Professor. Other appointments are expected shortly. Unrelated to these, except as they occur in the same field of scholarship, are the important American appointments. Of the total number of six announced below, five are new jobs, very recently established.

A.M.S. has at the same time set up, in Toronto, to link and coordinate the program in medical history, the Jason A. Hannah Institute for the History of Medical and Related Sciences, Dr. Hannah being founder and Director. Dr. G. R. Paterson, Professor of Pharmaceutical Chemistry at the University of Toronto, has recently accepted appointment as Assistant to Dr. Hannah. Approximately three thousand rare books, purchased from the Medical Society of London, have been placed in the Fisher Rare Book Library in the University of Toronto. This Jason A. Hannah Collection is to be augmented in size and scope by the A.M.S.

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The six professorial appointees named below are all of them armed with the Ph.D., or its equivalent, while two of them—Dr. Potter and Dr. Maulitz—have the M.D. as well. All have published in the history of medicine and all are, of course, actively pursuing plans for more research and writing. We hope to welcome their further work in the pages of the *Bulletin.*

University of Western Ontario

Dr. Paul Potter has been appointed Assistant Professor in the Department of the History of Medicine and Science, University of Western Ontario. Dr. Potter received his M.D. degree from McGill University and an M.A. in Greek from McMaster University. Under a German Academic Exchange Service Scholarship (1971-72) and a Canada Council Doctoral Fellowship (1972-73), he completed a graduate program in Greek, Latin and the history of medicine at Kiel University. His doctoral thesis, under Professors H. Diller and F. Kudlien (in German), is an edition, with translation and commentary, of the Hippocratic treatise *De Morbis III*. At present Dr. Potter is reworking his doctoral thesis for inclusion in the *Corpus Medicorum Graecorum* and is preparing a complete collection of the fragments of Herophilus with translation and introduction.

University of Wisconsin

Dr. Ronald L. Numbers has been appointed to the new position of Assistant Professor in the Department of the History of Medicine, University of Wisconsin Center for Health Sciences, Madison. Professor Numbers taught medical history at Loma Linda University from 1970 to 1973 and was a fellow in the Institute of the History of Medicine at the Johns Hopkins University during the past year. His recently completed book, *Ellen G. White: Prophetess of Health*, is scheduled for publication in 1975. The appointment will add considerable strength to the teaching program at Wisconsin, which now reaches undergraduates, students in the health sciences, and pre- and postdoctoral fellows.

Paul Potter

Ronald L. Numbers

University of Minnesota

John M. Eyler, Ph.D., has been appointed Assistant Professor of the History of Medicine in the Department of History of Medicine, University of Minnesota. Dr. Eyler received his Ph.D. degree in the History of Science from the University of Wisconsin in 1971 with a dissertation on "William Farr: An Intellectual Biography of a Social Pathologist." In 1971 he was awarded a postdoctoral fellowship in the history of medicine and biological sciences by the Josiah Macy, Jr. Foundation and in 1972 received a National Institutes of Health postdoctoral fellowship. During 1973-74 Dr. Eyler was an Assistant Professor in the Department of History, Northwestern University.

University of Pennsylvania

The Department of History and Sociology of Science at the University of Pennsylvania announces the appointment of Russell Maulitz, M.D. (Duke), Ph.D. (Duke) to an Assistant Professorship in the Department. Dr. Maulitz has also been appointed Assistant Professor of the History of Medicine in the Department of Medicine of the University Medical School. Dr. Maulitz's research interests lie in nineteenth-century European medicine. As well as teaching in the Medical School and the Graduate School of Arts and Sciences, Dr. Maulitz will aid in the development of new undergraduate programs for premedical students.

John M. Eyler

Russell Maulitz

Princeton University

Dr. Toby Gelfand has been appointed Assistant Professor in the Program in History and Philosophy of Science at Princeton University. Dr. Gelfand had served since 1971 as Instructor and then Assistant Professor in the Department of the History of Medicine at the University of Minnesota. He received his Ph.D. from the Institute of the History of Medicine at the Johns Hopkins University in 1973 for a dissertation entitled "The Training of Surgeons in Eighteenth-Century Paris and Its Influence on Medical Education." At Princeton, Dr. Gelfand joins Dr. Gerald L. Geison in the teaching of existing courses in the history of biology and medicine and in the designing of new ones with the aid of a $35,000 grant made to Princeton in 1972 by the Josiah Macy, Jr. Foundation.

Queen's University

Principal Ronald L. Watts of Queen's University has announced the appointment of Ruth G. Hodgkinson, B.Sc. (Econ.), Ph.D., as the Jason A. Hannah Professor of the History of Medical and Related Sciences to become effective November 1, 1974. A native of England, Dr. Hodgkinson received her education in economics and social history at the University of London. She has been Research Consultant and Assistant Editor at the Wellcome Institute for the History of Medicine in London. One of the small group of professional historians with special knowledge of the wide-ranging influence of health and diseases on society and on the course of history, she is also concerned that medicine, past, present and future, should be viewed in its broadest context. At Queen's Dr. Hodgkinson will conduct a program in the history of medicine which will be open to all Queen's students, but with preference given to medical students.

Toby Gelfand

Ruth G. Hodgkinson

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

APPOINTMENTS IN THE HISTORY OF MEDICINE

Six appointments in the history of medicine have been announced recently—several beginning in September 1974, or soon afterwards, the remainder early in 1975. Princeton University and the Universities of Minnesota, Pennsylvania, and Wisconsin are joined in this by two Canadian schools. Most of these appointments are at the level of assistant professor; on 1 November 1974, however, Dr. Ruth G. Hodgkinson becomes the Jason A. Hannah Professor of the History of Medicine and Related Sciences at Queen's University, Kingston, Ontario. This chair is supported by an endowment from the Associated Medical Services, Inc. Dr. Jason A. Hannah, who has long been President and Managing Director of Associated Medical Services in Ontario (what were once the functions of A.M.S. have been assumed by the government as Provincial responsibilities), has established chairs for medical history in the five Ontario universities which have medical faculties. The only other one to make an appointment at this time is the University of Western Ontario, London, Ontario, where Dr. Paul Potter has just been named Assistant Professor. Other appointments are expected shortly. Unrelated to these, except as they occur in the same field of scholarship, are the important American appointments. Of the total number of six announced below, five are new jobs, very recently established.

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$10,000. The curriculum was much the same as in earlier years. The charge for tuition was $240; it rose to $600 by 1920. The student body retained its exceptionally high quality.

In the war of 1914-17 the Johns Hopkins Medical School was deeply involved, because of its pre-eminent reputation, its nearness to Washington, and in no small degree to the national standing of its best-known professor, William H. Welch. About seven hundred graduates of the school were on active duty at home and abroad, including a number of the senior professors and hospital administrators. The Hospital manned Army Base Hospital No. 18, the first university-sponsored medical unit to go to France. Located in the Vosges area, it rendered valuable service during the fighting around Chateau Thierry. Also many Johns Hopkins men served with the British Army. The medical students were taken into the Student Army Training Corps. Turner gives a wryly amusing account of their uncomfortable and largely futile experience.

The Johns Hopkins Hospital was heavily involved in the influenza epidemic of 1919. As in all of the nation's medical centers, Johns Hopkins physicians, nurses, and students stayed at their posts of duty and ten among them died.

Once the grave emergencies of war and epidemic disease were over, things began to move rapidly again in the nineteen-twenties. The administrative relations of the School and Hospital were looked into and overhauled—a business to which Turner gives careful attention, together with a review of the finances of both institutions. The "second faculty" were now taking over departmental headships—Lewis Weed in anatomy, Mansfield Clark in biochemistry, Eli K. Marshall in pharmacology, W. G. McCallum in pathology. There was great activity in building during the 1920s and early 1930s, resulting in a new pathology building, the women's clinic, an outpatient building, Hampton House for the nurses, Hurd Hall auditorium, the Osler and Halsted clinics, and a new physiology building.

The School and Hospital were becoming more complex in internal structure as well as in their housing. Turner calls special attention to the ever-increasing specialization of the clinical departments, illustrated by the creation of the Wilmer ophthalmological clinic, and less visibly, by new subdepartments, for example those for tuberculosis and syphilis.

The largest step forward of the time was the organization of the third major element of the Johns Hopkins medical institutions, the School of Hygiene and Public Health. In 1916 William H. Welch, aided by W. H. Howell and W. W. Ford, drafted a plan for such a school, for presentation to the Rockefeller-supported General Education Board. When financial support was assured in 1917, a faculty was formed with Welch as director, and as professors Howell (physiology), E V. McCollum (nutrition), Raymond Pearl (biostatistics), and Charles J. Tilden (sanitary engineering). The school began its teaching in the old physics building on West Monument Street, and moved in 1925 into its own large new building adjacent to the Hospital grounds and the School of Medicine. From the first, the School of Hygiene had diverse and often competing aims—training for research, for high-level public health administration, and for the practical routines of sanitary laboratory work and inspection. Turner tells how the emphasis on these aims shifted back and forth as the School of Hygiene went on to gain international distinction in both research and practice.

The indispensable Doctor Welch retired from the directorship of the School of Hygiene in 1928, but was called on to lead one more great project for Johns Hopkins —the creation of a combined library for the three institutions and of an institute for the history of medicine. Readers of the *Bulletin of the History of Medicine* will appreciate Turner's narrative of the Institute's early years. Dr. Welch accepted its directorship somewhat reluctantly, for he knew that he was an amateur medical historian, although a talented one. What an odd group he got together!—Fielding Garrison, serious, judicious compiler of the best American text book of medical history, the unsteady cosmopolite Stephen D'Irsay, and the unfrocked, later refrocked priest John R. Oliver, whose two semi-autobiographical novels portray a troubled soul. But the great library formed by merging the collections of the two Schools and the Hospital provided an adequate research apparatus for the historians, the handsome new library building housed the group in comfort and dignity, and the Institute got off to a good start on its fruitful career.

When Welch retired for the last time, in 1931, his place was offered to Harvey Cushing. Here the present reviewer can add a trifling episode to Turner's story, which will at least illustrate the dearth of experienced medical historians in the United States at the time. In Christmas week of 1931, while on a visit to Baltimore, I chanced to meet Dr. Welch in a bookstore. He told me that Cushing had declined the chair of medical history and, to my astonishment, said that if I were interested he would place my name before the committee. Feeling that my historical experience was too narrow, and also unwilling to give up a flourishing program of endocrinological research at Rochester, I declined this tentative proposal, and was happy to learn a few months later that Henry Sigerist had accepted the appointment. Turner gives a full and understanding account of Sigerist's accomplishments and of his reasons, partly self-created, for his resignation in 1947.

An interesting chapter entitled "Visions Unfulfilled" deals with a topic not always discussed frankly in institutional histories—that of projects that were not realized. Lewis Weed, himself an anatomical neurologist, dreamed of a great neurological institute for research and therapy, worked hard for it while dean of the Medical School, but could not secure either the necessary funds or a man to head the institute, of sufficient standing to tempt foundation support. Projects for a dermatology clinic, a department of biophysics and a university-level school of nursing also failed, and an effort to create a joint department of bacteriology and immunology for the two Schools, was defeated by lack of funds caused by the depression of 1929-1930.

Welch's retirement in 1931 at the age of 80 took away, as Turner says, the *de facto* integrating force in the Johns Hopkins medical institutions, a force that was badly needed in that time of financial stress after the depression and the soon-to-follow World War II. The ties among the three institutions, affiliated but never closely coordinated, with different administrative heads and separate governing boards, were loosened and for some years to come the three were "muddling through, but the mud was getting deeper."

Lewis Weed, for whom the ill-defined position of director of the School of Medicine had been created, had something of Welch's ability to formulate long-range plans and to persuade the foundations to support them, but (here the reviewer speaks) he lacked Welch's ingratiating charm and was not popular with the clinical faculty. Moreover, he was drawn so deeply into war-time scientific affairs that his influence on the Baltimore institutions was limited The events of 1930-1940 are therefore colored, in Turner's account, by a feeling of nostalgia for the good days of the past. The very title of his volume, "Heritage of Excellence," suggests something to lean upon in a time of uncertainty. There were administrative matters at issue between the School of Medicine and the Hospital; the full-time system was still controversial; the depression had left financial problems soluble only by state and municipal support.

Nevertheless, medical schools and hospitals are very hardy institutions, for they meet a never-ending human need. The three Johns Hopkins medical institutions did not stand still even though for a while their pace was uncertain, the route not always clear, and the leaders sometimes pulling different ways. The School of Medicine in 1930 had a committee make a thorough survey of its problems and needs. Among the topics reviewed was the full-time system, which was regarded as worthy of continuance. Warfield T. Longcope, professor of medicine, by his even-tempered courtesy, did much to maintain good relations between the full-time and the part-time men. To the latter, incidentally, Turner pays tribute throughout the volume for their loyalty and great services to the School of Medicine and Hospital. As time took its toll of the older teachers and administrators, competent new men came forward. Useful affiliations were made with the Baltimore City Hospitals, the Sydenham Hospital, and others. The School of Hygiene, well launched in the preceding decade, made great progress, to which Turner appropriately allots a full chapter.

Turner devotes 17 pages to the appointment, in 1941, of Alfred Blalock to the chair of surgery, which he evidently and no doubt justifiably regards as one of the great events in the School's history. He describes the long and troubled search for a successor to Dean Lewis, in which Isaiah Bowman, president of the University, finally took a hand and secured Blalock's election. Blalock's contribution to the surgery of the heart and great vessels is enthusiastically told, with due credit to Dr. Helen Taussig and to Blalock's remarkable assistant, Vivien Thomas, for their part in the development of the operations for malposition of the great arteries. Among Turner's many excellent sketches of careers of faculty members, that of Blalock is the warmest.

The role of the Johns Hopkins medical institutions in World War II was as great as that in 1917-1918, especially on the home front. The revived Base Hospital No. 18 had a wretched experience of stagnation in Assam. The 118th had better luck in Australia and the Philippines. As for the medical students, the Armed Forces had learned much from the earlier war and their service as prospective medical officers was much more agreeable than that of their predecessors.

Turner comes to the end of his period in 1947, with the inevitable reorganization after the war, and bright hopes for the future. It is impossible to review the details of this comprehensive record, gathered, we may be sure, with immense labor but set down with clarity, warmth, and wisdom. Turner has omitted nothing, not even the Pithotomy Club show and the Turtle Derby. Alumni of the two schools and the Hospital will appreciate the numerous portraits of their teachers and fellow-students. Appendices furnish much valuable information, including a list of the portraits of faculty, trustees, and benefactors that adorn the buildings, with their locations and the names of the artists. The Johns Hopkins Press has provided excellent typography, good engravings, and attractive binding.

Among the many excellences of our Johns Hopkins heritage is the distinguished historical record of the medical instiutions begun by Alan Chesney, to which Thomas B. Turner's volume is a notable addition.

Reviewed by GEORGE W. CORNER, SR., M.D., Emeritus Professor of Embryology, The Johns Hopkins University, Executive Officer of the American Philosophical Society, Philadelphia, Pa.

The Correspondence of Henry Oldenburg. Edited and translated by A. Rupert Hall and Marie Boas Hall. Volume VII, 1670-1671. Madison: University of Wisconsin Press, 1970. xxvii+600 pp. Ill. $20.00. Volume VIII, 1671-1672. 1971. xxvi+663 pp. Ill. $20.00.

With Volumes VII and VIII, which carry the Oldenburg *Correspondence* into 1672, this reviewer feels impelled to a public recantation. I have reviewed all of the previous volumes in the edition, for the most part in a carping tone as I expressed my doubts that the edition was worth the effort. My estimate of the value of Oldenburg's *Correspondence* has in fact risen with each volume, and with these latest two I am ready to acknowledge my conversion. The edition is indeed worth the effort; it is bound to become one of the important sources for the history of science during the third quarter of the 17th century.

Part of its importance derives from the scientific content of the letters. The burden of my criticism of earlier volumes rested on the small amount of scientific content. It has increased with each volume—an index of the growth of a scientific community—and the editors rightly insist that the scientific content of these volumes is high. Most important in this respect is the correspondence of Lister, Willughby, and Ray on the generation of insects and the anatomy of plants, but there are also important letters on anatomy from Malpighi, on astronomy from Hevelius, Flamsteed, and Cassini, and on mechanics from Leibniz, Wallis, and Pardies. A number of men destined to contribute significantly to 17th century science enter the *Correspondence* in person for the first time—Flamsteed, Ray, Grew, Swammerdam, and overshadowing even these, Leibniz and Newton. The usefulness of the edition, however, will never rest primarily on its scientific content, which can only supplement published works. The letters of lesser scientific interest may prove to be the more valuable in sketching the intellectual milieu out of which modern science grew. Oldenburg himself, the central figure in the Royal Society's activities, seems not to have had a clear perception of what investigations would prove fruitful. If he received important papers from Leibniz and Newton, his heart lay more in the Baconian universal natural history that he continued to solicit and in the reports of prodigies that he continued to receive. He clearly relished the lengthy descriptions of salt springs and the like that he received—"useful towards the filling up of some blank corner in a page in the history of nature," as one correspondent candidly acknowledged.

Most important of all is the light the edition casts on the scientific community itself. Oldenburg's most enthusiastic correspondents were men isolated in provincial locations who had no colleagues and peers at hand and found through him their contact with the scientific movement. Because of Oldenburg, who played in his generation the role Mersenne had played for an earlier one, a scientific community that extended beyond intellectual centers such as Paris and London existed. In an Italy bereft of its former leadership, Malpighi looked to England for intellectual contact, and during the period covered by these volumes sent the Royal Society his treatises on embryology and vegetable anatomy for publication in London. Members of the *Academia curiosorum* in Germany, such as Sachs and Vogel, eagerly sought the support and good opinion of the Royal Society. Closer to home, Lister in York communicated with Willughby and Ray on questions of natural history through Oldenburg. Most eager of all was the young Leibniz, temporarily located at Mainz and determined to force his way into the community of learning to which he knew he belonged. Wooing both the Royal Society and the *Académie des sciences,* he dedicated one of the two parts of his *Hypothesis physica nova* to each. "Forgive me for bludgeoning you with questions," he wrote, without ceasing to pound away for one moment Members of the *Académie* in Paris, on the other hand, were wholly uninterested in Oldenburg. Even Huygens seldom wrote from Paris though he valued the Royal Society, and Oldenburg was reduced to following the *Académie's* activities through second-hand reports. In contrast, Father Pardies, who was not a member of the charmed circle of favorites though he was in Paris, gratefully absorbed Oldenburg's information. As the editors have insisted, Oldenburg's *Correspondence* furnishes a vast mine of information on the nascent scientific community of western Europe.

Reviewed by RICHARD S WESTFALL, Ph D., Professor in the Department of the History and Philosophy of Science, Indiana University, Bloomington, Ind.

L. J. RATHER. *Addison and the White Corpuscles: an Aspect of Nineteenth-Century Biology*. London: Wellcome Institute of the History of Medicine, 1972. (Publications of the Wellcome Institute of the History of Medicine, New Series, Vol. XXII). x + 236 pp. £3.00.

In a concise and elegant monograph, Prof. L. J. Rather plumbs one of the more neglected recesses of the history of scientific medicine. What questions were uppermost in the minds of mid-nineteenth century investigators, such as William Addison, about the blood and its solid or corpuscular components? How were the "colorless globules" seen to function? Addressing these questions, Rather fulfills his avowed purpose of avoiding the Whiggish historical fallacy of presentism: thus, as he demonstrates deftly, for many years the major issue concerning the white corpuscles was not that of defining precisely the role of the leucocyte in inflammation. Instead, the colorless globules and their passage through the capillary walls (the existence of which was itself in debate in the late 1830s) are to be viewed within the then unfolding context of the blastema theory. Stressing as it did the formative capacity of the fluid matrix of the organism to generate new solid tissue, this theory gained momentum and prominence as Theodor Schwann's cell theory of the 1839 *Microscopical Researches,* with its inherent analogy of solid elements "crystallizing out" of the fluids, and took hold of the scientific imagination of the 1840s.

Rather's *Addison* does not purport, then, to be a biographical profile of the Malvern practitioner who first proposed the leucocyte emigration hypothesis. It is instead a careful delineation of the blastema theory, of its rise and fall as seen through the prism of the work of "a relatively minor figure" who, for a while, came forward as one of its staunch opponents. In the vigorous debate over the colorless globules' function, Addison's antagonists were harshly critical of his hypothesis, which would after all have effectively both supplanted and controverted the blastema. Yet, ironically, the years which saw Addison steadily retreating from his early notion of white cell diapedesis also saw the decline of the blastema.

The opponents of the blastema theory, among whom Addison found himself at first, repudiated the idea that body fluids, which observably could give rise in certain instances to solid matter (e.g. fibrin on exposure to air), were responsible for the nutrition and indeed the very formation of the parenchymal cells through which such fluids coursed. In distinction to this view, Addison posited the colorless blood corpuscle as the source of tissue nutrition. Inflammation, in turn, represented but an abnormal accentuation or distortion of this essentially normal process; gradations, indeed, were possible on the scale between simple nutrition and extreme "inflammatory turgescence." But the blastema theory remained a powerful explanatory tool in medicine, potent enough in fact to outstrip for many years its promising early rival—Addison's colorless globule. In such an intellectual context, the science of the cells was thus constrained to look elsewhere—notably to embryology and botany—for empirical support favoring *omnis cellula e cellula.*

In the latter part of his monograph, Rather assesses Addison's retreat, especially in the 1856 *Cell Therapeutics,* to a more muddled and less cogent position with respect to the outward migration of leucocytes, their function in nutrition, and in the inflammatory response. After a brief examination of Virchow's impact, particularly regarding his rejection in 1855-58 of the blastema, and the attempts of the British to keep it alive, the author concludes the main trunk of the book with a brief (23 of a total 236 pages) consideration of the seminal work of Julius Cohnheim. It is this section that is in many ways the most interesting and appetite-whetting, but least wholly satisfactory, of the present work. Its elliptical quality was probably unavoidable within the compass of this monograph; one may hope that Prof. Rather will flesh it out in the near future with a longer work on this critical phase of the history of inflammation. In the meantime, there are tantalizing questions. What happened, for example, between 1861, when Cohnheim published his thesis on the inflammation of serous membranes (not, as Rather states, on pyemia) and 1867, when he published the better-known "Ueber Entzündung und Eiterung" in *Virchows Archiv?* Were the intervening studies of Friedrich von Recklinghausen critical in showing him the path, or were there other factors? And as for Addison, why did his view of white cell function ultimately blur in its focus? In recounting these later developments, Rather often suggests the logical interconnections but faintly, relying on the formula of "published exposition—published reception" to carry along his narrative.

An even more truncated account of Metchnikov and phagocytosis serves as the first section of an epilogue; in a second section, Rather returns to a "critical assessment" of Addison. He bases the appraisal on four criteria: "methodology, originality, consistency, and contemporaneity." On each Addison receives mixed marks. What falls out, however, is the image of a practitioner whose speculative science lagged ever more behind the new experimentalism. But for the reasons underlying such a lag (Addison's scientific contacts? English provincial science?), we are told (note, p. 203) only that "some biographical information might be helpful here"—and so it might.

There follow three useful appendices dealing with related topics. The third of these, on the subsequent fortunes of the blastema theory, is particularly of interest. There is a good index, and in general the book is well produced. The voluminous notes, though comprehensive, are rendered less useful by placement at the end of each chapter. In all, Dr. Rather has given the history of pathology a needed boost. He has presented an introductory essay in an area of growing interest. Hopefully it will not be the last of his efforts to bring his readers the fruit of years exploring a terrain that is still largely uncharted.

Reviewed by RUSSELL C. MAULITZ, M.D., Ph.D., Assistant Professor in the Department of History and Sociology of Science and Assistant Professor of the History of Medicine in the Department of Medicine, University of Pennsylvania, Philadelphia, Pa.

REDMOND MCLAUGHLIN. *The Royal Army Medical Corps.* New York: Hillary House Publishers; London: Leo Cooper, 1972. 121 pp. Ill. $6.75.

This slim volume, written by a prominent surgeon who served in the RAF in World War II, is part of a "Famous Regiments" series now appearing in Great Britain. This is the first one-volume popular history of British military medicine. The story begins in 1660, when Charles II raised the first standing army, and ends with the Suez adventure.

The first 238 years of this history, up to the reformation of the Corps in 1898, are covered in the first 18 pages—Hunter, McGrigor, and Nightingale are epitomized, the Army Medical School leaps into existence in 1860, and the Crimea flashes by. Seven pages cover the Boer War, and the remainder of the text is devoted mainly to World Wars I and II. A few points are of interest; the RAMC "share" of Victoria Crosses seems to get smaller with each war; Almroth Wright's typhoid vaccine was given only to unmarried volunteers (to avoid affecting "virility"); and the continued over-representation of Scots and Irish in the Corps. Bruce and Leishman are mentioned, but not Ross and Liston. There are a number of good war-time photographs and sketches; no index, no references, and no bibliography.

Carping ill behooves a member of a Corps whose last one-volume history was published in 1929, *but*—as well written and entertaining as Dr. McLaughlin's book is—it is not an historical treatment of the subject. The lay reader will find interesting brief accounts of gallant men and some of the triumphs and blunders of British Army military medicine. The historian will have to consult the official histories and records, memoirs, and the journal literature while waiting for the definitive monograph.

Reviewed by ROBERT J. T. JOY, M.D., Colonel, Medical Corps, U.S. Army; Deputy Director, Walter Reed Army Institute of Research, Washington, D.C.

HELEN E. MARSHALL. *Mary Adelaide Nutting: Pioneer of Modern Nursing.* Baltimore and London: The Johns Hopkins University Press, 1972. ix+396 pp. Ill. $12.00.

The line between history and biography is hard to draw and, perhaps because Helen E. Marshall is an American historian, she has managed very successfully to make this life of Adelaide Nutting also a record of nursing's progress from the time Miss Nutting graduated from the Johns Hopkins Hospital School of Nursing in 1891 until her retirement from Teachers College in 1925. Almost immediately after her graduation from the Hopkins, Miss Nutting was appointed head nurse on Dr. William Osler's medical ward, and when Isabel Hampton left a few years later to marry Dr. Hunter Robb in Cleveland, Miss Nutting succeeded her as director of the School. She seems also to have assumed the dominant role in nursing organizations that had been Miss Hampton's until her marriage.

Adelaide Nutting is revealed as a charming and gifted woman who was not only a talented writer but a fine pianist. She loved people, books, and travel, and seems to have been courted by several men, but she probably thought the rewards of marriage less satisfying than those offered by the development of a new profession that held such promise for human welfare. She spoke of Miss Hampton's marriage as "the biggest mistake of her life."

Miss Nutting was thirty years old when she applied for admission to the Hopkins and in describing her life up to this point the author has helped us understand the kind of woman she was. Although her parents came from New England stock they were Canadians, as Miss Nutting continued to be. She never returned to Canada to live, but she remained devoted to her family and visited them almost yearly throughout her life. Isabel Hampton seems to have recognized Adelaide Nutting's unusual gifts as soon as she met her, even though the thirteen other "probationers" included Emma Clever, who held a master's degree, Norma Anthony, a musician, and Mary Emory, a mother of five children who graduated first in the class.

The intimate picture of Johns Hopkins Hospital, the nursing and medical schools in these early years, is one of the most interesting parts of this engrossing biography. Isabel Hampton, the outstanding figure in nursing, the three great doctors, Osler, Welch, and Halsted, leaders in medicine, and Dr. Hurd, an authority on hospital administration, all came to be Adelaide Nutting's friends as well as professional associates. Extracts from the public addresses of Dr. Osler and Dr. Welch show that they fully appreciated the contribution a socially experienced, educated nurse could make and that they supported in principle Mrs. Robb's and Miss Nutting's belief that the education of nurses should be established on an economically sound basis and freed from an apprenticeship system that exploited students. The story of Miss Nutting's accomplishments at the Hopkins, the preliminary study period, the revision of the curriculum, and the reduction in the working day are described, as is her life-long struggle for an adequate endowment and the development of university control for the School.

As head of the School of Nursing and an active citizen Miss Nutting led a full and interesting life in Baltimore. She was admired and appreciated by the medical staff and her colleagues in nursing and almost worshipped, though possibly feared, by her students. She found time to work with Mrs. Robb, Lavinia Dock, and others on the creation of nursing organizations—international, national, and state—and on state registration for nurses. But in spite of her accomplishments she suffered from overwork and frustration at the Hopkins. She counted her failure to get the School established on a sound economic footing as one of "lost causes."

Miss Marshall describes Miss Nutting's unwillingness to be lured from Baltimore by Dean Russell of Teachers College, Columbia University, to direct its new program in institutional management. Mrs Robb and Miss Nutting, through the American Society of Superintendents of Training Schools for Nurses (later the National League of Nursing Education), had promoted this innovative program and both had contributed lectures on a regular basis. The work appealed to Miss Nutting but she couldn't bring herself to leave a city she had grown to love and a school whose development was so close to her heart. But when Dean Russell asked her a second time and Annie Goodrich, Superintendent of the New York Hospital Training School for Nurses, urged her to consider it, she decided to accept. Always "delicate" and forced by illness to take long leaves of absence, she concluded that she would never survive the combined demands of the Hopkins post and the social movements of which she was so vital a part.

Miss Marshall gives as much, if not more, attention to Miss Nutting's role in the International Council of Nurses, the National League of Nursing Education, the American Nurses' Association, the state associations, nurse registration, the recording of nursing history, and the development of nursing periodicals as she does to her accomplishments in the institutions where she was employed. She lists Miss Nutting's membership on so many international, national, state, and local committees that the reader wonders how Miss Nutting survived—and survive she did, like Miss Nightingale, another "delicate" woman who lived to be 91, while Miss Nutting died in her 90th year. Both of these women seem to have inspired in others a faith in their goals and a willingness to help them. This biography is in part a record of the support given Miss Nutting at the Hopkins by her associates and at Teachers College where Isabel M. Stewart, who came there to study, stayed to be Miss Nutting's first assistant, later her successor, and at the last her supporting friend and executor.

The reader can scarcely fail to be interested in the life of this remarkable woman; the story of her family with which she never lost contact; and the accounts of her friends and associates, like Lavinia Dock with whom she wrote the first two volumes of their classic 4-volume history of nursing which, if it were their only accomplishment, would make them candidates for nursing's hall of fame. But most particularly, the reader, especially if he or she is a nurse, is caught up in the development of a profession, to almost every facet of which Miss Nutting contributed directly or indirectly.

The head of the beautiful portrait by Cecelia Beaux, now hanging at the Hopkins, is the frontispiece of this volume. Adelaide Nutting had even in youth an arresting presence which, as captured by Miss Beaux, stays with any one who has seen the portrait.

The United States Public Health Service supported the writing of this biography and "Friends of Adelaide Nutting" facilitated Miss Marshall's work. She mentions many of them but gives special credit to Virginia Dunbar. The history of the John Hopkins School of Nursing by Blanche Pfefferkorn and Ethel Johns is often quoted. The entire book is carefully documented with repeated reference to Miss

Nutting's letters. Because she herself was an historian, a writer of journals and letters, Adelaide Nutting contributed most to the authenticity of Miss Marshall's biography.

If one senses any lack in this book, it is the author's failure to show the human side of her subject—her faults or limitations, whether she had a sense of humor, a sense of fun, or even moments of frivolity. Occasional lapses into a novel form, in which the author describes what she assumes happened and what people said, detract from rather than enliven the narrative. The reader is grateful for the cited passages of Miss Nutting's letters for they sometimes express her deeper feelings, her hopes, her sorrows, and her sadness that the wonderful things she expected around the corner in her youth "never, never happened."

Reviewed by VIRGINIA HENDERSON, R.N, nursing educator and author, Research Associate, Yale University School of Nursing, New Haven, Conn.

J. SAJNER (Editor). *Beiträge zur Geschichte der Medizin und der Naturwissenschaften in Mähren.* Proceedings of a symposium held in Brno, Czechoslovakia, July 3-July 5, 1969. Stuttgart: Hippokrates Verlag, 1971. 64 pp.

VÍTĚZSLAV OREL (Editor-in-Chief). *Proceedings of the Gregor Mendel Colloquium,* June 29-July 3, 1970. *Folia Mendeliana* (Brno: Moravian Museum), vol. 6, 1971. 353 pp.

The proceedings of both conferences, held with international participation, will provide in turn stimulus and substance to scientific cooperation.

The symposium, proceedings of which were edited by Sajner, deals with the history of medicine and the natural sciences in Moravia, a part of today's Czechoslovakia, located between Bohemia to the west and Slovakia to the east. The editor presents a brief overview of significant events and accomplishments, going back to the Middle Ages. Important contributions to the study of prehistoric man were summarized by V. Zapletal.

While Gregor Mendel's (1822-1884) contributions to the experimental study and interpretation of inheritance are not lost, V. Orel considered Mendel's activities in the field of meteorology. Briefly noted is the involvement in 1867/68 of Anton Schrötte von Kristelli (1802-1875), a chemist working in Vienna but born in Olomouc, in what may have been the last experiment in alchemy. Note is taken of the work of the veterinarians, Václav Cech (Czech, 1827-1904), active with Gregor Mendel in the Moravian-Silesian Society for the Advancement of Agriculture, and Florian Koudelka (1862-1921) who wrote not only on veterinary medicine but also on prehistorical osteology and geology.

On the fringes of human medicine, G. J. Kamel (1661-1706), a Jesuit, was an *infirmarius* and *apothecarius* but became famous as a *botanicus* in the Phillipines. The work of the apothecaries of Moravia is treated in some detail, with emphasis on the 18th and the 19th century.

In the present context, of principal interest are the articles on the surgeons of

Olomouc; two homeopathically inclined physicians of Brno, Anton Fischer and Adolf Gerstel; and hydrotherapy, including the activities of the advocate of cold-water therapy, Josef Schindler (1814-1890), the follower and successor of Vincenz Priessnitz (1799-1881).

The editor of the proceedings was hard pressed for space and was forced, by economic considerations, to eliminate illustrations, footnotes, and the lists of references. This is most unfortunate since it makes it difficult to examine in greater depth the literature referred to in the text. There is a table of contents but not a name and subject index, an omission that is particularly regrettable in view of the heterogeneity of the content and a large number of persons to whom reference is being made.

The 1970 colloquium was centered on the work of Gregor Mendel. The largest number of papers are devoted to Mendel's hybridizing experiments and the echoes of Mendel's theory abroad. Separate sessions dealt with various aspects of Mendel's scientific and public activities, Mendel and evolution, and Mendel's personality. Lea Svancarová contributed an interesting psycholinguistic study of the style of Mendel's writings, including his letters.

Historians of genetics everywhere, as well as the hosts whose work is centered in Brno in the *Mendelianum,* will do well to heed the recommendations made by Bentley Glass (p. 341) that research in the next years be directed to three topics:

1. The genesis of Mendel's experimental work and ideas (Mendel's interest in the problem of hybrids and varieties, use of numerical records of the data, application of probabilities and ratios).
2. Evaluation by Mendel of his experimental results (recognition of the generality of his findings when applied to other species, relation of the principles of heredity to the theory of evolution and Darwin's theory of natural selection).
3. The influence of Mendel (the initial delay, the eventual success of the new conceptual model, relation to the chromosome theory of inheritance and the theory of the gene).

The history of the impact of Mendelian theory on the study of inheritance of disease is a logical and needed extension of the last point

Reviewed by Josef Brožek, Ph.D., Research Professor of Psychology, Lehigh University, Bethlehem, Pa.

THE BULLETIN RECENTLY RECEIVED *

Emilio Balaguer Perigüel *La introducción del modelo físico-matemático en la medicina moderna.* Análisis de la obra de G. A. Borelli (1608-1679) *De motu* animalium. Valencia-Granada, 1974 (Cuadernos Hispánicos de Historia de la Medicina y de la Ciencia, XIV, Serie A). 163 pp Ill.

Luis Commenge y Ferrer. *La medicina en el reino de Aragón (siglo XIV).* Introducción por J. Riera. Valladolid, 1974 (Acta Historica-Médica Vallisoletana, Monografías II). 91 pp. Ill.

W. Sterling Edwards and Peter D. Edwards. *Alexis Carrel, Visionary Surgeon.* With a Foreword by Charles A. Lindbergh. Springfield, Ill., Charles C Thomas, 1974. xi+143 pp. Ill. $5.00.

Joseph S. Fruton. *Selected Bibliography and Biographical Data for the History of Biochemistry since 1880.* Philadelphia, 1974 (American Philosophical Society Library, Library Publication No. 6). viii+140 pp $2 50 (paperbound)

Juan Manuel Jimenez Muñoz. *Historia legislativa del cuerpo de médicos forenses.* Valladolid, 1974 (Acta Historica-Medica Vallisoletana, Monografías II). 68 pp.

J. M. López Piñero, M Peset Reig and L Garcia Ballester. *Bibliografía histórica sobra la ciencia y la técnica en España.* Parte 1. Valencia-Granada, 1973 (Cuadernos Hispanicos de Historia de la Medicina y de la Ciencia, XIII, Serie C). 205 pp. Con la coloboración de M. Luz Terrada Ferrandis y J. R. Zaraboza Rubira.

Gordon McLachlan (Editor). *The Way Ahead in Postgraduate Medical Education.* London-New York-Toronto: Published for the Nuffield Provincial Hospital Trust by the Oxford University Press, 1974. x+113 pp $4 00 (paperbound).

Gerrit Jan Meulenbeld. *The Madhavanidana and Its Chief Commentary* Chapters 1-10. Introduction, Translation and Notes Leiden: E. J. Brill, 1974 (Proefschrift, Rijksuniversiteit te Utrecht). xviii+709 pp.

Juan Riera. *La introducción en España del método antiséptico de Lister.* Valladolid, 1973 (Acta Historica-Médica Vallisoletana, Monografías I). 81 pp. Ill.

Gerhard Schmidt. *Das geistige Vermächtnis von Gustav v. Bunge.* Zürich: Juris Druck & Verlag, 1973 (Inaug. -Diss. aus dem Medizinhistorischen Institut der Universität Basel). 88 pp. Ill.

Hans Selye. *Stress without Distress.* Philadelphia & New York J. B. Lippincott, 1974. 171 pp. Ill. $6.95.

Dorothy M. Schullian (Editor). *The Baglivi Correspondence from the Library of Sir William Osler.* Ithaca and London: Cornell University Press, 1974. xxi+531 pp. $45.00.

Arnold Thackray and E. Mendelsohn (Editors). *Science and Values: Patterns of Tradition and Change.* New York: Humanities Press, 1974. viii+251 pp. $11.00.

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Richard L. Blanco

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IMPERIALISM AND MEDICAL EDUCATION IN CHINA *

JOHN Z. BOWERS

The era of foreign imperialism—of cutting up the Chinese melon—began in 1894-95 when Japan's overwhelming victory in the Sino-Japanese War demonstrated China's helplessness. Immediately there was a rush by the foreign powers to obtain economic and political concessions and to establish exclusive areas of influence.

Russia, Germany, and France believed they were entitled to special privileges in China because they had intervened in her behalf in the treaty following the Sino-Japanese War.

The British, at the height of their maritime empire, and as the oldest and most broadly established Western power in China, were equally determined to strengthen their position.

The attitude of the United States was epitomized by its "open door"

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* Presented at the 47th annual meeting of the American Association for the History of Medicine, Charleston, South Carolina, May 2, 1974.

I am grateful to Mrs. Akiko K. Bowers for translating the Japanese sources on the South Manchuria Medical College.

President Michio Okamoto of Kyoto National University made the archives on Manchuria available. Père Joseph Dehergne, S.J , made the sources on Aurora University available. L. Carrington Goodrich was a valuable source of information and corrected drafts of this manuscript.

policy toward China.[1] In September 1899 Secretary of State John Hay proposed that there should be "spheres of influence" to preserve and increase foreign trade with China and the principle of the open door followed in a supplementary note in July 1900. The open door principle was reaffirmed by Great Britain and Germany on 16 October 1900. It was aimed at preserving the integrity of China as a nation.

The era of imperialism that we are concerned with was marked by a number of important benefits for China. Carrington Goodrich has noted some of these:

> Bridges were built, lighthouses erected, and roads laid, new crops and techniques were introduced . . . there developed in the end greater respect for the public health and welfare of the common man than ever obtained in imperial China, countless millions were touched by the selfless devotion of unarmed civil servants, teachers, nurses, physicians, and evangelists who sought no reward.[2]

In this communication we shall describe briefly the development of representative medical schools established by the major imperialist powers. All were of course motivated in part by the wretched state of medicine and public health in China, where only a handful of physicians were trained in Western medicine.[3]

German Medical Education

At the beginning of the twentieth century German medical science was in a position of world leadership. Following the murder of two German missionaries, in 1897 the Germans occupied Tsingtao where they opened a small medical school, which was closed in 1920. A second German medical school opened at Tong-Houn, four kilometers from

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[1] Edward Thomas Williams, *China, Yesterday and Today,* rev. ed. (New York: Thomas Y. Crowell, 1923) and J. K. Fairbank, *The United States and China* (Cambridge, Mass.. Harvard University Press, 1958), pp. 254-7.

[2] L. Carrington Goodrich, "Book Review," *The China Quarterly,* January-March 1970, *41*: 146.

[3] K. Chimin Wong and Wu Lien-Teh, *History of Chinese Medicine* (Tientsin: The Tientsin Press, 1932). This work includes numerous references to the medical schools described in this study. See also for comments on visits to the schools: E. M. Merrins, "Medical education in China," *The Chinese Recorder,* 1917, *48* 497-506; China Medical Commission of the Rockefeller Foundation, *Medicine in China* (China and New York: University of Chicago Press, 1914); Simon Flexner, "Report of the Second China Medical Commission," mimeographed, the Archives of the American Philosophical Society, Philadelphia. E. V. Cowdry, "Anatomy in China," *Anatomical Record,* 1920-21, *20*: 32-60, includes general information, as well as descriptions of programs in anatomy. For a later period, see Professor Knud Faber, Professor of Medicine at Copenhagen University, *Report on Medical Schools in China* (Geneva: League of Nations Organisation Publications, Series II, 30 June 1931, Official No. C.H. 961, No. 8).

Canton. The principal German effort, however, was in Shanghai, China's leading seaport and largest city. Both the Germans and the French had major concessions in Shanghai, where sizeable numbers of their citizens received medical care from expatriate physicians and surgeons.

In 1899, with financial support from two German trading cartels, two German physicians, E. H. Paulun and F. von Schwab, established a hospital for indigent sick Chinese on Burkhill Road, in the Zikawei section of Shanghai adjoining the concessions. The hospital was named for Paulun after his death in 1909.

In 1905, the German consul-general in Shanghai, Dr. J. von Knappe, proposed that a medical school symbolizing Germany's preeminence in medical science should be established in China. Under von Knappe's vigorous and persuasive leadership, the German Physicians' Association of Shanghai, led by Paulun and von Schwab, opened a medical school adjoining the German hospital in 1907 with ten students. The imperialistic significance of the school was described in a German communication: "a model school—which would cast throughout the country the shining light of the glory of German science. . . . The school of medicine, thanks above all to the participation of the German doctors in Shanghai . . . performs a service to Germanism far in excess of the goal of German medical science." [1]

Major financial support was obtained from the Koppel Foundation of Berlin, the Committee for German Civic Authority in China, and the German Asiatic Society of Berlin. The zoological, anatomical, and pathological museums were donated by medical faculties in Germany. The basic science faculty of the new school was drawn from German medical schools; all clinical subjects were taught by practitioners from the German Physicians' Association of Shanghai. Established as the German School of Medicine for Chinese, it was popularly known as *Tung-chi* (literally, harmony) medical school.

The curriculum was patterned strictly on the German system, beginning with four years in a preliminary, or language, school. The first two years were assigned to the *Vorklinicum* for the study of the basic medical sciences. The *Klinicum* was three years in duration and was spent at the Paulun Hospital. Graduates seeking postgraduate studies were transferred to medical faculties in Germany. There were research programs in dysentery, kala-azar, and sprue.

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[1] Quoted by M. Frédet in *Echo de Chine* (Paris, 21, 25, and 27 March 1916), reprinted under the subtitle, "La création d'une école de médecine à Changhai," in "Ce qu'on dit de l'Ecole française de médecine à Changhai," *Rélations de Chine* (Paris, 14 July 1919), p. 481.

The students were exclusively Chinese, with approximately 80 percent selected by the faculty. The remainder were divided between the Chinese government and Red Cross for the assignment of students. All instruction was in German. A unique development in 1912 was the establishment of a technical college for the study of engineering in association with the medical school, financed by Krupp and other major industries in Germany under the sponsorship of Prince Henry of Prussia. The purpose of the school was enunciated in a German communication:

> The Chinese will learn, under German guidance, modern engineering techniques; the knowledge they will have gained in the workshop will give them a basis on which to proclaim the high quality of German material, of German workmanship and the superior intelligence of the German effort.[5]

Thus Germany's major educational effort in China was to train Chinese engineers as well as physicians.

When Simon Flexner visited China as a member of the Second China Medical Commission of the Rockefeller Foundation in the fall of 1915, he ranked the educational program at Tung-chi with that at the Harvard Medical School of China. He described the Harvard program as "perhaps better done than anywhere in China," and in a footnote added, "except the German school at Shanghai."[6]

The medical school and hospital were enlarged in 1910 but seized by the French in 1914 at the outbreak of World War I. Tung-chi later opened under new auspices. It was recognized in 1921 under a Chinese board of directors; in 1923 its status was raised to a university and it came under sponsorship of the provincial government. The basic science departments were transferred to the Woosung section of Shanghai with the Technical College in the same year.[7]

French Medical Education

The major French effort was primarily a result of the mounting bitterness between France and Germany in the years preceding World War I. French nationalism and royalism had experienced a sharp resurgence in 1906 after the Dreyfus case.

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[5] *Ibid.*

[6] Perhaps Flexner was influenced in part by his total devotion to the German philosophy and system of medical education. See Simon Flexner, "Report of the Second China Medical Commission," mimeographed, Papers of Simon Flexner, Archives of the American Philosophical Society, Philadelphia.

[7] Wên-ya Chuang (ed.), *Handbook of Cultural Institutions in China* (Shanghai: Chinese National Committee on Intellectual Cooperation, 1936), p. 10

The outcome of the Sino-French War in 1883-85 established French sovereignty over Cochin China, Cambodia, Annam, and Tonkin. Subsequently, with the acquisition of Laos and Vietnam, French Indochina came into being. In 1890 the Pasteur Institute was founded in Saigon as a branch of the Paris Institute and under its general direction. It has been described as the first laboratory of microbiology in the Far East.[8] Pasteur Institutes were subsequently founded in Hanoi and in Annam, and microbiological centers at Hue, Pnompenh, and Vientiane. The primary purpose of the programs was to prevent infectious disease and to study all aspects of tropical pathology.

In 1900 the Hôpital Doumer was opened in Canton, in honor of Paul Doumer, governor-general (1897-1902) and the architect of France's development of Indochina. Three years later, in 1903, with financial support from the governor-general of Indochina, a program was established to train young Chinese men as *infirmiers*. On March 19, 1902, Aurora University was opened in Shanghai by Siang-Pe Ma (also known as Hsiang-fo Ma), a Chinese Catholic literateur and political leader; the institution was turned over to the Jesuits shortly thereafter.[9]

St. Mary's Hospital, operated by the Sisters of Charity, St. Vincent de Paul, was opened in 1907 in a lovely park in the heart of the French concession in Shanghai. Subsequently a Pavillion du Sacre Coeur for maternity cases and children and St. Vincent's Pavillion for indigent men were added.

It was not until 1913, however, that serious consideration was given to a program in medical education at Aurora University. In that year Professor Agrégé Eugène Vincent, of the faculty of medicine at Lyon, came to China to make recommendations on medical education. In his report Vincent expressed the sentiments of a French patriot toward the German effort:

> The Germans vie with tenacity, many marks and soldiers to conquer a very important place in China; they follow us step by step to profit from our long labours in all branches of the [Chinese] civilization.[10]

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[8] Lieow Yong-Chuen, "Les Instituts Pasteur d'Indochine," *Université L'Aurore Bulletin Médical,* August 1934, *10*: 146-71.

[9] "Université L'Aurore Shanghai," *Renseignements généraux et organisationales études* (Shanghai, 1959). The archives of Aurora University are deposited at Les Fontaines, Chantilly, France.

[10] Eugène Vincent, Professor agrégé on the faculty of medicine at Lyon, *L'influence française en Chine et les entreprises allemandes—necessité de créer en Chine une Faculté Française de Médecine* (Lyon: A. Rey, Imprimateur de l'Académie, 1914), p. 9.

In another report Vincent compared the aggressive programs of the other Western powers with those of France:

> At the present time, the Americans, British, and Germans are creating in the great centers of China, medical schools and hospitals which are also the foyers of propaganda, intended to assure the moral supremacy of their country. Money is readily available for these institutions, while for us every *sou* is given with the greatest parsimony.[11]

Although a start had been made in Canton, Vincent recommended that developments in medical education should be centered in Shanghai:

> And we must make it very clear that it is at Shanghai that French influence faces the greatest challenge from the Germans.[12] He compared the need to counterbalance British and American programs in China with the challenge that had been posed to France by Anglo-American educational developments in the Levant. St. Joseph's University, and its faculty of medicine, had been founded in Beirut by French Jesuits in response to the establishment of the medical program of the Syrian Presbyterian College—now the American University of Beirut.[13]

Vincent's recommendation for a medical faculty at Aurora was enthusiastically adopted; as a sign of the national significance of the event and of governmental endorsement the Chamber of Deputies in Paris promptly appropriated 25,000 *francs* in support of the venture.

The French press voiced strong support; writing in *Débats*, 27 February 1914, M. M. Courant stated:

> A French faculty of medicine has an equally important role to play on the Yangtze as it does in the Near East: all is ready and waiting. Shall today's French citizens be any less intelligent and enterprising than those of the previous generation?[14]

The first students were admitted to the Aurora Faculty of Medicine in 1914. The emphasis was on training Chinese, and 90 percent of the students were of native origin. All instruction was in French. The philosophy and programs were typically French, with two years premedical and four years medical education. From his first day in medical school, the student spent every morning at St. Mary's Hospital.

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[11] Eugène Vincent, *La médecine en Chine au XX^e^ siècle* (Paris, G. Steinheil, 1915), p. 14.

[12] Vincent, *L'influence française en Chine*, p. 9.

[13] American missionaries, led by Philip Bayard Dodge, announced plans for the Syrian Presbyterian College at Beirut in 1862, and premedical and medical subjects were added in 1867. The college became the American University of Beirut in 1920 (P. Bayard Dodge, *The American University of Beirut* [Beirut, 1958]). The French Jesuits, at the urging of French statesman Leon Gambetta, opened St. Joseph's University at Beirut in 1875. The faculty of medicine was opened in 1883.

[14] Maurice Courant, *Débats* (Paris, 27 February 1917), reprinted under the subtitle, "Une faculté française de médecine à Changhai," in "Ce qu'on dit de l'école française de médecine pour Changhai," *Rélations de Chine* (Paris, 14 July 1916), p. 476.

American Medical Education

One of the finest expressions of international good will by the United States was its decision to remit the indemnity owed by the Peking government, as a result of the Boxer Rebellion, in order to establish a program of education and culture in China. This step had a major impact on every field of learning, including medicine.

The original indemnity amounted to US $53,348,000, and the proposal to remit it came from Dr. Arthur H. Smith, a Congregational missionary of the American Board of Commissioners for Foreign Missions in China, with President Theodore Roosevelt in a meeting on 6 March 1906. Smith was introduced to Roosevelt by the Reverend Doctor Lyman Abbott, also a Congregational clergyman and editor of the magazine *The Outlook*.[15] In 1908, on the recommendation of Secretary of State Elihu Root, the Congress authorized the president to modify the bond to $13,655,192 and to remit the balance in favor of China.

The funds were used for scholarships awarded to Chinese youths to study in colleges in the United States. Selection was through a national competition, with preliminary examinations at the provincial level and the final examinations in Peking. The first Boxer Indemnity Scholarships were awarded in 1909 and the following year seventy Chinese students went to the United States. In the first eighteen years of the program, 1,300 men and women were given scholarships. The Chinese government made further use of the indemnity money by founding Tsing Hua College in Peking in 1911 as a junior college to prepare students for study in the United States on Boxer Scholarships.[16]

In 1917, Henry Cabot Lodge, chairman of the Senate Committee on Foreign Relations, recalled his role in the original remission and proposed that the remaining debt be cancelled:

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[15] "Theodore Roosevelt and Arthur Smith," *The Missionary Herald*, 1925, *121* (1): 3-5; Laurence F. Abbott, *Impressions of Theodore Roosevelt* (Garden City, N.Y.: Doubleday, Page, 1919), pp. 143-6; Philip C. Jessup. *Elihu Root*, 2 vols. (New York: Dodd, Mead, 1938), vol. 1, pp. 387-8.

[16] In 1925 a policy of development to a university status was adopted for Tsing Hua and the name was changed to Tsing Hua University. Later the school was designated as National Tsing Hua University. [Tsing Hua or Ch'ing Hua, the name of a garden adjoining the old summer palace, the site of Tsing Hua University, which was destroyed by the British and French in 1860. From Shui mu ch'ing hua. the water is clear and the trees are beautiful.]

> I introduced a resolution to return a portion of the indemnity to China. It amounted, I think, altogether, to eleven million dollars. This was passed, and the money returned, and I do not think we did anything that caused a better feeling toward us in China. It is still remembered. When the war came we arranged for a suspension of further payments from China to us on the indemnity account (see telegram of 5 September 1917) . . . Now what I would suggest is this . . . to put an end to any further payments from China . . . there is a very friendly feeling toward China . . and sympathy with China in the Shantung business.[17]

In May 24 the United States remitted the remainder of the indemnity funds to be used for further educational and cultural work in China.

The Boxer Indemnity program produced a group of young men and women who became leaders in Chinese scientific, cultural, and educational fields. One of the more distinguished medical scientists from the Tsing Hua program was Chen Ko-kuei, who, working with Carl F. Schmidt in the laboratories of the Peking Union Medical College (PUMC) identified ephedrine as the active principle in an indigenous medication, Ma Huang.[18] Another was Hsien Wu, who with Otto Folin at Harvard, developed the Folin-Wu method for preparing a protein-free filtrate of the blood.[19]

British and North American Missionary Medical Education

The educational programs of the United States were under a variety of auspices. Sponsorship by American universities included Yale-in-China, Harvard Medical School of China, and the University of Pennsylvania at Canton and later Shanghai. Peking Union Medical College (PUMC) was financed and sponsored by the China Medical Board (CMB) of the Rockefeller Foundation. These institutions have been described extensively elsewhere.[20] Our principal interest is in the

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[17] *Foreign Relations of the United States, 1921,* vol. 1, National Archives, pp. 398-9. The failure at Versailles to return the province of Shantung, which had been held by Germany, to China caused a violent reaction in China with attacks on the foreign legations. Shantung remained in Japanese hands until 1922 when Japan agreed at the Washington Conference, 4 February 1922, to return the province and the railways to China The Japanese had seized Shantung from the Germans at the outbreak of World War I.

[18] K. K. Chen and Carl Schmidt, "The action of ephedrine, the active principle of the Chinese drug Ma Huang," *Journal of Pharmacology and Experimental Therapeutics,* 1924-25, *25*: 339-57.

[19] Hsien Wu, *A System of Blood Analysis, with Special References to Uric Acid* (Ph.D. diss., Harvard University, 1919).

[20] For a discussion of Yale-in-China see Edward H. Hume, *Doctors East, Doctors West: An American Physician's Life in China* (New York: W. W. Norton, 1946); William Reeves, Jr., "Sino-American Cooperation in Medicine: The Origins of Hsiang-Ya (1902-1914)" in *American Missionaries in China,* papers from Harvard seminars, Kwang-Ching, ed. (Cam-

British and North American missionary medical schools because of their unique character and their contributions to medicine in China.[21]

Before the Boxer Rebellion, the work of British, United States, and Canadian missionaries was carried out in small, poorly-housed, understaffed, and inadequately financed medical schools. After the rebellion emphasis was placed on the development of "union" schools, whereby the smaller institutions were merged and received multiple sources of missionary support.

The leading union missionary school was the School of Medicine of Shantung Christian University, designated Cheeloo after 1915,[22] in Tsinan.[23] It had developed in 1909 as Union Medical College by an amalgamation of two small programs: one at Tsinan operated by the American Presbyterian Mission, and the other at Tsingchowfu, under the English Baptist Mission with classes of "peripatetic" students rotating between the two institutions. The CMB made a major grant of $150,000 (Chinese) to the Shantung School of Medicine in 1915, at the time of the Transfer to Tsinan of the three lower classes from the Union

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bridge, Mass.: East Asia Research Center, 1966), pp. 129-183. For Harvard Medical School of China see Harvard Medical School of China, Inc., *Annual Reports of the Executive Committee* (Cambridge, Mass., 1902-1914); Jean A. Curran, "The Harvard Medical School of China," *Harvard Medical Alumni Bulletin,* 1963, *38* (2): 12-19. For University of Pennsylvania see W. W. Cadbury and M. H Jones, *At the Point of a Lancet: One Hundred Years of the Canton Hospital, 1835-1935* (Shanghai: Kelly and Walsh, 1935); Mary Lambertson, *St. John's University, Shanghai, 1879-1951* (New York: United Board of Christian Colleges in China, 1955). For Peking Union Medical College see Mary E. Ferguson, *China Medical Board and Peking Union Medical College: A Chronicle of Fruitful Collaboration: 1914-1951* (New York: China Medical Board of New York, 1970); John Z. Bowers, *Western Medicine in a Chinese Palace: Peking Union Medical College, 1917-1951* (New York: The Josiah Macy, Jr. Foundation, 1972).

[21] For a general survey of the missionary medical schools see Harold Balme, *China and Modern Medicine: A Study in Medical Missionary Development* (London: United Council for Missionary Education, 1921).

[22] The ancient China of Confucius and Mencius was considered to be divided into two kingdoms: Chee and Loo.

[23] Personal communication, E. B. Struthers (May 10, 1974). See also: R. T. Shields, "The origin and development of the School of Medicine of Shantung Christian University," *China Medical Journal,* 1926, *40*: 759-62; J. G. Lutz, *China and the Christian Colleges, 1850-1950* (Ithaca and London: Cornell University Press, 1971), pp. 150-1; K. S. Latourette, *A History of Christian Missions in China* (London: London Society for Promoting Christian Knowledge, 1929); John L. Stuart, *Fifty Years in China: The Memoirs of John Leighton Stuart, Missionary and Ambassador* (New York. Random House, 1954), p. 43; Charles H. Corbett, *Shantung Christian University (Cheeloo)* (New York: United Board for Christian Higher Education in Asia, 1955), William P. Chalfant, "The opening of the Union Medical College in Tsinan," *The China Medical Journal,* 1911, *25*: 316.

Medical College in Peking, which the CMB had acquired and which was to become PUMC.

As the drive to bring the splintered medical schools into a small group of union institutions grew, other institutions were merged with Cheeloo: the Union Medical College of Nanking (1917); the Medical School in Hankow (1919); and the North China Union Medical College for Women in Peking (1924). By 1926 a total of ten United States, British, and Canadian missionary societies were cooperating in Cheeloo and it was described by Shields as follows: "From the international, denominational, and coeducational standpoint this is the largest union enterprise in China, if not in the world." [24]

From the beginning, the language of instruction in most of the missionary schools was Chinese, first because it was the language of the people, and because of the missionaries' historical contributions in translating medical texts into Chinese.[25] As early as 1851, Benjamin Hobson, M.B., B.S., an English missionary, had prepared a Chinese text on anatomy and physiology. Other missionaries continued and expanded the practice. John Glasgow Kerr, M.D., L.L.D., for forty years an American missionary at Canton, published over fifteen texts and monographs.[26] With the growth of the union medical colleges, Cheeloo, led by Randolph T. Shields, an anatomist, became the center for translations into Mandarin of such works as Gray's *Anatomy* and Osler's *Principles and Practices of Medicine.*

Although Mandarin was the language of instruction at Cheeloo, students were required to learn English and urged to read English texts and medical literature. Indeed, the students frequently urged their teachers to lecture in English.[27] A teacher might spend eight to ten hours preparing a lecture in Mandarin, and then be distressed to find the students taking notes in English.

Probably the first free school health program in a missionary school was established in 1936 at Tung Chia Chang by Professor E. B. Struthers, from Cheeloo. With groups of twenty-five medical students, Struthers conducted periodic physical examinations and immunization; opened a club for mothers; and instructed peasants in protecting their heavily contaminated wells.

The missionaries usually spent their first year in China studying at language schools in Peking, Nanking, or Canton. The missionary doctors were so eager to get on with their medical work, however, that many of them gave only half-hearted attention to their language studies.[28] However, there were faculty members at the leading union medical colleges who were able to teach medicine effectively in Chinese.

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[24] R. Shields, in *The China Medical Journal,* 1926, *40*: 760.

[25] The exception was at St. John's University in Shanghai.

[26] Cadbury and Jones, *At the Point of a Lancet.*

[27] Personal communication, Gordon King (April 1972).

Admission requirements at the missionary schools included graduation from a six-year middle school followed by two years of premedical studies. All the missionary schools operated their own premedical department; the medical course was five years in duration, with the fifth year as an internship.

Cheeloo played the leading role in producing graduates to staff the North American and British missionary hospitals.[29]

Significant parasitology studies were carried out at the missionary hospitals. The most important work was done by Claude H. Barlow in Shaohsing, Chekiang Province, on Fasciolopsis. As one phase of his studies Barlow ingested ova and adult Fasciolopsis buski to determine transit time from the moment eggs were swallowed until they appeared in the stool.[30] He was also interested in learning the time required for a patient to become ova-free. Barlow subsequently published his definitive monograph on the life cycle of Fasciolopsis based in part on these studies.[31]

In Egypt in 1944 Barlow renewed his self-experimentation by infecting himself with Schistosoma haematobium in order to bring the parasite to the United States to determine if any species of native snail could act as the intermediate host.[32]

Japanese Medical Education

Japan was represented in China by a unique and excellent institution, the South Manchuria Medical College in Mukden.[33] It was sponsored by the South Manchuria Railway Company, and while jointly owned by private capital and the Japanese government, absolute control rested in the hands of the government.[34]

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[28] Personal communication, L C. Goodrich (April 1974).

[29] Personal communication, Jean A. Curran (April 29, 1974).

[30] C. H. Barlow, "Experimental ingestion of ova of Fasciolopsis buski; also the ingestion of adult Fasciolopsis buski for the purpose of artificial infestation," *Journal of Parasitology,* 1921-2, *8*: 40-44.

[31] C. H. Barlow, *The Life Cycle of the Human Intestinal Fluke, Fasciolopsis Buski (Lankester)*, American Journal of Hygiene, Monographic Series, No. 4. (Baltimore: Johns Hopkins Press, 1925).

[32] C. H. Barlow and H E. Meleney, "A voluntary infection with Schistosoma haematobium," *American Journal of Tropical Medicine,* 1949, *29*: 79-87.

[33] The major archives are in the historical section of the medical library of Kyoto University, Kyoto, Japan.

Construction of the 650-mile-long South Manchuria Railway (SMR) was begun by Russia in 1898 to connect her naval bases at Dairen and Port Arthur with the Chinese Eastern Railway. It was an enterprise of the Russo-Chinese Bank under a secret treaty between Russia and China, signed in 1896. The two railways, together with the Trans-Siberian line, were part of Russia's grand design, started under Tsar Alexander III in 1866, to establish land power in the Far East. The special prize for Russia was Manchuria, a virgin land of vast untapped natural resources such as timber and minerals, and the largest seams of bituminous coal in the world.[35]

Japan also had her designs on Manchuria, not only because of the resources but because of its value as a fortress against China to the south and Russia to the north. France and Germany encouraged Russia, while Britain was officially allied with Japan. While the other powers were preoccupied with the Boxer Rebellion, Russia occupied most of Manchuria in 1900. Refusing to withdraw her troops, she moved toward the occupation of Korea, a country over which Japan considered herself to have proprietary rights. Hostility between the two nations mounted, culminating in the outbreak of the Russo-Japanese War when Japan attacked Port Arthur on 8 February 1904. Japan emerged victorious and the Portsmouth Treaty of 5 September 1905 restored Manchuria to China. The treaty ceded the SMR and leaseholds on the Liaotung Peninsula and the Kwantung Leased Territory.

On 7 June 1907 a Japanese Imperial Ordinance established the South Manchuria Railway Company (*Minami Manshū Tetsudō Kabushiki Gaisha*), generally known as *Mantetsu,* with headquarters in Dairen.

As with other major Japanese enterprises, the railway maintained hospitals and dispensaries for its employees and their dependents. They were located in the seventeen major communities along the right-of-way with the largest at Dairen.

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[34] *Mantetsu Sanjūnen Shi* [Thirty years of the South Manchuria Railway Company] (Dairen, 1937). E. H Harriman, President of the Union Pacific Railroad, tried unsuccessfully to purchase the railway from the Japanese government in the fall of 1905. It was part of his plan for a round-the-world transportation line by way of the Pacific, Japan, Manchuria, Siberia, European Russia, and the Atlantic Ocean. See George Kennan, *E. H. Harriman, A Biography,* 2 vols. (Boston and New York: Houghton Mifflin, 1922), vol. 2, p. 2.

[35] Thomas S. Logan, Inc. (comp.), *Manchuria, Land of Opportunities* (New York: Thomas S. Logan, Inc., 1922); Owen Lattimore, *Manchuria, Cradle of Conflict* (New York: Macmillan, 1932); Richard Story, *A History of Modern Japan* (Middlesex, Eng.: Penguin Books, 1960); Hugh Borton, *Japan's Modern Century* (New York: Ronald Press, 1955); and Chitoshi Yanaga, *Japan since Perry* (New York: McGraw Hill, 1949).

The railway also instituted a major program in public health with a hygiene institute in Dairen, isolation hospitals in Dairen and Port Arthur, the principal seaports, and public health stations in the principal towns along the railway.

Public health was a keystone of Japanese colonial policy in Manchuria:

> It may be taken for granted that the public health system of any country reflects the stage of progress of that country . . . the enlightenment of a country stands proportionate to how near perfection its public hygiene system is. The genius of a correct colonizing policy lays down the public hygiene enterprise as its herald or forerunner.[36]

Massive public health problems existed in Manchuria—tuberculosis, cholera, smallpox, typhus fever, and pneumonic plague; 60,000 died from the epidemic of pneumonic plague in 1910-11; and 15 percent of the patients in general hospitals and 30 percent of the troops had active syphilis.[37]

Japanese apprehensions about the health of the railway's employees became manifest at the time of the pneumonic plague epidemic. The government threatened publicly to take over public health control in Manchuria unless China immediately established an effective plague-prevention program.

The man responsible for the emphasis on public health as a keystone of the railway's program was its first president, a physician, Gotō Shimpei (1857-1929).[38] He had previously served as chief civil administrator in Taiwan, where he established an enlightened and comprehensive public health program. The program Gotō established in Manchuria was based on "his belief that the development of Manchuria should rest on the improvement of public health and hygiene."[39] He was also responsible for the establishment of a diversified research department as a major resource for the railway's colonial development scheme.[40]

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[36] M. Tsurumi, "Public hygiene in Manchuria and Mongolia," introduction, *The Light of Manchuria* (1 February 1921), p. 1.

[37] Carl F. Nathan, "Plague prevention and politics in Manchuria, 1910-1931," (Cambridge, Mass.: Harvard East Monographs, 1967); Wu Lien-teh, *Plague Fighter: The Autobiography of a Modern Chinese Physician* (Cambridge, Eng. W. Heffer, 1959); and W. H. Jefferys and J. L. Maxwell, *The Diseases of China* (London: John Bale Sons and Danielson, 1910).

[38] Shinobu Seizaburō, *Kaga Kuteki Seijika no Shigai* (Tokyo: Hakubun-kan, 1941); "Gotō Mantetsu Sōsai shusoku jōyūsho, Gotō Hakukokyo tsūita to shitai" (Dairen: Mantetsu Shain Kai, 1929); and Charles A. Beard, "Japan's statesman of research," *American Review of Reviews,* September 1924.

[39] Shinobu Seizaburō, *Gotō Shimpei* (Tokyo: Hokai, 1941), p. 6.

The railway's establishment of a medical college in Manchuria was seen as one aspect of the goal of improving public health. There was at that time only one Western medical institution of consequence in Manchuria—the Mukden Medical College led by the famed Scottish Presbyterian missionary, Dugald Christie (1855-1937).[41]

The railway's request for approval of a new medical school was endorsed in Tokyo. The proposal called for the establishment of a *semmon-gakko*—a second-level medical school of a type found throughout Japan—that would become a university-level institution at some subsequent date.[42] The South Manchuria Medical College, always known as *Nanman igaku-do,* was officially opened on 15 June 1911.[43]

The medical schools in Japan were divided into two feudatories: the medical schools in northeast Japan were controlled by Tokyo University, while the faculties of the medical schools in southwest Japan and Nanman Igakudo were appointed by Kyoto Imperial University. This relationship with Kyoto was especially beneficial because of the deep interest that Torasaburo Araki, the president of the university from 1915 to 1929, and a physician, had in the college.

As a *semmon-gakko,* the college had a four-year curriculum to which students were admitted directly from middle school, without any period of premedical education. A special "preparatory" course at the South Manchuria Middle School prepared Chinese for the study of medicine at the college, and the first Chinese graduates of the course began the study of medicine in 1913.

The faculty was exclusively Japanese, except for two Germans who taught that language in all four years of the medical curriculum. All instruction was in Japanese with, as in all other Japanese schools, a liberal sprinkling of German scientific and technical terms.

The South Manchuria Medical College was elevated to the level of a university faculty of medicine in 1922 and became *Manshu Ikadaigaku,* with a two-year preparatory school for all students.

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[40] John Young, *The Research Activities of the South Manchurian Railway Company,* 1907-1945 (New York: The East Asia Institute, Columbia University, 1966).

[41] Dugald Christie, *Thirty Years in Moukden, 1883-1913: Being the Experiences and Recollections of Dugald Christie, C.M.G.,* Edited by his wife (London: Constable, 1914)

[42] The *semmon-gakko* approach was also followed by the Japanese when they acquired colleges in Taiwan and Korea. The schools were later raised to imperial universities. See Y. Kusama, "Medical education in Japan," *Journal of Medical Education,* 1956, *31*: 393-8.

[43] "South Manchuria Medical College," in *The Light of Manchuria* (Mukden: Manchurian Enlightening Society, 1 November 1921), pp. 42-50.

The college moved rapidly to become one of the leading medical research centers in the Japanese Empire, approached only by the imperial universities in Tokyo and Kyoto. Its most distinguished scientist, Yas Kuno, a physiologist, became Japan's leading medical scientist through his monumental studies on the physiology of sweat production and control.[44] He was later to say that these studies could only have been accomplished at Mukden because of the excellent facilities, special equipment, and technical support made available to him. In his words, "The railway spared no expenses on the medical college." [45]

In line with the railway's declared policy to conduct research on broad health problems in Manchuria and Mongolia, faculty-student expeditions carried out public health field studies in Inner Mongolia, as well as in Manchuria. The report of the first expedition in 1923 describes how the team developed a simple distillation apparatus to prepare safe drinking water, commented on the prevalence of trachoma and on the exceedingly high incidence of venereal disease among the population, especially the lama priests.[46]

S. Kubata and T. Okanishi of the Department of Pharmacology collected, classified, and described the pharmaceutical properties of over 1700 specimens from plant, vegetable, fish, and animal sources in China, Korea, Japan, and India.[47] These studies, and those of Bernard Read, a pharmacologist on the faculty of PUMC, were the only significant efforts by foreign medical schools in China to explore the vast and historic Chinese materia medica, the exclusive source of therapeusis for hundreds of millions of Chinese at home and abroad.[48]

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[44] Professor Yas Kuno, "Ueber die Schweisskreiten des Menschen," *Transactions of the Fourth Congress, Far Eastern Association of Tropical Medicine* (Weltevreden, Java: Javasche Boekhandel en Drukkerij, 1921), vol. 2, pp. 260-70; Kuno, "Ein Apparat für die Bestimmung der Schweissekretin," *Fourth Congress,* vol 2, pp. 271-3; and "Manchu Ikadaigaku Honjinkai," *Hojin,* 1 November 1964, 7 (special number to honor Professor Kuno).

[45] Personal communication, Kyoto, Japan (1962).

[46] *Manshu Ikadaigaku Daukai Tomo Junkai Shinryo Hokoku,* Report of the First East Mongolian Travelling Clinical Survey (Mantetsu Etseika, 1923)

[47] S. Kubota and T. Okanishi, *Folia Pharmacorum Sinicorum: List of Collection of Chinese Materia Medica* (Mukden, 1931).

[48] Bernard E. Read and Liu Ju-Chiang, *Chinese Medicinal Plants from the Pen Ts'ao Kang Mu A.D. 1596: A Botanical, Chemical, and Pharmacological Reference List* (Peking: Peking National Historical Bulletin, 1936) For Read's full bibliography on the Chinese materia medica, see Joseph Needham, *Science and Civilisation in China,* vol. 1 (Cambridge, Eng.: Cambridge University Press, 1961), pp. 290-1

A less praiseworthy Japanese endeavor entailed sending Chinese students to Japan to study medicine in the first decade of the twentieth century. Thirteen thousand students went to Japan between 1902 and 1910.[49] Unfortunately, they were trained in the *semmon-gakko,* since the entrance examinations of the eight numbered higher schools leading to the study of medicine were not only too difficult but were given exclusively in Japanese. When the graduates returned to China they taught at medical schools that replicated the inferior Japanese institutions. The graduates also practiced a poor quality of medicine and, unfortunately, staffed all of the governmental and Red Cross hospitals, including army and navy medical facilities.

Summary and Conclusion

Russia was the only major imperialist power that failed to establish a program in medical education in China, because its domestic resources were so slight, and because her status in China began to decline after the Russo-Japanese War.

The foreign medical schools studiously ignored and/or denounced Chinese traditional medicine, although it was followed to some degree by essentially every citizen of China. Only the Japanese school in Manchuria and PUMC compiled and explored the enormous historical and highly popular Chinese materia medica.

Despite the massive and intriguing public health problems in China, the only academic programs of significance were conducted by the Japanese, Cheeloo, and PUMC. With limited financial and academic resources, it was logical for the schools to concentrate on the rudiments of medicine—anatomy, physiology, pathology, medicine, and surgery.

In contrast to the French, German, and Japanese programs, the British and North American schools were established solely for the benefit of the Chinese students, and they were the only schools where the language of instruction was Chinese. The missionaries made major contributions in compiling and translating medical texts into Chinese. The Germans, French, and Japanese viewed the use of their own languages as an inherent element of their imperialist designs.

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[49] Ling Chi Hong in *Educational Review,* April 1910, p. 8.

FROM 48 TO 46: CYTOLOGICAL TECHNIQUE, PRECONCEPTION, AND THE COUNTING OF HUMAN CHROMOSOMES

MALCOLM JAY KOTTLER

Introduction

Advances in cytology have commonly been the direct result of improvements in cytological technique. The emergence of human cytogenetics as a new discipline in the late 1950s was totally dependent upon a technical revolution in mammalian cytology completed in the early 1950s. The first success of the new cytology was the discovery of the actual number of chromosomes in human cells. In this paper I shall discuss the origin of that discovery and the history of human chromosome counting, an episode in the history of cytology throughout which cytological technique played a decisive role.[1]

Late in December 1955 the cytologist J. H. Tjio, working in the laboratory of A. Levan, counted just 46 chromosomes in human embryonic lung cells. These counts were entirely unexpected. But after a total of 261 concordant counts, Tjio and Levan accepted and published their results. The counts came as a complete surprise to all mammalian cytologists who, for twenty years, had been virtually certain diploid human cells possessed 48 chromosomes. Their certainty was derived from chromosome counts made during the 1920s and 1930s by a number of esteemed cytologists all over the world. Furthermore, counts undertaken as recently as the early 1950s had continued to confirm the prevailing belief in 48 chromosomes. Thus in 1954, just two years before Tjio and Levan's bombshell, the cytologist L. Sachs had declared, "the diploid chromosome number of 48 in man can . . . now be considered as an established fact." [2]

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[1] Several cytologists have written brief but insightful histories of human chromosome counting: R. Matthey, "Chromosomes et intersexualité chez l'homme," *Gynaecologia,* 1963, *155*: 288-298; J German, "Studying human chromosomes today," *Am. Scientist,* 1970, *58*: 182-201; C. Stern, "The chromosomes of man," *Am. J. Human Genetics,* 1959, *11*: 301-304; J. Hamerton, *Human Cytogenetics* (New York: Academic Press, 1971), I, 1-13; C. Ford and J. Hamerton, "The chromosomes of man," *Nature,* 1956, *178*: 1020-1023; E. Chu, "The chromosome complements of human somatic cells," *Am. J. Human Genetics,* 1960, *12*: 97-100; G. Wendt and B. Wolf, "Die Chromosomenzahl beim Menschen," *Deutsche med. Wchnschr.,* 1957, *82*: 1832-1836; R. Turpin and J. Lejeune, *Human Afflictions and Chromosomal Aberrations* (Oxford: Pergamon, 1969), 7-10. Three invaluable reviews prior to 1956 are: G. Heberer, "Die Ergebnisse der Chromosomenforschung beim Menschen," *Ztschr. f. menschliche Vererbungs- und Konstitutionslehre,* 1935, *19*: 96-129; R. Matthey, *Les Chromosomes des vertébrés* (Lausanne: F. Rouge, 1949), 19-34, 65-68, 74-75, 106-119, 147-152; R. Beatty, "How many chromosomes in mammalian somatic cells?" *Internat. Rev. Cytology,* 1954, *3*: 177-186. Since the completion of my paper, a full-length treatment of the history of human chromosome counting has been published: J.-D. Murken and H. von Wilmowsky, *Die Chromosomen des Menschen* (München: W. Fritsch, 1973).

In view of the long held and repeatedly confirmed conviction that human cells possessed 48 chromosomes, a period of controversy and conflicting chromosome counts following the discovery of Tjio and Levan might reasonably have been expected. But instead of controversy, the Tjio and Levan counts were rapidly confirmed. Within a few months, C. Ford and J. Hamerton, who only a year before (1955) had rejected an offer of human material for chromosome counts because "everyone [knew] that the diploid number [was] 48 and there [was] probably very little more we [could] learn at present," confirmed, in spermatogonia, Tjio and Levan's somatic cell counts and also counted the haploid number of 23 bivalents in primary spermatocytes.[3] During the next three years, many cytologists counted 46 chromosomes in literally thousands of cells from many tissues obtained from many individuals. By 1960, there was no longer any doubt that the diploid number of chromosomes in normal cells was 46.

The extreme ease and rapidity with which Tjio and Levan's discovery was confirmed makes one wonder how so many cytologists could have repeatedly counted 48 chromosomes in diploid cells and 24 chromosomes in haploid cells *prior to 1956* and why, all of a sudden, nearly all cytologists counted 46 and 23 chromosomes *after 1956.* This puzzle is readily solved once one realizes that "cytological technique is coextensive with cytology." [4] Counting might seem to be the most straightforward form of observation. But in the 1920s and 1930s, when the first counts of 48 chromosomes in human cells were made, cytologists still relied on the traditional section technique, developed in the late 19th century. With such sections, counting human chromosomes was extremely difficult and problematic. In contrast, in 1956 and after, with unsectioned material prepared with the new cytological techniques, chromosome counting suddenly became routine and unequivocal. Under such circumstances, the immediate discovery of the actual number of chromosomes in human cells was inevitable.

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[2] J. H. Tjio, personal communication. A. Levan, personal communication L. Sachs, "Sex-linkage and the sex chromosomes in man," *Ann. Eugenics,* 1953-1954, *18*: 256.

[3] C. Ford, "Human cytogenetics: its present place and future possibilities," *Am. J. Human Genetics,* 1960, *12*· 104.

[4] J. Baker, *Cytological Technique* (London: Methuen, 1950), 1.

The First Human Chromosome Counts, 1890-1920

Human chromosomes were first observed in the 1870s when, due to the incorporation of staining and fixation into standard cytological procedure, chromosomes and the phases of mitosis in many species were brought to light. Human chromosomes were first counted in the 1890s after the recent emergence of the chromosome theory of heredity which created an intense interest in the number of chromosomes in cells. In the 1870s and early 1880s, cytologists concentrated on the behavior of chromosomes and evinced little interest in their number. The few chromosome counts attempted in the first half of the 1880s suggested that the number of chromosomes might be constant within a species, though different among species. But no one attached any significance to this apparent numerical constancy. In the second half of the 1880s, however, with the enunciation of the chromosome theory of heredity, chromosome number constancy assumed a much greater importance. Advocates of the "individuality" of chromosomes, like T. Boveri and C. Rabl, argued that the number of chromosomes in cells was constant because the chromosomes were persistent cell structures. They opposed the prevailing notion that the chromosomes were merely transitory structures which disaggregated into chromatic granules in the resting cell nucleus and re-formed anew from those granules at the beginning of each cell division. The support lent by the chromosome number constancy "law" to the embattled chromosome theory of heredity unquestionably stimulated cytologists to count the number of chromosomes in many species, including man.

From the 1890s through about 1920 human chromosome counts varied widely, from 8 to over 50, but the majority clustered around 24 in diploid cells and 12 in haploid cells.[5] Consequently most cytologists firmly believed human cells possessed 24 chromosomes and this number was incorporated into standard textbooks where, in some cases, it remained as late as 1930. It is essential to realize that low chromosome counts were characteristic for *all* mammalian species studied during this period.[6] Cytologists were unaware of the necessity for the *immediate* fixation of mammalian tissue removed from a *living* body to guarantee the preservation of chromosomes in a lifelike condition. Thus they were perfectly satisfied with material obtained from corpses, usually those of executed criminals. Unfortunately, in dead mammalian cells, chromosomes clump together extraordinarily rapidly.[7] In a sense, then, the low counts were not entirely inaccurate; however, the approximately 24 clumps counted were not single chromosomes. If one wishes to assess human chromosome counts prior to 1920, the "failure" of cytologists was not so much the incorrectness of their counts as their inability to appreciate the conditions for the lifelike preservation of chromosomes and the resulting disparity between chromosomes in their material and in living cells.

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[5] See the following tables for the counts of W. Flemming, J. Duesberg, M. Guyer, A. Branca, T. Montgomery, S. Gutherz, H. Jordan, H. Wieman, O. Grosser, and S. Schachow: K. Oguma and S. Makino, "A revised check-list of the chromosome number in vertebrata," *J. Genetics*, 1932, *26*: 253-254; S. Makino, *An Atlas of the Chromosome Numbers in Animals* (Ames: Iowa State College, 1951), 286-290.

The work of Hans de Winiwarter[8] was a notable exception during the first period of human chromosome counting. Winiwarter counted chromosomes in several mammalian species, including man, between 1900 and 1920; his counts alone came close to the actual number of chromosomes in living cells. Winiwarter succeeded where his contemporaries failed because of his exemplary technique. Winiwarter was the first to fully realize the necessity for immediate fixation of fresh tissue. Furthermore he insisted that the appropriate fixative mixture had to be selected for the faithful preservation of chromosomes. When his strictures were finally heeded by other cytologists in the 1920s they, too, began to make accurate mammalian chromosome counts.

Winiwarter was a student of the renowned cytologist E. von Beneden. Winiwarter began his study of mammalian chromosomes at the end of the 19th century. In an important paper published in 1900, he argued from his observations of rabbit oogenesis that chromosomes formed side-by-side pairs during the synapsis stage of the first maturation division. In the last decade of the 19th century, several different conceptions of chromosome behavior during germ cell maturation coexisted. A minority view held that univalent chromosomes paired to form bivalents, but this pairing was presumably end-to-end. Winiwarter was the first to contend that pairing was side-by-side.[9]

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[6] The possibility has been raised several times that cytologists prior to 1920 mistook primary spermatocytes for spermatogonia so that their counts of about 24 chromosomes were accurate *haploid* counts. However, this is inconceivable since these same cytologists also made counts of about 12 chromosomes (in supposed haploid cells) which cannot possibly be rationalized as "correct". Beatty, *op. cit.* (n. 1 above), 181; Heberer, *op. cit.* (n. 1 above), 100; T. Painter, "Studies in mammalian spermatogenesis II. The spermatogenesis of man," *J. Exper. Zool.*, 1923, *37*: 313.

[7] R. Hance, "The fixation of mammalian chromosomes," *Anat. Record*, 1917, *12*: 371-387.

[8] Winiwarter was an ardent patriot who, after the German invasion of his homeland Belgium in World War I, changed his name from von to de Winiwarter. He also gave up the German language, which he had often used before the war, for publication I. Diller, personal communication.

In the same year (1900) Winiwarter reported his first chromosome counts. It is revealing to note that while W. Flemming at the same time counted only about 24 chromosomes in the rabbit, Winiwarter counted 42 chromosomes, very close to the actual number 44. Winiwarter reported his counts of 36 chromosomes (actual number 38) in the cat in 1909. Then in 1912 he published a lengthy account of human spermatogenesis. On the basis of his counts of 47 chromosomes in spermatogonia and 48 chromosomes in oogonia, he challenged the widespread belief that human cells possessed only 24 chromosomes. Six years earlier (1906) Winiwarter had turned to the study of human chromosomes in order to check the claim of the British cytologists J. Farmer, J. Moore, and C. Walker that chromosomes in human cancer cells underwent heterotypic mitoses otherwise known only in maturing germ cells. Winiwarter was unable to confirm either these observations or the chromosome counts of Farmer, Moore, and Walker. Winiwarter was sure that there were "40 or 50 and more" chromosomes in tumor cells. Since tumor cells were known to be hyperchromatic, Winiwarter was able to square his counts of 40 or more chromosomes with the accepted counts of 24 chromosomes in normal cells.[10]

By 1912 Winiwarter had reconsidered this conclusion. His exacting technique produced chromosome preparations far superior to those of his contemporaries (Fig. 1). He relied exclusively upon fresh testis material removed during surgical operations and fixed immediately in Flemming's fixative mixture. A few words about this fixative are in order. Flemming concocted it in the course of his classic investigation of mitosis in the late 1870s and early 1880s. Flemming was a master of

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[9] H. von Winiwarter, "Recherches sur l'ovogenèse et l'organogenèse de l'ovaire des mammifères (lapin et homme)," *Arch. Biologie,* 1900, *17*: 105-107; E. B. Wilson, *The Cell in Development and Inheritance* (New York: Macmillan, 1900), 246-258.

[10] H. von Winiwarter, "Über Pagetsche Krankheit," *Arch. f. Dermatologie u. Syphilis,* 1907, *85*: 239-240, 250-259; H. von Winiwarter, "Le corpuscle intermediare et le nombre des chromosomes chez le lapin," *Arch. Biologie,* 1900, *16*: 698-702; H. von Winiwarter and G. Sainmont, "Nouvelles recherches sur l'ovogenèse et l'organogenèse de l'ovaire des mammifères (chat). IV. Ovogenèse de la zone corticale primitive," *Arch. Biologie,* 1908-1909, *24*: 195-196; W. Flemming, "Ueber die Chromosomenzahl beim Menschen," *Anat. Anzeiger,* 1898, *14*: 173-174.

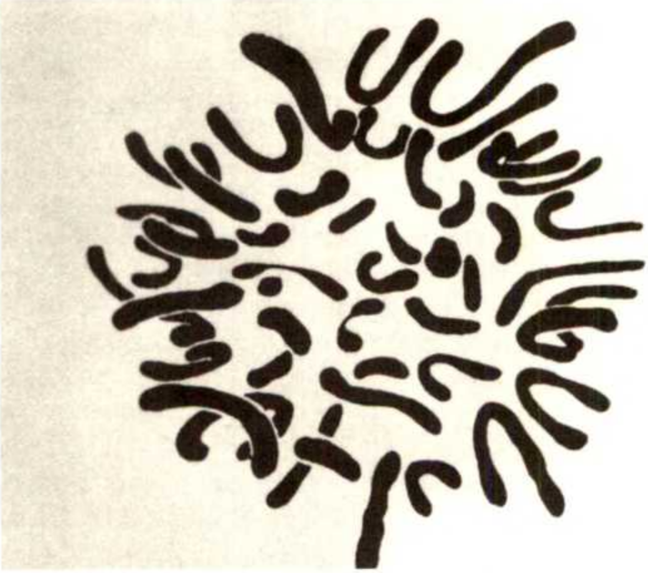

Fig. 1. a. H. von Winiwarter's drawing of a section preparation of (47) spermatogonial chromosomes; tissue was fixed immediately after removal from a living body.

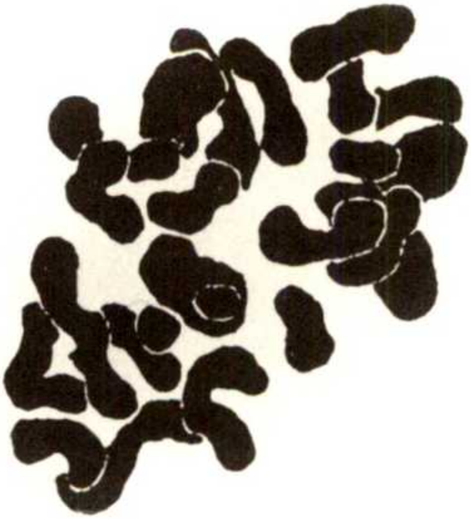

Fig. 1. b. H. Wieman's drawing of a section preparation of (24) spermatogonial chromosomes; tissue was fixed two hours after death. (H. von Winiwarter, *op. cit.* [n. 12], Fig. 16, redrawn by R. Matthey, *op. cit.* [1949, n. 1], 149; H. Wieman, *op. cit.* [n. 14], Fig. 1.)

all phases of cytological technique and so his preeminence in cytology in the late 19th century is hardly surprising. J. Baker commented in 1950 that Flemming's fixative was still unsurpassed for chromosome study. The mixture was composed of "osmic acid" (osmium tetroxide), chromic acid, and acetic acid. Each component used alone as a fixative possessed both good and bad properties. The greatness of the mixture resided in the fact that the disadvantages of one component (such as the shrinkage due to chromic acid) were compensated by the disadvantages of another (such as the swelling due to acetic acid).[11]

The inferior preparations of Winiwarter's contemporaries usually contained few mitoses suitable for even approximate counts. However Winiwarter's material contained many countable mitoses. Winiwarter devoted a great deal of time to the careful repetition of the fatiguing process of chromosome counting because of the vast discrepancy between his high counts and the lower counts of everyone else. In the end he was able to make well over one hundred "exact" and concordant chromosome counts in spermatogonia, primary and secondary spermatocytes and could have made many more counts if he had had the patience.[12] Spermatogonia possessed 47 chromosomes. Primary spermatocytes possessed 24 chromosomes, one of which appeared and behaved differently from the other 23. During the first maturation division Winiwarter saw this unusual chromosome pass undivided to only one of the two poles. Thus in the two anaphase groups formed during a primary spermatocyte division, Winiwarter counted 24 chromosomes in one but just 23 in the other. Accordingly, secondary spermatocytes were of two varieties, with 23 or 24 chromosomes. Winiwarter concluded that males were of the XO sex chromosome type, known for over a decade in insects. In spermatogonia, there were 46 autosomes and one X sex chromosome. In primary spermatocytes, there were 23 (46/2) bivalents and one X. Among secondary spermatocytes, some possessed 23 autosomes and one X, while others possessed just 23 autosomes. If the male were of the XO type, the female should be of the XX type, with a total of 48 chromosomes. Winiwarter was able, with difficulty, to make a few chromosome counts in fetal oogonia and did find the expected 48 chromosomes.[13]

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[11] H. de Winiwarter, "Le nombre des chromosomes chez l'homme," *Rev. Anthropologique,* 1920, *30*: xxvii-xxviii; H. de Winiwarter, "La formule chromosomiale dans l'espèce humaine," *Compt. Rend. Soc. Biologie,* 1921, *85*: 266. Baker, *op. cit.* (n. 4 above), 95-100; for an appreciation of Winiwarter's masterful technique, see Matthey, *op. cit.* (1949, n. 1 above), 22, 106-107.

[12] H. von Winiwarter, "Études sur la spermatogenèse humaine. (I. Cellules de Sertoli. II. Hétérochromosome et mitoses de l'épithelium séminal)," *Arch. Biologie,* 1912, *27*: 93, 147-149; de Winiwarter, *op. cit.* (1920, n. 11 above), xxx.

Other counts of more or fewer than 24 chromosomes were ignored or rejected by those cytologists who believed human cells possessed 24 chromosomes. But Winiwarter's discrepant counts were treated differently. The exceptional quality of his preparations was very apparent, so his counts could not be readily dismissed. Several explanations were offered to reconcile Winiwarter's baffling counts with the consensus in favor of 24 chromosomes. The geneticist T. Morgan, noting that 48 was twice 24 and that the Americans M. Guyer and T. Montgomery—who had counted 24 chromosomes—had obtained their material from Negroes while Winiwarter had obtained his from Caucasians, suggested the possibility of interracial differences underlying different counts. But Morgan failed to take into account the existing counts of 24 chromosomes in whites as well as blacks.[14]

Though Winiwarter's counts could not be rejected, as long as they remained unconfirmed there was no great crisis of confidence in the accepted belief in 24 chromosomes in human cells. Indeed, as late as the 1920s, when counts of 47 or 48 chromosomes were becoming common, some cytologists still sought to reconcile their high counts with the actual presence of only 24 chromosomes in human cells. The case of O. Grosser is illuminating. In two immediately fixed embryos, Grosser confidently counted about 47 or 48 chromosomes in somatic cells of thin membranes which were favorable for counting. Published photomicrographs bear out the accuracy of these counts. However, in a third embryo, which was fixed only after a thirty minute treatment in a Ringer's solution, Grosser found between 20 and 30 chromosome pairs. He interpreted each pair as the two chromatids of a longitudinally-split chromosome. Thus these counts confirmed the prevailing belief in 24 chromosomes. Grosser proceeded to reinterpret his counts of 48 chromosomes as follows. The 48 "chromosomes" were actually prematurely-separated chromatids. 48 chromatids were consistent with the presence of only 24 chromosomes. The treatment of the third embryo with the Ringer's solution had therefore prevented this premature separation. Winiwarter, in contrast, pointed out that the material from the third embryo was clearly inferior just because of its delayed fixation. The Ringer's solution had permitted clumping, hence the low counts.[15]

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[13] von Winiwarter, *op. cit.* (1912, n. 12 above), 129-131, 136-145.

[14] T. H. Morgan, *Heredity and Sex* (New York: Columbia University, 1914), 245-248; T. H. Morgan, "Has the white man more chromosomes than the negro?" *Science,* 1914, *39*: 827-828; M. Guyer, "A note on the accessory chromosomes of man," *Science,* 1914, *39*: 721-722. Other unsuccessful explanations for Winiwarter's high counts were that there had been an abnormal doubling of chromosomes during embryonic development in the individuals Winiwarter had happened to study (but why had no one else ever come across such an individual and why didn't Winiwarter ever count 24 chromosomes?) and that Winiwarter had counted chromosomes in giant germ cells which cytologists knew possessed an increased number of chromosomes. In fact, Wieman had even counted 48 chromosomes in one such giant spermatogonium (cf. below n. 36). H. Wieman, "The chromosomes of human spermatocytes," *Am. J. Anatomy,* 1917, *21*: 12; de Winiwarter, *op. cit.* (1920, n. 11 above), xxxi; de Winiwarter, *op. cit.* (1921, n. 11 above), 267.

48 Chromosomes in Human Cells, 1920-1956

In the 1920s Winiwarter ceased to be an isolated figure in mammalian cytology. T. S. Painter's studies of mammalian chromosomes in the 1920s inaugurated the second period of human chromosome counting, which lasted into the 1950s. Painter was the first to count 48 chromosomes in spermatogonia[16], and his influence proved decisive in the establishment of the long-lasting belief that diploid human cells possessed 48 chromosomes. Painter's interest in chromosomes and spermatogenesis began while he was a Yale graduate student. His thesis (1913) dealt with spider spermatogenesis. Around 1920 he published the first thorough cytological investigation of spermatogenesis in reptiles (lizards). Painter finally turned to the study of mammalian chromosomes because there happened to be an abundance of material available. "There was 'possum meat all over the lab" because of C. Hartman's research on opossum reproduction. Painter's choice of the opossum was a fortunate one. Marsupials possess many fewer chromosomes than eutherian mammals. In the opossum the sex chromosomes are much smaller than the autosomes and therefore easily recognized. Consequently Painter had no difficulty correctly counting the number of opossum chromosomes (22) and identifying X and Y sex chromosomes in spermatogonia and primary spermatocytes. He proceeded to find the two X chromosomes he now expected in female somatic cells. Painter later remarked that his unequivocal identification of X and Y chromosomes in the first mammalian species he studied had a significant impact on his subsequent studies of mammalian chromosomes in species in which the increased number of chromosomes made observations more difficult.[17]

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[15] O. Grosser, "Ueber die Chromosomenzahl beim Menschen," *Anat. Anzeiger,* Erg., 1921, *54*: 181-185. When K. Oguma showed Grosser a longitudinal split in each of the 48 chromosomes, Grosser agreed each had to be a whole chromosome, not a chromatid. S. Schachow and A. Branca tried to explain their counts of about 48 chromosomes as the result of two overlapping anaphase groups of just 24 chromosomes each. O. Grosser, "Entwicklungsgeschichte des Menschen von der Keimzelle bis zur Ausbildung der äusseren Körperform . . .," in *Biologie und Pathologie des Weibes,* ed. J. Halban and L. Seitz, (Berlin: Urban and Schwarzenberg, 1925), VI, pt. 1, 10-12; S. Schachow, "Uber die Chromosomenzahl in den somatischen Zellen des Menschen," *Anat. Anzeiger,* 1926, *62*: 126; A. Branca, "Les canalicules testiculaires et la spermatogenèse de l'homme (Éudes cytologiques)," *Arch. Zoologie expérimentale et générale,* 1924, *62*: 106; H. de Winiwarter and K. Oguma, "Nouvelles recherches sur la Spermatogenèse humaine," *Arch. Biologie,* 1926, *36*: 140-145.

[16] In 1918 H. Evans did count 48 chromosomes in spermatogonia but his count was only reported in a footnote to Babcock and Clausen's genetics textbook and remained unnoticed until Evans brought it to Painter's attention in 1923. Painter, *op. cit.* (n. 6 above), 311.

Having completed his work on opossum chromosomes, Painter began his work on human chromosomes. "I happened to be in the right place at the right time, for one of my former premedical students was practicing in a state mental institution in Austin where, for therapeutic reasons ('excessive self abuse coupled with certain phases of insanity which made the removal of the sex glands desirable') they occasionally castrated male individuals." The testes removed from three inmates (one white and two black) were quickly minced with a "multibladed knife [Painter] had devised by mounting a number of safety razor blades close together . . ." and then, following Winiwarter's lead, immediately fixed. Painter selected E. Allen's modification of P. Bouin's well known fixative instead of Flemming's fixative, an unfortunate choice which became a matter of controversy.[18]

Painter's first brief communication on human chromosomes actually appeared before his more detailed treatment of opossum spermatogenesis. In May 1921, Painter announced that in spermatogonia from both races there were, with "absolutely no question," between 45 and 48 chromosomes, "very close to the number (47) given by Winiwarter." In primary spermatocytes Painter "knew what to look for" and found two chromosomes which looked and behaved just like the X and Y chromosomes in the opossum. Thus he concluded man was of the

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[17] T. Painter, "Chromosomes and genes viewed from a perspective of fifty years of research," *Stadler Symposia,* 1971, *1*: 33-34; C. Oliver, "In memoriam, Theophilus Shickel Painter," *Report of the Special Theophilus Shickel Painter Committee,* University of Texas, 1970, 8; T. Painter, "Studies in mammalian spermatogenesis I. The spermatogenesis of the opossum (*Didelphys virginiana*)," *J Exper. Zoology,* 1922, *35*: 13-45; Matthey, *op. cit.* (1949, n. 1 above), 108.

[18] Oliver, *op. cit.* (n. 17 above), 8; Painter, *op. cit* (1971, n. 17 above), 34; Painter, *op. cit.* (n. 6 above), 293; de Winiwarter and Oguma, *op. cit.* (n. 15 above), 104-105; R. Matthey, "Le problème des hétérochromosomes chez les mammifères," *Arch. Biologie,* 1936, *47*: 321, 364, 374.

XY/XX sex chromosome type and therefore the diploid number of chromosomes should be even, either 46 or 48. It is tantalizing to read that "in the clearest equatorial plates so far studied only 46 chromosomes" were counted.[19] By September 1921, though, Painter had decided in favor of 48, not 46 chromosomes.[20] My suspicion is that Winiwarter, who denied all his life that spermatogonia possessed 48 chromosomes, was ultimately responsible for Painter's changed count. Painter, I believe, was convinced that Winiwarter had conclusively established the existence of a minimum of 47 chromosomes in spermatogonia. But Painter was equally convinced his own work had conclusively established the existence of both X and Y chromosomes in spermatogonia. Thus he added his sure Y to Winiwarter's sure 47 to get, in his opinion, a sure 48 chromosomes in human cells. The fact that the Y was by far the smallest of the 48 chromosomes made it conceivable that Winiwarter had merely overlooked it. Indeed, Painter later noted that in some preparations he occasionally failed to find the Y, no doubt because it was obscured by an overlying large chromosome.[21]

In 1923 and 1924, Painter published definitive studies of human chromosomes in which he presented his complete case for 48 chromosomes, including X and Y chromosomes, in spermatogonia. In addition to spermatogonial counts of 48 chromosomes, Painter reported primary spermatocyte counts of 24 chromosomes. All 24 were bivalents but one was an unequal bivalent. When this bivalent divided during the first maturation division, Painter observed the separation of the X and Y chromosomes. Painter also prepared karyotypes of his chromosome preparations. Shortly before (1922) T. Rappeport published the first human karyotype. In embryonic somatic cells she counted 42 chromosomes. The procedure was to make a drawing of the chromosomes which was usually then magnified before the individual chromosomes were cut out and paired. By process of elimination Painter was left with two unlike chromosomes, a medium length rod and a very small chromosome.

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[19] T. Painter, "The Y-chromosome in mammals," *Science,* 1921, *53*: 503-504; Painter, *op. cit.* (1971, n. 17 above), 34.

[20] T. Painter, "The spermatogenesis of man," *Anat. Record,* 1922, *23*: 129; Painter, *op. cit.* (n. 6 above), 292.

[21] T. Painter, "The sex chromosomes of man," *Am. Naturalist,* 1924, *58*: 510, 512; P. Shiwago and A. Andres, "Die Geschlechtschromosomen in der Spermatogenese des Menschen," *Ztschr. f. Zellforschung u. mikroskopische Anatomie,* 1932, *16*: 429-430; Heberer, *op. cit.* (n. 1 above), 115, 124.

Painter identified them as X and Y chromosomes, respectively.[22] To clinch the presence of a Y chromosome in male cells, Painter introduced genetic evidence. "A father having a certain character (webbed toes) transmitted it to all his sons and none of his daughters. The sons, in turn, transmitted this character to all their sons and none of their daughters. The daughters from the several generations involved never showed the character, nor did they transmit it to any of their offspring." This pedigree could be explained if webbed toes was a Y chromosome-linked trait.[23]

Painter's confidence in his observations and counts of human chromosomes was bolstered by his studies of chromosomes in other mammalian species. In all those species he found X and Y chromosomes. In several he also counted 48 chromosomes. Most persuasive was his investigation of monkey spermatogenesis because "it seemed probable that if the structures which I identified in man really had the fundamental function [sex chromosomes] which I attributed to them, similar elements would be found in other primates." Painter not only found X and Y chromosomes in monkeys but he also counted 48 chromosomes in the rhesus monkey. Painter concluded, "in view of the results obtained in the Old-World [rhesus] monkey, there can be no doubt that the sex chromosomes of man were correctly identified" (see below p. 486).[24]

Not surprisingly, Winiwarter challenged Painter's counts and conclusions. Painter and Winiwarter agreed that spermatogonia possessed 23 pairs of autosomes and that there were at least 23 (equal) bivalents in primary spermatocytes. Their disagreement over the diploid number of chromosomes—48 *vs.* 47—hinged upon the number of sex chromosomes—2 *vs.* 1—in spermatogonia and spermatocytes. In both cat and man Winiwarter had found only one sex chromosome, the X, in male germ cells. His new studies with new material in the 1920s and 1930s confirmed his original observations in both species. Winiwarter's student/associate, K. Oguma, also counted 47 chromosomes in spermatogonia. Karyotypes of spermatogonia with 47 chromosomes revealed just one unpaired chromosome, the X. All small chromosomes were paired. There was no unpaired Y.[25]

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[22] Painter, *op. cit.* (n. 6 above), 296-308, 315-321; Painter, *op. cit.* (n. 21 above), 509-519; T. Rappeport, "Ueber die somatische Mitose des Menschen," *Arch. f. Zellforschung*, 1922, *16*: 374-375.

[23] In a 1932 paper on the possible correlation between cytological abnormalities and genetic defects, J.B.S. Haldane suggested that such a pedigree could also be explained if the mother were XXY, the father were XY, and the trait in question were a rare, X-linked recessive. Though XXY individuals do indeed exist, we now know they are sterile (Klinefelter's) males (see below, p. 501). Painter, *op. cit.* (n. 21 above), 522-523; J. Haldane, "Genetical evidence for a cytological abnormality in man," *J. Genetics*, 1932, *26*: 341-345.

[24] Since the rhesus monkey has only 42 chromosomes, it might appear that Painter had a bad case of 48 "on the brain." But he did count 54 chromosomes in another primate, the cebus monkey, and more and less than 48 chromosomes in other mammalian species. H. Evans and O. Swezy also found 48 chromosomes in both man and rhesus monkey. Painter, *op. cit.* (n. 21 above), 519-522; T. Painter, "Studies in mammalian spermatogenesis IV. The sex chromosomes of monkeys," *J. Exper. Zool.*, 1924, *39*: 438-451; H. Evans and O. Swezy, "The chromosomes in man, sex and somatic," *University of California Memoirs*, 1929, *9*: 35-36.

Winiwarter and Oguma tried to explain away Painter's observations. What Painter had interpreted as an unequal XY bivalent was actually an aberrant autosomal bivalent.[26] But there was no way they would discount his genetic evidence in support of a Y chromosome. Thus they reluctantly entertained the possibility of a chromosomal polymorphism in man. Since the XY sex chromosome type was generally considered primitive and the XO type derived from it after the degeneration of the (small) Y, this polymorphism was possible. However, the remarkable coincidence that Winiwarter and Oguma had always dealt with individuals with 47 chromosomes, while Painter had always encountered those with 48 chromosomes, made the polymorphism hypothesis virtually inconceivable.[27]

Only Winiwarter and his students counted 47 chromosomes in human cells and only they found just a single sex chromosome in the mam-

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[25] de Winiwarter, *op. cit.* (1921, n. 11 above), 266-267; K. Oguma and H. Kihara, "Étude des chromosomes chez l'homme," *Arch. Biologie,* 1923, *33*: 493-514; de Winiwarter and Oguma, *op. cit.* (n. 15 above), 99-166; K. Oguma, "A further study on the human chromosomes," *Arch. Biologie,* 1930, *40*: 205-226; K. Oguma, "The segmentary structure of the human X-chromosome compared with that of rodents," *J. Morphology,* 1937, *61*: 59-93; Matthey, *op. cit.* (1949, n. 1 above), 106-111, 122-124, 150-151.

[26] de Winiwarter and Oguma, *op. cit.* (n. 15 above), 123-125, 135, 152-153. In 1930, Oguma expanded upon this effort to reinterpret Painter's observations While accepting Painter's identification of the X, Oguma argued that either a small displaced "tetrad," a "chromatoid body," or a "plasmosome" had simulated a small Y chromosome. Still later (1937), Oguma adopted a strikingly new position. The supposed XY bivalent was actually just the X chromosome. The X chromosome possessed a variable tripartite structure which had previously escaped observation because of inadequate fixation. The third part of the X (often separated by a thin thread from the other two parts) was what other cytologists mistook for a small Y. Oguma, *op. cit.* (1930, n. 25 above), 217-224; Oguma, *op. cit.* (1937, n. 25 above), 63-79; *cf.* Matthey, *op. cit.* (1949, n. 1 above), 152.

[27] de Winiwarter and Oguma, *op. cit.* (n. 15 above), 156-158; Heberer, *op. cit.* (n. 1 above), 116; Matthey, *op. cit.* (n 18 above), 322.

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malian species they studied.[28] In contrast, Painter's work was widely confirmed in the 1920s and 1930s. In all mammalian species examined, both X and Y chromosomes were recognized in male cells. Counts of 48 chromosomes in diploid cells and/or 24 bivalents in haploid cells were reported by the Americans H. Evans and O. Swezy, the Dane T. Kemp, the Russians A. Andres, P. Shiwago, and M. Navaschin, the Japanese O. Minouchi and T. Ohta, and the Englishman P. Koller.[29] 1937 marked the end, so it seemed, of the controversy over the number of chromosomes in human cells. Though Winiwarter and Oguma never abandoned their claims, the many concordant counts of 48 chromosomes and the many observations of both X and Y chromosomes had apparently settled the matter once and for all.

If there were, indeed, 48 chromosomes in diploid cells this brief history would be nearly complete. The only puzzle would be how the accomplished cytologists Winiwarter and Oguma consistently overlooked the Y chromosome in male mammalian cells. However, diploid human cells possess 46, not 47 or 48 chromosomes and haploid cells possess 23, not 24 chromosomes. Thus the puzzle becomes much larger. How can the repeated counts of 47 or 48, and 24 chromosomes be explained?

Cytologists in the 1920s and 1930s were confronted with many technical obstacles. Fresh tissue from living bodies was almost unobtainable. Evans and Swezy literally waited at the foot of the gallows in order to fix the testis of an executed criminal immediately after death before the chromosomes could clump. Consequently, cytologists based their counts on material from only a few individuals. The preparation of material for microscopy was a tedious task. The fixed tissue was washed in water to remove excess fixative, dehydrated with alcohol, "cleared" with a clearing agent to remove the alcohol, infiltrated with paraffin dissolved in the clearing agent, and finally imbedded in pure molten paraffin. The paraffin-imbedded tissue was cooled and the paraffin block was thin-sectioned with a microtome. The "ribbon" of sections was affixed to the slide. From the sections, the paraffin was dissolved by the clearing agent, the clearing agent was removed with alcohol, and the alcohol was removed with water. The sections were now stained and often counterstained. The stained sections were dehydrated with alcohol, cleared with the clearing agent, and finally mounted in a mounting medium dissolved in the clearing agent. Only then, after several days and many steps, were the cells ready for chromosome counts.

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[28] Matthey, *op. cit.* (n. 18 above), 338; R. Matthey, *op. cit.* (1949, n. 1 above), 132-136, 142-146; *cf.* T. Kemp, "Ueber die somatischen Mitosen beim Menschen und warmblütigen Tieren unter normalen und pathologischen Verhältnissen," *Ztschr. f. Zellforschung u. mikroskopische Anatomie,* 1930, *11*: 434-435.

[29] The Americans R. King and H. Beams did not attempt to count human chromosomes but they did establish the presence of X and Y chromosomes in primary spermatocytes. Evans and Swezy, *op. cit.* (n. 24 above), 1-65; T. Kemp, "Ueber das Verhalten der Chromosomen in den somatischen Zellen des Menschen," *Ztschr. f. mikroskopische-anatomische Forschung,* 1929, *16*: 1-20; Shiwago and Andres, *op. cit.* (n. 21 above) 413-431; A. Andres and M. Navaschin, "Ein Beitrag zur morphologischen Analyse der Chromosomen des Menschen," *Ztschr. f. Zellforschung u. mikroskopische Anatomie,* 1936, *24*: 411-426; O. Minouchi and T. Ohta, "On the number of chromosomes and the type of sex-chromosomes in man," *Cytologia,* 1933-1934, *5*: 472-490; P. Koller, "The genetical and mechanical properties of sex chromosomes. III. Man," *Proceedings of the Royal Society of Edinburgh,* 1937, *57*: 194-214; R. King and H. Beams, "The sex chromosomes in man, with special reference to the first spermatocyte," *Anat. Record,* 1936, *65*: 165-175.

Most importantly, this standard section procedure created serious problems for actual chromosome counting. To begin with, cytologists faced an immense inherent difficulty in counting human chromosomes. In living tissue, human cell nuclei are small, while the number of chromosomes is large.[30] The inescapable consequence is that chromosomes in dividing cells are very crowded and cluttered. They readily overlap and contact each other. The many chemical and physical treatments in the preparation of sections only made matters worse. They intensified the inherent difficulty of counting by causing cells to shrink and chromosomes to swell. The result was that chromosome preparations were hardly clear and distinct. The resolution of individual chromosomes from each other so that each was recorded and then counted one time only was a laborious and controversial endeavor which required constant interpretation that was necessarily subjective. In addition, the microtome knife frequently cut through the equatorial plate of chromosomes, so that all the chromosomes within one cell were not contained in a single section. In reconstructing the chromosomes of a cell from more than one section, the cytologist was never absolutely sure whether the chromosomes in one section were cut ends of chromosomes already counted in the previous section or intact chromosomes not yet recorded.

Given these technical circumstances, the first marvel is that cytologists such as Winiwarter and Painter came so very close in their counts to the actual number of chromosomes, not that they counted 47 or 48 chromosomes instead of 46.[31] Indeed, I want to argue that, with section preparations, the counts of 47 or 48 chromosomes were as accurate as a count of 46 chromosomes would have been. Therefore it would be a truly presentist error to fault cytologists of the 1920s and 1930s for their "incorrect" chromosome counts. Some of Painter's chromosome counts in other mammalian species were, in fact, absolutely correct (housebat 48, hedgehog 48, rabbit 44, mouse 40, rat 42, and horse 60). But, generally speaking, his preparations for these species were no clearer than his preparations of human chromosomes. Paradoxical though it may seem, Painter's "correct" chromosome counts were really no more accurate than his "incorrect" human chromosome counts. It should be recalled that, initially, Painter thought there were only 46 chromosomes in his best human chromosome preparations. However, this "correct" count was certainly no more accurate or reliable than Painter's many subsequent "incorrect" counts of 48 chromosomes. I should add that other cytologists occasionally counted 46 chromosomes but the circumstances were no different from those in which they more frequently found 48.

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[30] It is hardly accidental that the first precise chromosome counts involved salamander cells in which nuclei are very large and the number of chromosomes (24) is small. See C. Rabl, "Ueber Zelltheilung," *Morphologisches Jahrbuch*, 1885, *10*: 238-250.

Photomicrographs of section preparations from the 1920s and 1930s readily convey the problems in human chromosome counting (Figs. 2-4). In contrast, published drawings—by themselves—of the chromosome preparations are misleading. From such drawings one could easily get the impression that 47 (or 48) chromosomes were clearly and distinctly visible to any observer. However, a comparison of what the cytologist actually looked at (the photomicrograph) with what the cytologist claimed he saw (the drawing) illustrates the interpretative element in counting (Figs. 3, 4, and 9). An even more revealing example of the subjectivity in counting is the trio of drawings in figure 5. Evans sent two preparations to Winiwarter and Oguma to convince them of the presence of 48 chromosomes in human cells. I think it is safe to conclude from the circumstances that Evans considered these preparations among his best. There are no photomicrographs of the preparations, but the drawings of them by the three cytologists were published. In one of them, Evans counted 48 chromosomes while Winiwarter counted 54 and Oguma counted 53. Besides the blackened chromosomes, the four major areas

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[31] *Cf.* T. C. Hsu's remark, "I did get a chance once to see a slide prepared by Dr. Painter, and I failed to make any sense out of the twisted, crowded, stacked chromosomes. It's amazing that he even came that close!" T. C. Hsu, personal communication, (see below p. 490).

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IMPERIALISM AND MEDICAL EDUCATION IN CHINA *

JOHN Z BOWERS

The era of foreign imperialism—of cutting up the Chinese melon—began in 1894-95 when Japan's overwhelming victory in the Sino-Japanese War demonstrated China's helplessness. Immediately there was a rush by the foreign powers to obtain economic and political concessions and to establish exclusive areas of influence.

Russia, Germany, and France believed they were entitled to special privileges in China because they had intervened in her behalf in the treaty following the Sino-Japanese War.

The British, at the height of their maritime empire, and as the oldest and most broadly established Western power in China, were equally determined to strengthen their position.

The attitude of the United States was epitomized by its "open door"

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I am grateful to Mrs. Akiko K. Bowers for translating the Japanese sources on the South Manchuria Medical College.

President Michio Okamoto of Kyoto National University made the archives on Manchuria available. Père Joseph Dehergne, S.J., made the sources on Aurora University available. L. Carrington Goodrich was a valuable source of information and corrected drafts of this manuscript.

policy toward China.[1] In September 1899 Secretary of State John Hay proposed that there should be "spheres of influence" to preserve and increase foreign trade with China and the principle of the open door followed in a supplementary note in July 1900. The open door principle was reaffirmed by Great Britain and Germany on 16 October 1900. It was aimed at preserving the integrity of China as a nation.

The era of imperialism that we are concerned with was marked by a number of important benefits for China. Carrington Goodrich has noted some of these:

> Bridges were built, lighthouses erected, and roads laid, new crops and techniques were introduced . . . there developed in the end greater respect for the public health and welfare of the common man than ever obtained in imperial China, countless millions were touched by the selfless devotion of unarmed civil servants, teachers, nurses, physicians, and evangelists who sought no reward.[2]

In this communication we shall describe briefly the development of representative medical schools established by the major imperialist powers. All were of course motivated in part by the wretched state of medicine and public health in China, where only a handful of physicians were trained in Western medicine.[3]

German Medical Education

At the beginning of the twentieth century German medical science was in a position of world leadership. Following the murder of two German missionaries, in 1897 the Germans occupied Tsingtao where they opened a small medical school, which was closed in 1920. A second German medical school opened at Tong-Houn, four kilometers from

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[1] Edward Thomas Williams, *China, Yesterday and Today*, rev. ed. (New York: Thomas Y. Crowell, 1923) and J. K. Fairbank, *The United States and China* (Cambridge, Mass· Harvard University Press, 1958), pp. 254-7.

[2] L. Carrington Goodrich, "Book Review," *The China Quarterly*, January-March 1970, *41*: 146.

[3] K. Chimin Wong and Wu Lien-Teh, *History of Chinese Medicine* (Tientsin: The Tientsin Press, 1932). This work includes numerous references to the medical schools described in this study. See also for comments on visits to the schools: E. M Merrins, "Medical education in China," *The Chinese Recorder*, 1917, *48*· 497-506; China Medical Commission of the Rockefeller Foundation, *Medicine in China* (China and New York: University of Chicago Press, 1914); Simon Flexner, "Report of the Second China Medical Commission," mimeographed, the Archives of the American Philosophical Society, Philadelphia E. V. Cowdry, "Anatomy in China," *Anatomical Record*, 1920-21, *20*. 32-60, includes general information, as well as descriptions of programs in anatomy For a later period, see Professor Knud Faber, Professor of Medicine at Copenhagen University, *Report on Medical Schools in China* (Geneva: League of Nations Organisation Publications, Series II, 30 June 1931, Official No. C.H. 961, No. 8).

Canton. The principal German effort, however, was in Shanghai, China's leading seaport and largest city. Both the Germans and the French had major concessions in Shanghai, where sizeable numbers of their citizens received medical care from expatriate physicians and surgeons.

In 1899, with financial support from two German trading cartels, two German physicians, E. H. Paulun and F. von Schwab, established a hospital for indigent sick Chinese on Burkhill Road, in the Zikawei section of Shanghai adjoining the concessions. The hospital was named for Paulun after his death in 1909.

In 1905, the German consul-general in Shanghai, Dr. J. von Knappe, proposed that a medical school symbolizing Germany's preeminence in medical science should be established in China. Under von Knappe's vigorous and persuasive leadership, the German Physicians' Association of Shanghai, led by Paulun and von Schwab, opened a medical school adjoining the German hospital in 1907 with ten students. The imperialistic significance of the school was described in a German communication: "a model school—which would cast throughout the country the shining light of the glory of German science. . . . The school of medicine, thanks above all to the participation of the German doctors in Shanghai . . . performs a service to Germanism far in excess of the goal of German medical science." [4]

Major financial support was obtained from the Koppel Foundation of Berlin, the Committee for German Civic Authority in China, and the German Asiatic Society of Berlin. The zoological, anatomical, and pathological museums were donated by medical faculties in Germany. The basic science faculty of the new school was drawn from German medical schools; all clinical subjects were taught by practitioners from the German Physicians' Association of Shanghai. Established as the German School of Medicine for Chinese, it was popularly known as *Tung-chi* (literally, harmony) medical school.

The curriculum was patterned strictly on the German system, beginning with four years in a preliminary, or language, school. The first two years were assigned to the *Vorklinicum* for the study of the basic medical sciences. The *Klinicum* was three years in duration and was spent at the Paulun Hospital. Graduates seeking postgraduate studies were transferred to medical faculties in Germany. There were research programs in dysentery, kala-azar, and sprue.

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[4] Quoted by M. Frédet in *Echo de Chine* (Paris, 21, 25, and 27 March 1916), reprinted under the subtitle, "La création d'une école de médecine à Changhai," in "Ce qu'on dit de l'Ecole française de médecine à Changhai," *Rélations de Chine* (Paris, 14 July 1919), p. 481.

The students were exclusively Chinese, with approximately 80 percent selected by the faculty. The remainder were divided between the Chinese government and Red Cross for the assignment of students. All instruction was in German. A unique development in 1912 was the establishment of a technical college for the study of engineering in association with the medical school, financed by Krupp and other major industries in Germany under the sponsorship of Prince Henry of Prussia. The purpose of the school was enunciated in a German communication:

> The Chinese will learn, under German guidance, modern engineering techniques; the knowledge they will have gained in the workshop will give them a basis on which to proclaim the high quality of German material, of German workmanship and the superior intelligence of the German effort.[5]

Thus Germany's major educational effort in China was to train Chinese engineers as well as physicians.

When Simon Flexner visited China as a member of the Second China Medical Commission of the Rockefeller Foundation in the fall of 1915, he ranked the educational program at Tung-chi with that at the Harvard Medical School of China. He described the Harvard program as "perhaps better done than anywhere in China," and in a footnote added, "except the German school at Shanghai." [6]

The medical school and hospital were enlarged in 1910 but seized by the French in 1914 at the outbreak of World War I. Tung-chi later opened under new auspices. It was recognized in 1921 under a Chinese board of directors; in 1923 its status was raised to a university and it came under sponsorship of the provincial government. The basic science departments were transferred to the Woosung section of Shanghai with the Technical College in the same year.[7]

French Medical Education

The major French effort was primarily a result of the mounting bitterness between France and Germany in the years preceding World War I. French nationalism and royalism had experienced a sharp resurgence in 1906 after the Dreyfus case.

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[5] *Ibid.*

[6] Perhaps Flexner was influenced in part by his total devotion to the German philosophy and system of medical education. See Simon Flexner, "Report of the Second China Medical Commission," mimeographed, Papers of Simon Flexner, Archives of the American Philosophical Society, Philadelphia.

[7] Wên-ya Chuang (ed.), *Handbook of Cultural Institutions in China* (Shanghai: Chinese National Committee on Intellectual Cooperation, 1936), p. 10.

The outcome of the Sino-French War in 1883-85 established French sovereignty over Cochin China, Cambodia, Annam, and Tonkin. Subsequently, with the acquisition of Laos and Vietnam, French Indochina came into being. In 1890 the Pasteur Institute was founded in Saigon as a branch of the Paris Institute and under its general direction. It has been described as the first laboratory of microbiology in the Far East.[8] Pasteur Institutes were subsequently founded in Hanoi and in Annam, and microbiological centers at Hue, Pnompenh, and Vientiane. The primary purpose of the programs was to prevent infectious disease and to study all aspects of tropical pathology.

In 1900 the Hôpital Doumer was opened in Canton, in honor of Paul Doumer, governor-general (1897-1902) and the architect of France's development of Indochina. Three years later, in 1903, with financial support from the governor-general of Indochina, a program was established to train young Chinese men as *infirmiers.* On March 19, 1902, Aurora University was opened in Shanghai by Siang-Pe Ma (also known as Hsiang-fo Ma), a Chinese Catholic literateur and political leader; the institution was turned over to the Jesuits shortly thereafter.[9]

St. Mary's Hospital, operated by the Sisters of Charity, St. Vincent de Paul, was opened in 1907 in a lovely park in the heart of the French concession in Shanghai. Subsequently a Pavillion du Sacre Coeur for maternity cases and children and St. Vincent's Pavillion for indigent men were added.

It was not until 1913, however, that serious consideration was given to a program in medical education at Aurora University. In that year Professor Agrégé Eugène Vincent, of the faculty of medicine at Lyon, came to China to make recommendations on medical education. In his report Vincent expressed the sentiments of a French patriot toward the German effort:

> The Germans vie with tenacity, many marks and soldiers to conquer a very important place in China; they follow us step by step to profit from our long labours in all branches of the [Chinese] civilization.[10]

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[8] Lieow Yong-Chuen, "Les Instituts Pasteur d'Indochine," *Université L'Aurore Bulletin Médical,* August 1934, *10*: 146-71.

[9] "Université L'Aurore Shanghai," *Renseignements généraux et organisationales études* (Shanghai, 1959). The archives of Aurora University are deposited at Les Fontaines, Chantilly, France.

[10] Eugène Vincent, Professor agrégé on the faculty of medicine at Lyon, *L'influence française en Chine et les entreprises allemandes—necessité de créer en Chine une Faculté Française de Médecine* (Lyon: A. Rey, Imprimateur de l'Académie, 1914), p. 9.

In another report Vincent compared the aggressive programs of the other Western powers with those of France:

> At the present time, the Americans, British, and Germans are creating in the great centers of China, medical schools and hospitals which are also the foyers of propaganda, intended to assure the moral supremacy of their country. Money is readily available for these institutions, while for us every *sou* is given with the greatest parsimony.[11]

Although a start had been made in Canton, Vincent recommended that developments in medical education should be centered in Shanghai:

> And we must make it very clear that it is at Shanghai that French influence faces the greatest challenge from the Germans.[12] He compared the need to counterbalance British and American programs in China with the challenge that had been posed to France by Anglo-American educational developments in the Levant. St. Joseph's University, and its faculty of medicine, had been founded in Beirut by French Jesuits in response to the establishment of the medical program of the Syrian Presbyterian College—now the American University of Beirut.[13]

Vincent's recommendation for a medical faculty at Aurora was enthusiastically adopted; as a sign of the national significance of the event and of governmental endorsement the Chamber of Deputies in Paris promptly appropriated 25,000 *francs* in support of the venture.

The French press voiced strong support; writing in *Débats*, 27 February 1914, M. M. Courant stated:

> A French faculty of medicine has an equally important role to play on the Yangtze as it does in the Near East: all is ready and waiting. Shall today's French citizens be any less intelligent and enterprising than those of the previous generation? [14]

The first students were admitted to the Aurora Faculty of Medicine in 1914. The emphasis was on training Chinese, and 90 percent of the students were of native origin. All instruction was in French. The philosophy and programs were typically French, with two years premedical and four years medical education. From his first day in medical school, the student spent every morning at St. Mary's Hospital.

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[11] Eugène Vincent, *La médecine en Chine au XX^e siècle* (Paris, G. Steinheil, 1915), p. 14.

[12] Vincent, *L'influence française en Chine*, p. 9.

[13] American missionaries, led by Philip Bayard Dodge, announced plans for the Syrian Presbyterian College at Beirut in 1862, and premedical and medical subjects were added in 1867. The college became the American University of Beirut in 1920 (P. Bayard Dodge, *The American University of Beirut* [Beirut, 1958]). The French Jesuits, at the urging of French statesman Leon Gambetta, opened St. Joseph's University at Beirut in 1875. The faculty of medicine was opened in 1883.

[14] Maurice Courant, *Débats* (Paris, 27 February 1917), reprinted under the subtitle, "Une faculté française de médecine à Changhai," in "Ce qu'on dit de l'école française de médecine pour Changhai," *Rélations de Chine* (Paris, 14 July 1916), p. 476.

American Medical Education

One of the finest expressions of international good will by the United States was its decision to remit the indemnity owed by the Peking government, as a result of the Boxer Rebellion, in order to establish a program of education and culture in China. This step had a major impact on every field of learning, including medicine.

The original indemnity amounted to US $53,348,000, and the proposal to remit it came from Dr. Arthur H. Smith, a Congregational missionary of the American Board of Commissioners for Foreign Missions in China, with President Theodore Roosevelt in a meeting on 6 March 1906. Smith was introduced to Roosevelt by the Reverend Doctor Lyman Abbott, also a Congregational clergyman and editor of the magazine *The Outlook.*[15] In 1908, on the recommendation of Secretary of State Elihu Root, the Congress authorized the president to modify the bond to $13,655,192 and to remit the balance in favor of China.

The funds were used for scholarships awarded to Chinese youths to study in colleges in the United States. Selection was through a national competition, with preliminary examinations at the provincial level and the final examinations in Peking. The first Boxer Indemnity Scholarships were awarded in 1909 and the following year seventy Chinese students went to the United States. In the first eighteen years of the program, 1,300 men and women were given scholarships. The Chinese government made further use of the indemnity money by founding Tsing Hua College in Peking in 1911 as a junior college to prepare students for study in the United States on Boxer Scholarships.[16]

In 1917, Henry Cabot Lodge, chairman of the Senate Committee on Foreign Relations, recalled his role in the original remission and proposed that the remaining debt be cancelled:

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[15] "Theodore Roosevelt and Arthur Smith," *The Missionary Herald,* 1925, *121* (1). 3-5; Laurence F. Abbott, *Impressions of Theodore Roosevelt* (Garden City, N.Y: Doubleday, Page, 1919), pp. 143-6; Philip C Jessup. *Elihu Root,* 2 vols. (New York: Dodd, Mead, 1938), vol. 1, pp. 387-8.

[16] In 1925 a policy of development to a university status was adopted for Tsing Hua and the name was changed to Tsing Hua University. Later the school was designated as National Tsing Hua University. [Tsing Hua or Ch'ing Hua, the name of a garden adjoining the old summer palace, the site of Tsing Hua University, which was destroyed by the British and French in 1860. From Shui mu ch'ing hua. the water is clear and the trees are beautiful.]

> I introduced a resolution to return a portion of the indemnity to China. It amounted, I think, altogether, to eleven million dollars. This was passed, and the money returned, and I do not think we did anything that caused a better feeling toward us in China. It is still remembered. When the war came we arranged for a suspension of further payments from China to us on the indemnity account (see telegram of 5 September 1917) . . . Now what I would suggest is this . . . to put an end to any further payments from China . . . there is a very friendly feeling toward China . . . and sympathy with China in the Shantung business.[17]

In May 24 the United States remitted the remainder of the indemnity funds to be used for further educational and cultural work in China.

The Boxer Indemnity program produced a group of young men and women who became leaders in Chinese scientific, cultural, and educational fields. One of the more distinguished medical scientists from the Tsing Hua program was Chen Ko-kuei, who, working with Carl F. Schmidt in the laboratories of the Peking Union Medical College (PUMC) identified ephedrine as the active principle in an indigenous medication, Ma Huang.[18] Another was Hsien Wu, who with Otto Folin at Harvard, developed the Folin-Wu method for preparing a protein-free filtrate of the blood.[19]

British and North American Missionary Medical Education

The educational programs of the United States were under a variety of auspices. Sponsorship by American universities included Yale-in-China, Harvard Medical School of China, and the University of Pennsylvania at Canton and later Shanghai. Peking Union Medical College (PUMC) was financed and sponsored by the China Medical Board (CMB) of the Rockefeller Foundation. These institutions have been described extensively elsewhere.[20] Our principal interest is in the

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[17] *Foreign Relations of the United States, 1921,* vol. 1, National Archives, pp. 398-9. The failure at Versailles to return the province of Shantung, which had been held by Germany, to China caused a violent reaction in China with attacks on the foreign legations. Shantung remained in Japanese hands until 1922 when Japan agreed at the Washington Conference, 4 February 1922, to return the province and the railways to China. The Japanese had seized Shantung from the Germans at the outbreak of World War I.

[18] K. K. Chen and Carl Schmidt, "The action of ephedrine, the active principle of the Chinese drug Ma Huang," *Journal of Pharmacology and Experimental Therapeutics,* 1924-25, *25*: 339-57.

[19] Hsien Wu, *A System of Blood Analysis, with Special References to Uric Acid* (Ph.D. diss., Harvard University, 1919).

[20] For a discussion of Yale-in-China see Edward H. Hume, *Doctors East, Doctors West: An American Physician's Life in China* (New York: W. W. Norton, 1946); William Reeves, Jr., "Sino-American Cooperation in Medicine: The Origins of Hsiang-Ya (1902-1914)" in *American Missionaries in China,* papers from Harvard seminars, Kwang-Ching, ed. (Cam-

British and North American missionary medical schools because of their unique character and their contributions to medicine in China.[21]

Before the Boxer Rebellion, the work of British, United States, and Canadian missionaries was carried out in small, poorly-housed, understaffed, and inadequately financed medical schools. After the rebellion emphasis was placed on the development of "union" schools, whereby the smaller institutions were merged and received multiple sources of missionary support.

The leading union missionary school was the School of Medicine of Shantung Christian University, designated Cheeloo after 1915,[22] in Tsinan.[23] It had developed in 1909 as Union Medical College by an amalgamation of two small programs: one at Tsinan operated by the American Presbyterian Mission, and the other at Tsingchowfu, under the English Baptist Mission with classes of "peripatetic" students rotating between the two institutions. The CMB made a major grant of $150,000 (Chinese) to the Shantung School of Medicine in 1915, at the time of the Transfer to Tsinan of the three lower classes from the Union

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bridge, Mass.: East Asia Research Center, 1966), pp. 129-183. For Harvard Medical School of China see Harvard Medical School of China, Inc., *Annual Reports of the Executive Committee* (Cambridge, Mass., 1902-1914); Jean A. Curran, "The Harvard Medical School of China," *Harvard Medical Alumni Bulletin*, 1963, *38* (2)· 12-19. For University of Pennsylvania see W. W. Cadbury and M. H. Jones, *At the Point of a Lancet: One Hundred Years of the Canton Hospital, 1835-1935* (Shanghai. Kelly and Walsh, 1935); Mary Lambertson, *St. John's University, Shanghai, 1879-1951* (New York: United Board of Christian Colleges in China, 1955). For Peking Union Medical College see Mary E. Ferguson, *China Medical Board and Peking Union Medical College: A Chronicle of Fruitful Collaboration: 1914-1951* (New York: China Medical Board of New York, 1970); John Z. Bowers, *Western Medicine in a Chinese Palace: Peking Union Medical College, 1917-1951* (New York: The Josiah Macy, Jr. Foundation, 1972).

[21] For a general survey of the missionary medical schools see Harold Balme, *China and Modern Medicine: A Study in Medical Missionary Development* (London: United Council for Missionary Education, 1921).

[22] The ancient China of Confucius and Mencius was considered to be divided into two kingdoms: Chee and Loo.

[23] Personal communication, E. B. Struthers (May 10, 1974). See also: R. T. Shields, "The origin and development of the School of Medicine of Shantung Christian University," *China Medical Journal*, 1926, *40*: 759-62; J. G Lutz, *China and the Christian Colleges, 1850-1950* (Ithaca and London: Cornell University Press, 1971), pp 150-1; K. S Latourette, *A History of Christian Missions in China* (London: London Society for Promoting Christian Knowledge, 1929); John L. Stuart, *Fifty Years in China: The Memoirs of John Leighton Stuart, Missionary and Ambassador* (New York: Random House, 1954), p. 43; Charles H. Corbett, *Shantung Christian University (Cheeloo)* (New York: United Board for Christian Higher Education in Asia, 1955); William P. Chalfant, "The opening of the Union Medical College in Tsinan," *The China Medical Journal*, 1911, *25*: 316.

Medical College in Peking, which the CMB had acquired and which was to become PUMC.

As the drive to bring the splintered medical schools into a small group of union institutions grew, other institutions were merged with Cheeloo: the Union Medical College of Nanking (1917); the Medical School in Hankow (1919); and the North China Union Medical College for Women in Peking (1924). By 1926 a total of ten United States, British, and Canadian missionary societies were cooperating in Cheeloo and it was described by Shields as follows: "From the international, denominational, and coeducational standpoint this is the largest union enterprise in China, if not in the world." [24]

From the beginning, the language of instruction in most of the missionary schools was Chinese, first because it was the language of the people, and because of the missionaries' historical contributions in translating medical texts into Chinese.[25] As early as 1851, Benjamin Hobson, M.B., B.S., an English missionary, had prepared a Chinese text on anatomy and physiology. Other missionaries continued and expanded the practice. John Glasgow Kerr, M.D., L.L.D., for forty years an American missionary at Canton, published over fifteen texts and monographs.[26] With the growth of the union medical colleges, Cheeloo, led by Randolph T. Shields, an anatomist, became the center for translations into Mandarin of such works as Gray's *Anatomy* and Osler's *Principles and Practices of Medicine.*

Although Mandarin was the language of instruction at Cheeloo, students were required to learn English and urged to read English texts and medical literature. Indeed, the students frequently urged their teachers to lecture in English.[27] A teacher might spend eight to ten hours preparing a lecture in Mandarin, and then be distressed to find the students taking notes in English.

Probably the first free school health program in a missionary school was established in 1936 at Tung Chia Chang by Professor E. B. Struthers, from Cheeloo. With groups of twenty-five medical students, Struthers conducted periodic physical examinations and immunization; opened a club for mothers; and instructed peasants in protecting their heavily contaminated wells.

The missionaries usually spent their first year in China studying at language schools in Peking, Nanking, or Canton. The missionary doctors were so eager to get on with their medical work, however, that many of them gave only half-hearted attention to their language studies.[28] However, there were faculty members at the leading union medical colleges who were able to teach medicine effectively in Chinese.

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[24] R. Shields, in *The China Medical Journal,* 1926, *40*: 760.

[25] The exception was at St. John's University in Shanghai

[26] Cadbury and Jones, *At the Point of a Lancet*

[27] Personal communication, Gordon King (April 1972).

Admission requirements at the missionary schools included graduation from a six-year middle school followed by two years of premedical studies. All the missionary schools operated their own premedical department; the medical course was five years in duration, with the fifth year as an internship.

Cheeloo played the leading role in producing graduates to staff the North American and British missionary hospitals.[29]

Significant parasitology studies were carried out at the missionary hospitals. The most important work was done by Claude H. Barlow in Shaohsing, Chekiang Province, on Fasciolopsis. As one phase of his studies Barlow ingested ova and adult Fasciolopsis buski to determine transit time from the moment eggs were swallowed until they appeared in the stool.[30] He was also interested in learning the time required for a patient to become ova-free. Barlow subsequently published his definitive monograph on the life cycle of Fasciolopsis based in part on these studies.[31]

In Egypt in 1944 Barlow renewed his self-experimentation by infecting himself with Schistosoma haematobium in order to bring the parasite to the United States to determine if any species of native snail could act as the intermediate host.[32]

Japanese Medical Education

Japan was represented in China by a unique and excellent institution, the South Manchuria Medical College in Mukden.[33] It was sponsored by the South Manchuria Railway Company, and while jointly owned by private capital and the Japanese government, absolute control rested in the hands of the government.[34]

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[28] Personal communication, L. C. Goodrich (April 1974).

[29] Personal communication, Jean A. Curran (April 29, 1974).

[30] C. H. Barlow, "Experimental ingestion of ova of Fasciolopsis buski; also the ingestion of adult Fasciolopsis buski for the purpose of artificial infestation," *Journal of Parasitology,* 1921-2, *8*: 40-44.

[31] C. H. Barlow, *The Life Cycle of the Human Intestinal Fluke, Fasciolopsis Buski (Lankester)*, American Journal of Hygiene, Monographic Series, No. 4. (Baltimore: Johns Hopkins Press, 1925).

[32] C. H. Barlow and H. E. Meleney, "A voluntary infection with Schistosoma haematobium," *American Journal of Tropical Medicine,* 1949, *29*: 79-87.

[33] The major archives are in the historical section of the medical library of Kyoto University, Kyoto, Japan.

Construction of the 650-mile-long South Manchuria Railway (SMR) was begun by Russia in 1898 to connect her naval bases at Dairen and Port Arthur with the Chinese Eastern Railway. It was an enterprise of the Russo-Chinese Bank under a secret treaty between Russia and China, signed in 1896. The two railways, together with the Trans-Siberian line, were part of Russia's grand design, started under Tsar Alexander III in 1866, to establish land power in the Far East. The special prize for Russia was Manchuria, a virgin land of vast untapped natural resources such as timber and minerals, and the largest seams of bituminous coal in the world.[35]

Japan also had her designs on Manchuria, not only because of the resources but because of its value as a fortress against China to the south and Russia to the north. France and Germany encouraged Russia, while Britain was officially allied with Japan. While the other powers were preoccupied with the Boxer Rebellion, Russia occupied most of Manchuria in 1900. Refusing to withdraw her troops, she moved toward the occupation of Korea, a country over which Japan considered herself to have proprietary rights. Hostility between the two nations mounted, culminating in the outbreak of the Russo-Japanese War when Japan attacked Port Arthur on 8 February 1904. Japan emerged victorious and the Portsmouth Treaty of 5 September 1905 restored Manchuria to China. The treaty ceded the SMR and leaseholds on the Liaotung Peninsula and the Kwantung Leased Territory.

On 7 June 1907 a Japanese Imperial Ordinance established the South Manchuria Railway Company (*Minami Manshū Tetsudō Kabushiki Gaisha*), generally known as *Mantetsu,* with headquarters in Dairen.

As with other major Japanese enterprises, the railway maintained hospitals and dispensaries for its employees and their dependents. They were located in the seventeen major communities along the right-of-way with the largest at Dairen.

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[34] *Mantetsu Sanjūnen Shi* [Thirty years of the South Manchuria Railway Company] (Dairen, 1937). E. H. Harriman, President of the Union Pacific Railroad, tried unsuccessfully to purchase the railway from the Japanese government in the fall of 1905. It was part of his plan for a round-the-world transportation line by way of the Pacific, Japan, Manchuria, Siberia, European Russia, and the Atlantic Ocean. See George Kennan, *E. H. Harriman, A Biography,* 2 vols (Boston and New York: Houghton Mifflin, 1922), vol. 2, p. 2.

[35] Thomas S. Logan, Inc. (comp.), *Manchuria, Land of Opportunities* (New York: Thomas S. Logan, Inc., 1922); Owen Lattimore, *Manchuria, Cradle of Conflict* (New York: Macmillan, 1932); Richard Story, *A History of Modern Japan* (Middlesex, Eng.. Penguin Books, 1960), Hugh Borton, *Japan's Modern Century* (New York: Ronald Press, 1955); and Chitoshi Yanaga, *Japan since Perry* (New York: McGraw Hill, 1949).

The railway also instituted a major program in public health with a hygiene institute in Dairen, isolation hospitals in Dairen and Port Arthur, the principal seaports, and public health stations in the principal towns along the railway.

Public health was a keystone of Japanese colonial policy in Manchuria:

> It may be taken for granted that the public health system of any country reflects the stage of progress of that country . . . the enlightenment of a country stands proportionate to how near perfection its public hygiene system is. The genius of a correct colonizing policy lays down the public hygiene enterprise as its herald or forerunner.[36]

Massive public health problems existed in Manchuria—tuberculosis, cholera, smallpox, typhus fever, and pneumonic plague; 60,000 died from the epidemic of pneumonic plague in 1910-11; and 15 percent of the patients in general hospitals and 30 percent of the troops had active syphilis.[37]

Japanese apprehensions about the health of the railway's employees became manifest at the time of the pneumonic plague epidemic. The government threatened publicly to take over public health control in Manchuria unless China immediately established an effective plague-prevention program.

The man responsible for the emphasis on public health as a keystone of the railway's program was its first president, a physician, Gotō Shimpei (1857-1929).[38] He had previously served as chief civil administrator in Taiwan, where he established an enlightened and comprehensive public health program. The program Gotō established in Manchuria was based on "his belief that the development of Manchuria should rest on the improvement of public health and hygiene."[39] He was also responsible for the establishment of a diversified research department as a major resource for the railway's colonial development scheme.[40]

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[36] M. Tsurumi, "Public hygiene in Manchuria and Mongolia," introduction, *The Light of Manchuria* (1 February 1921), p. 1

[37] Carl F. Nathan, "Plague prevention and politics in Manchuria, 1910-1931," (Cambridge, Mass: Harvard East Monographs, 1967); Wu Lien-teh, *Plague Fighter: The Autobiography of a Modern Chinese Physician* (Cambridge, Eng: W. Heffer, 1959); and W. H. Jefferys and J. L. Maxwell, *The Diseases of China* (London: John Bale Sons and Danielson, 1910).

[38] Shinobu Seizaburō, *Kaga Kuteki Seijika no Shigai* (Tokyo: Hakubun-kan, 1941); "Gotō Mantetsu Sōsai shusoku jōyūsho, Gotō Hakukokyo tsūita to shitai" (Dairen: Mantetsu Shain Kai, 1929); and Charles A. Beard, "Japan's statesman of research," *American Review of Reviews,* September 1924.

[39] Shinobu Seizaburō, *Gotō Shimpei* (Tokyo: Hokai, 1941), p 6

The railway's establishment of a medical college in Manchuria was seen as one aspect of the goal of improving public health. There was at that time only one Western medical institution of consequence in Manchuria—the Mukden Medical College led by the famed Scottish Presbyterian missionary, Dugald Christie (1855-1937).[41]

The railway's request for approval of a new medical school was endorsed in Tokyo. The proposal called for the establishment of a *semmon-gakko*—a second-level medical school of a type found throughout Japan—that would become a university-level institution at some subsequent date.[42] The South Manchuria Medical College, always known as *Nanman igaku-do,* was officially opened on 15 June 1911.[43]

The medical schools in Japan were divided into two feudatories: the medical schools in northeast Japan were controlled by Tokyo University, while the faculties of the medical schools in southwest Japan and Nanman Igakudo were appointed by Kyoto Imperial University. This relationship with Kyoto was especially beneficial because of the deep interest that Torasaburo Araki, the president of the university from 1915 to 1929, and a physician, had in the college.

As a *semmon-gakko,* the college had a four-year curriculum to which students were admitted directly from middle school, without any period of premedical education. A special "preparatory" course at the South Manchuria Middle School prepared Chinese for the study of medicine at the college, and the first Chinese graduates of the course began the study of medicine in 1913.

The faculty was exclusively Japanese, except for two Germans who taught that language in all four years of the medical curriculum. All instruction was in Japanese with, as in all other Japanese schools, a liberal sprinkling of German scientific and technical terms.

The South Manchuria Medical College was elevated to the level of a university faculty of medicine in 1922 and became *Manshu Ikadaigaku,* with a two-year preparatory school for all students.

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[40] John Young, *The Research Activities of the South Manchurian Railway Company,* 1907-1945 (New York: The East Asia Institute, Columbia University, 1966).

[41] Dugald Christie, *Thirty Years in Moukden, 1883-1913: Being the Experiences and Recollections of Dugald Christie, C.M.G.,* Edited by his wife (London: Constable, 1914).

[42] The *semmon-gakko* approach was also followed by the Japanese when they acquired colleges in Taiwan and Korea. The schools were later raised to imperial universities. See Y. Kusama, "Medical education in Japan," *Journal of Medical Education,* 1956, *31*: 393-8.

[43] "South Manchuria Medical College," in *The Light of Manchuria* (Mukden. Manchurian Enlightening Society, 1 November 1921), pp. 42-50.

The college moved rapidly to become one of the leading medical research centers in the Japanese Empire, approached only by the imperial universities in Tokyo and Kyoto. Its most distinguished scientist, Yas Kuno, a physiologist, became Japan's leading medical scientist through his monumental studies on the physiology of sweat production and control.[44] He was later to say that these studies could only have been accomplished at Mukden because of the excellent facilities, special equipment, and technical support made available to him. In his words, "The railway spared no expenses on the medical college." [45]

In line with the railway's declared policy to conduct research on broad health problems in Manchuria and Mongolia, faculty-student expeditions carried out public health field studies in Inner Mongolia, as well as in Manchuria. The report of the first expedition in 1923 describes how the team developed a simple distillation apparatus to prepare safe drinking water, commented on the prevalence of trachoma and on the exceedingly high incidence of venereal disease among the population, especially the lama priests.[46]

S. Kubata and T. Okanishi of the Department of Pharmacology collected, classified, and described the pharmaceutical properties of over 1700 specimens from plant, vegetable, fish, and animal sources in China, Korea, Japan, and India.[47] These studies, and those of Bernard Read, a pharmacologist on the faculty of PUMC, were the only significant efforts by foreign medical schools in China to explore the vast and historic Chinese materia medica, the exclusive source of therapeusis for hundreds of millions of Chinese at home and abroad.[48]

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[44] Professor Yas Kuno, "Ueber die Schweisskreiten des Menschen," *Transactions of the Fourth Congress, Far Eastern Association of Tropical Medicine* (Weltevreden, Java: Javasche Boekhandel en Drukkerij, 1921), vol. 2, pp 260-70; Kuno, "Ein Apparat für die Bestimmung der Schweissekretin," *Fourth Congress,* vol. 2, pp. 271-3; and "Manchu Ikadaigaku Honjinkai," *Hojin,* 1 November 1964, 7 (special number to honor Professor Kuno).

[45] Personal communication, Kyoto, Japan (1962).

[46] *Manshu Ikadaigaku Daukai Tomo Junkai Shinryo Hokoku,* Report of the First East Mongolian Travelling Clinical Survey (Mantetsu Etseika, 1923).

[47] S. Kubota and T. Okanishi, *Folia Pharmacorum Sinicorum: List of Collection of Chinese Materia Medica* (Mukden, 1931).

[48] Bernard E. Read and Liu Ju-Chiang, *Chinese Medicinal Plants from the Pen Ts'ao Kang Mu A.D. 1596: A Botanical, Chemical, and Pharmacological Reference List* (Peking: Peking National Historical Bulletin, 1936). For Read's full bibliography on the Chinese materia medica, see Joseph Needham, *Science and Civilisation in China,* vol. 1 (Cambridge, Eng.: Cambridge University Press, 1961), pp 290-1

A less praiseworthy Japanese endeavor entailed sending Chinese students to Japan to study medicine in the first decade of the twentieth century. Thirteen thousand students went to Japan between 1902 and 1910.[49] Unfortunately, they were trained in the *semmon-gakko,* since the entrance examinations of the eight numbered higher schools leading to the study of medicine were not only too difficult but were given exclusively in Japanese. When the graduates returned to China they taught at medical schools that replicated the inferior Japanese institutions. The graduates also practiced a poor quality of medicine and, unfortunately, staffed all of the governmental and Red Cross hospitals, including army and navy medical facilities.

Summary and Conclusion

Russia was the only major imperialist power that failed to establish a program in medical education in China, because its domestic resources were so slight, and because her status in China began to decline after the Russo-Japanese War.

The foreign medical schools studiously ignored and/or denounced Chinese traditional medicine, although it was followed to some degree by essentially every citizen of China. Only the Japanese school in Manchuria and PUMC compiled and explored the enormous historical and highly popular Chinese materia medica.

Despite the massive and intriguing public health problems in China, the only academic programs of significance were conducted by the Japanese, Cheeloo, and PUMC. With limited financial and academic resources, it was logical for the schools to concentrate on the rudiments of medicine—anatomy, physiology, pathology, medicine, and surgery.

In contrast to the French, German, and Japanese programs, the British and North American schools were established solely for the benefit of the Chinese students, and they were the only schools where the language of instruction was Chinese. The missionaries made major contributions in compiling and translating medical texts into Chinese. The Germans, French, and Japanese viewed the use of their own languages as an inherent element of their imperialist designs.

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[49] Ling Chi Hong in *Educational Review,* April 1910, p. 8.

FROM 48 TO 46: CYTOLOGICAL TECHNIQUE, PRECONCEPTION, AND THE COUNTING OF HUMAN CHROMOSOMES

MALCOLM JAY KOTTLER

Introduction

Advances in cytology have commonly been the direct result of improvements in cytological technique. The emergence of human cytogenetics as a new discipline in the late 1950s was totally dependent upon a technical revolution in mammalian cytology completed in the early 1950s. The first success of the new cytology was the discovery of the actual number of chromosomes in human cells. In this paper I shall discuss the origin of that discovery and the history of human chromosome counting, an episode in the history of cytology throughout which cytological technique played a decisive role.[1]

Late in December 1955 the cytologist J. H. Tjio, working in the laboratory of A. Levan, counted just 46 chromosomes in human embryonic lung cells. These counts were entirely unexpected. But after a total of 261 concordant counts, Tjio and Levan accepted and published their results. The counts came as a complete surprise to all mammalian cytologists who, for twenty years, had been virtually certain diploid human cells possessed 48 chromosomes. Their certainty was derived from chromosome counts made during the 1920s and 1930s by a number of esteemed cytologists all over the world. Furthermore, counts undertaken as recently as the early 1950s had continued to confirm the prevailing belief in 48 chromosomes. Thus in 1954, just two years before Tjio and Levan's bombshell, the cytologist L. Sachs had declared, "the diploid chromosome number of 48 in man can . . . now be considered as an established fact." [2]

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[1] Several cytologists have written brief but insightful histories of human chromosome counting: R. Matthey, "Chromosomes et intersexualité chez l'homme," *Gynaecologia,* 1963, *155*: 288-298; J. German, "Studying human chromosomes today," *Am. Scientist,* 1970, *58*: 182-201; C. Stern, "The chromosomes of man," *Am. J. Human Genetics,* 1959, *11*: 301-304; J. Hamerton, *Human Cytogenetics* (New York: Academic Press, 1971), I, 1-13; C. Ford and J. Hamerton, "The chromosomes of man," *Nature,* 1956, *178*: 1020-1023; E. Chu, "The chromosome complements of human somatic cells," *Am. J. Human Genetics,* 1960, *12*: 97-100; G. Wendt and B. Wolf, "Die Chromosomenzahl beim Menschen," *Deutsche med. Wchnschr.,* 1957, *82*: 1832-1836; R. Turpin and J. Lejeune, *Human Afflictions and Chromosomal Aberrations* (Oxford: Pergamon, 1969), 7-10. Three invaluable reviews prior to 1956 are: G. Heberer, "Die Ergebnisse der Chromosomenforschung beim Menschen," *Ztschr. f. menschliche Vererbungs- und Konstitutionslehre,* 1935, *19*: 96-129; R. Matthey, *Les Chromosomes des vertébrés* (Lausanne: F. Rouge, 1949), 19-34, 65-68, 74-75, 106-119, 147-152; R. Beatty, "How many chromosomes in mammalian somatic cells?" *Internat. Rev. Cytology,* 1954, *3*: 177-186. Since the completion of my paper, a full-length treatment of the history of human chromosome counting has been published: J.-D. Murken and H. von Wilmowsky, *Die Chromosomen des Menschen* (München: W. Fritsch, 1973).

In view of the long held and repeatedly confirmed conviction that human cells possessed 48 chromosomes, a period of controversy and conflicting chromosome counts following the discovery of Tjio and Levan might reasonably have been expected. But instead of controversy, the Tjio and Levan counts were rapidly confirmed. Within a few months, C. Ford and J. Hamerton, who only a year before (1955) had rejected an offer of human material for chromosome counts because "everyone [knew] that the diploid number [was] 48 and there [was] probably very little more we [could] learn at present," confirmed, in spermatogonia, Tjio and Levan's somatic cell counts and also counted the haploid number of 23 bivalents in primary spermatocytes.[3] During the next three years, many cytologists counted 46 chromosomes in literally thousands of cells from many tissues obtained from many individuals. By 1960, there was no longer any doubt that the diploid number of chromosomes in normal cells was 46.

The extreme ease and rapidity with which Tjio and Levan's discovery was confirmed makes one wonder how so many cytologists could have repeatedly counted 48 chromosomes in diploid cells and 24 chromosomes in haploid cells *prior to 1956* and why, all of a sudden, nearly all cytologists counted 46 and 23 chromosomes *after 1956.* This puzzle is readily solved once one realizes that "cytological technique is coextensive with cytology." [4] Counting might seem to be the most straightforward form of observation. But in the 1920s and 1930s, when the first counts of 48 chromosomes in human cells were made, cytologists still relied on the traditional section technique, developed in the late 19th century. With such sections, counting human chromosomes was extremely difficult and problematic. In contrast, in 1956 and after, with unsectioned material prepared with the new cytological techniques, chromosome counting suddenly became routine and unequivocal. Under such circumstances, the immediate discovery of the actual number of chromosomes in human cells was inevitable.

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[2] J. H. Tjio, personal communication. A. Levan, personal communication. L. Sachs, "Sex-linkage and the sex chromosomes in man," *Ann. Eugenics,* 1953-1954, *18*: 256.

[3] C. Ford, "Human cytogenetics: its present place and future possibilities," *Am. J. Human Genetics,* 1960, *12*: 104.

[4] J. Baker, *Cytological Technique* (London: Methuen, 1950), 1.

The First Human Chromosome Counts, 1890-1920

Human chromosomes were first observed in the 1870s when, due to the incorporation of staining and fixation into standard cytological procedure, chromosomes and the phases of mitosis in many species were brought to light. Human chromosomes were first counted in the 1890s after the recent emergence of the chromosome theory of heredity which created an intense interest in the number of chromosomes in cells. In the 1870s and early 1880s, cytologists concentrated on the behavior of chromosomes and evinced little interest in their number. The few chromosome counts attempted in the first half of the 1880s suggested that the number of chromosomes might be constant within a species, though different among species. But no one attached any significance to this apparent numerical constancy. In the second half of the 1880s, however, with the enunciation of the chromosome theory of heredity, chromosome number constancy assumed a much greater importance. Advocates of the "individuality" of chromosomes, like T. Boveri and C. Rabl, argued that the number of chromosomes in cells was constant because the chromosomes were persistent cell structures. They opposed the prevailing notion that the chromosomes were merely transitory structures which disaggregated into chromatic granules in the resting cell nucleus and re-formed anew from those granules at the beginning of each cell division. The support lent by the chromosome number constancy "law" to the embattled chromosome theory of heredity unquestionably stimulated cytologists to count the number of chromosomes in many species, including man.

From the 1890s through about 1920 human chromosome counts varied widely, from 8 to over 50, but the majority clustered around 24 in diploid cells and 12 in haploid cells.[5] Consequently most cytologists firmly believed human cells possessed 24 chromosomes and this number was incorporated into standard textbooks where, in some cases, it remained as late as 1930. It is essential to realize that low chromosome counts were characteristic for *all* mammalian species studied during this period.[6] Cytologists were unaware of the necessity for the *immediate* fixation of mammalian tissue removed from a *living* body to guarantee the preservation of chromosomes in a lifelike condition. Thus they were perfectly satisfied with material obtained from corpses, usually those of executed criminals. Unfortunately, in dead mammalian cells, chromosomes clump together extraordinarily rapidly.[7] In a sense, then, the low counts were not entirely inaccurate; however, the approximately 24 clumps counted were not single chromosomes. If one wishes to assess human chromosome counts prior to 1920, the "failure" of cytologists was not so much the incorrectness of their counts as their inability to appreciate the conditions for the lifelike preservation of chromosomes and the resulting disparity between chromosomes in their material and in living cells.

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[5] See the following tables for the counts of W. Flemming, J. Duesberg, M. Guyer, A. Branca, T. Montgomery, S. Gutherz, H. Jordan, H. Wieman, O. Grosser, and S. Schachow: K. Oguma and S. Makino, "A revised check-list of the chromosome number in vertebrata," *J. Genetics*, 1932, *26*: 253-254; S. Makino, *An Atlas of the Chromosome Numbers in Animals* (Ames: Iowa State College, 1951), 286-290.

The work of Hans de Winiwarter[8] was a notable exception during the first period of human chromosome counting. Winiwarter counted chromosomes in several mammalian species, including man, between 1900 and 1920; his counts alone came close to the actual number of chromosomes in living cells. Winiwarter succeeded where his contemporaries failed because of his exemplary technique. Winiwarter was the first to fully realize the necessity for immediate fixation of fresh tissue. Furthermore he insisted that the appropriate fixative mixture had to be selected for the faithful preservation of chromosomes. When his strictures were finally heeded by other cytologists in the 1920s they, too, began to make accurate mammalian chromosome counts.

Winiwarter was a student of the renowned cytologist E. von Beneden. Winiwarter began his study of mammalian chromosomes at the end of the 19th century. In an important paper published in 1900, he argued from his observations of rabbit oogenesis that chromosomes formed side-by-side pairs during the synapsis stage of the first maturation di-

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[6] The possibility has been raised several times that cytologists prior to 1920 mistook primary spermatocytes for spermatogonia so that their counts of about 24 chromosomes were accurate *haploid* counts. However, this is inconceivable since these same cytologists also made counts of about 12 chromosomes (in supposed haploid cells) which cannot possibly be rationalized as "correct". Beatty, *op. cit.* (n. 1 above), 181; Heberer, *op. cit.* (n. 1 above), 100; T. Painter, "Studies in mammalian spermatogenesis II. The spermatogenesis of man," *J. Exper. Zool.*, 1923, *37*: 313.

[7] R. Hance, "The fixation of mammalian chromosomes," *Anat. Record*, 1917, *12*: 371-387.

[8] Winiwarter was an ardent patriot who, after the German invasion of his homeland Belgium in World War I, changed his name from von to de Winiwarter. He also gave up the German language, which he had often used before the war, for publication. I Diller, personal communication.

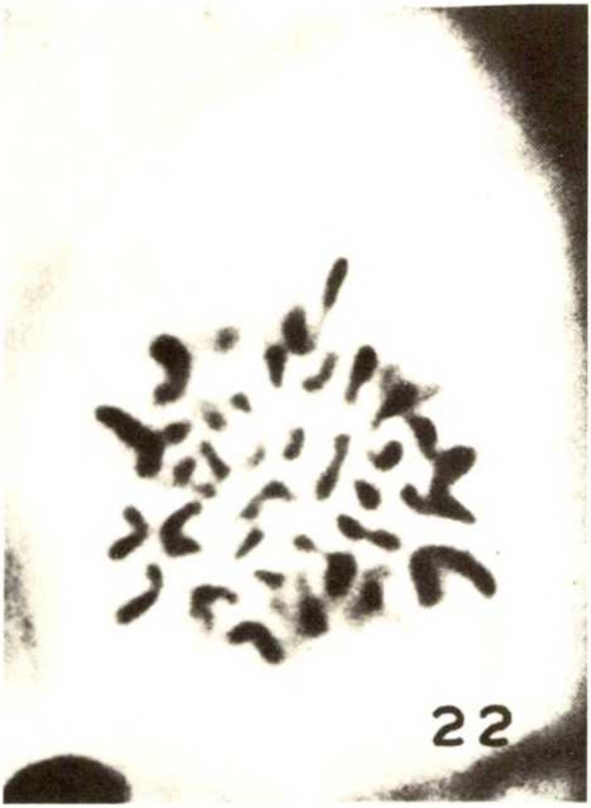

Fig. 2. Photomicrograph of a high quality section preparation of spermatogonial chromosomes. K. Oguma counted 47 "without the least uncertainty." (K. Oguma, *op. cit.* [1937, n. 25], 73 and Fig. 22.)

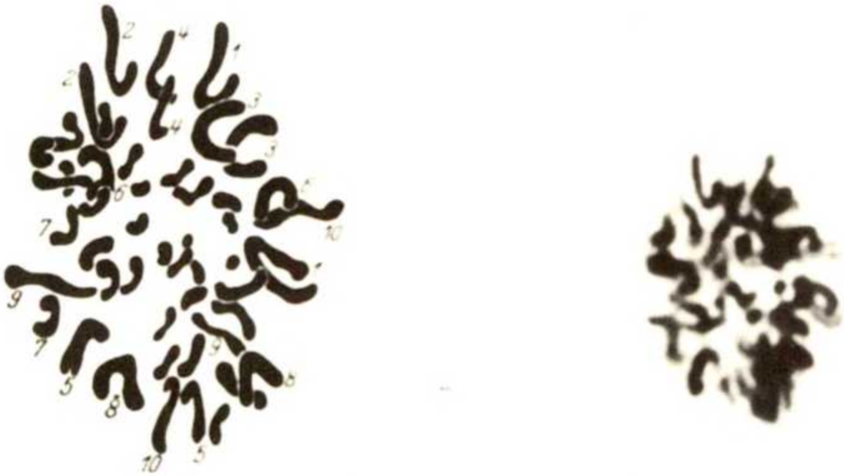

Fig. 3. Photomicrograph and drawing of an average quality section preparation of (48) spermatogonial chromosomes. (A. Andres and M. Navaschin, *op. cit.* [n. 29], Figs. 10 and 11.)

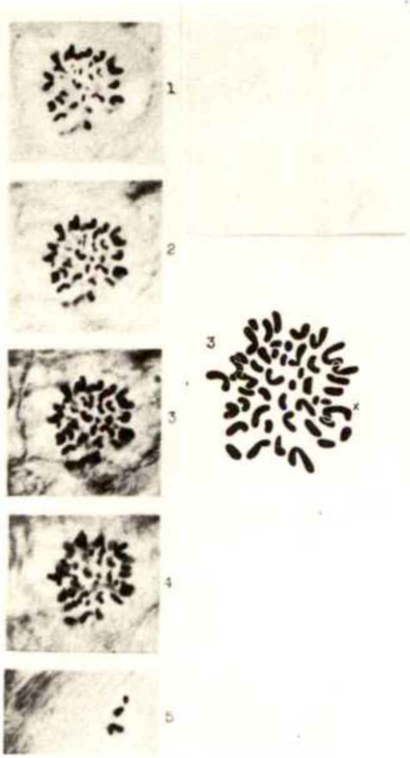

Fig. 4. Photomicrographs and drawing of section preparation of spermatogonial chromosomes. Photomicrographs 1-4 are four different focal planes of same section; photomicrograph 5 is the following section containing three additional chromosomes (or parts of chromosomes ?). K. Oguma and H. Kihara counted 47 chromosomes "with the greatest ease." This preparation was held up as one of the clearest examples of 47 chromosomes in spermatogonia. (K. Oguma and H. Kihara, *op. cit.* [n. 25], 497 and Pls. A and C; H. de Winiwarter and K. Oguma, *op. cit.* [n. 32], 547.)

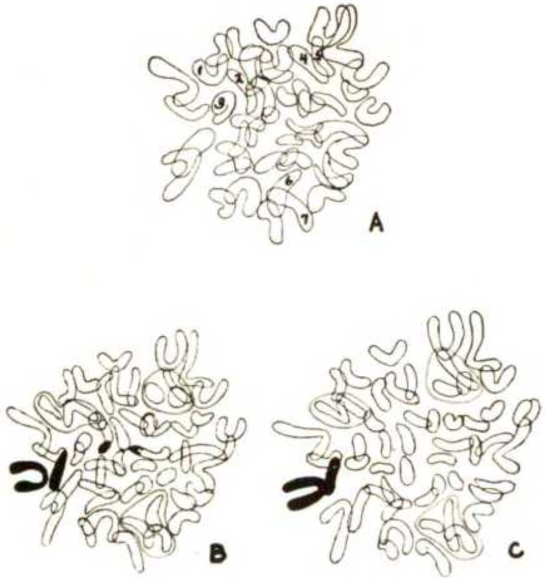

Fig. 5. Independent drawings of the same section preparation of lip carcinoma cell chromosomes by H. Evans(A), H. de Winiwarter(B), and K. Oguma(C). Evans counted 48 chromosomes, Winiwarter 54, Oguma 53. Chromosomes numbered in A are circled in B and C. See text for details. (R. King and H. Beams, *op. cit.* [n. 29], 169; originally published in H. de Winiwarter and K. Oguma, *op. cit.* [n. 32], 548-549.)

of dispute have been numbered in the figure. All three cytologists agreed that there were two chromosomes in area 1/2, but Evans and Oguma connected the chromosome arms one way and Winiwarter another. In area 3, Evans counted one chromosome while Winiwarter and Oguma counted two. In area 4/5, Evans counted two chromosomes while Winiwarter and Oguma counted three. In area 6/7, Evans counted two chromosomes while Winiwarter and Oguma counted three; but Winiwarter and Oguma then disagreed as to which arms belonged to which chromosomes. Winiwarter and Oguma included the two large blackened chromosomes in the preparation while Evans did not (he must have seen them but, for some reason, considered them extraneous). Lastly, Winiwarter included a small blackened chromosome that Oguma failed to see. In addition, even a cursory examination of the drawings reveals many lesser differences as to the positions and extents of overlap of the chromosomes.[32]

From their analysis of these drawings, R. King and H. Beams came to the conclusion that "diploid human cells in which the count may not vary from observer to observer, or even from time to time for the same observer, must therefore be extremely rare." [33] I fully concur with their judgment. Therefore, the certainty and confidence with which the proponents of either 47 or 48 chromosomes presented and defended their own counts was as marvelous as the accuracy of human chromosome counting in the 1920s and 1930s. Cytologists did not hesitate to acknowledge the many difficulties involved in counting, from the preparation of material to the proper interpretation of the preparations. The disputed counts of *other* cytologists were usually attributed to their failure to completely overcome all these difficulties. Either the chromosomes were so poorly fixed that meaningful counts and karyotypes were hopeless or the well-fixed chromosomes had been misinterpreted. Thus one cytologist saw 47 chromosomes where another saw 48 and one saw just an X where another saw both X and Y chromosomes.[34]

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[32] One of the most intriguing of these lesser differences is the consistently fatter chromosomes in Evans' drawing, resulting in increased chromosome overlap. Most cytologists considered Evans and Swezy's preparations to be of inferior quality, in part because of the fat forms and plentiful overlap of chromosomes in their drawings. Yet from Fig. 5 it seems as if Evans "naturally" drew chromosomes fatter and saw more overlap than other cytologists did. H. de Winiwarter and K Oguma, "La formule chromosomiale humaine (à propos de deux travaux récents)," *Arch. Biologie,* 1930, *40*: 544-551; King and Beams, *op. cit.* (n. 29 above), 168-170.

[33] King and Beams, *op. cit.* (n. 29 above), 170.

[34] For some representative examples, see Oguma, *op. cit.* (1930, n. 25 above), 212-213;

Such comments were invariably followed by the triumphant declaration that a few cells had at last been found in which chromosomes were so well fixed and dispersed that absolutely sure counts were finally possible. This certainty was unjustified. A count of 47 chromosomes was no more accurate than a count of 48 chromosomes or *vice versa.* Thus one might expect a cytologist to have reported 47 chromosomes one time and 48 another. But this never really happened. Winiwarter and Oguma insisted that in their best preparations there were always, in spermatogonia, 47 chromosomes and never a small unpaired Y and, in primary spermatocytes, 23 bivalents and a univalent, unpaired X. Yet at the same time, Painter and his followers contended that in their best preparations there were always, in spermatogonia, 48 chromosomes, including both X and Y, and, in primary spermatocytes, 24 bivalents, one of which was the unequal XY.[35]

The remarkable certainty and consistency in the counts of a given cytologist can be explained in only one way. I have emphasized that chromosome counting involved continuous subjective interpretation. This interpretation was not made in a vacuum. It was forcefully guided by preconception. Winiwarter's students, obviously under his influence, not unexpectedly confirmed his human chromosome counts and the presence of the XO/XX sex chromosome type in several mammalian species. However, Painter's authoritative chromosome studies in the 1920s were a very strong influence on all other cytologists in their counts of human chromosomes. Cytologists firmly expected to find 48 chromosomes and, in haploid cells, 24 chromosomes. Not surprisingly, in view of the trying technical circumstances, they found them. A

Oguma, *op. cit.* (1937, n. 25 above), 73-84; de Winiwarter and Oguma, *op. cit.* (n. 32 above), 550; Minouchi and Ohta, *op. cit.* (n. 29 above), 486-488; Painter, *op. cit.* (n. 21, above), 512; Heberer, *op. cit.* (n. 1, above), 114.

[35] Chromosomes were "completely separated from one another" (Kemp); there was "little or no overlapping or crowding" of chromosomes (Evans and Swezy); chromosomes were "seldom overlapping" (Oguma); preparations were "rejected *a priori* even if only one chromosome was doubtful" (Winiwarter and Oguma). As a result it was "not difficult to count accurately" (Oguma); karyotyping was "easily done without the slightest hesitation" (Winiwarter and Oguma). 48 chromosomes were counted "with great certainty" (Kemp); "we were not confronted with even slight variations or aberrations in chromosome numbers, e.g., 46, 47, or 49, but secured constantly 48 of these elements" (Evans and Swezy); "we have never come across a discordant number be it more or less [than 47], for example 46 or 48 or even 49 chromosomes" (Winiwarter and Oguma); 47 chromosomes were counted "without the least uncertainty" (Oguma); "the numerical agreement of a long series of observations . . . guarantees their exactness" (Winiwarter); counts were "incontestable" and it was "impossible" to come to any other conclusion (Winiwarter and Oguma). *Cf.* Matthey, *op. cit.* (1949, n. 1, above), 26.

close preconception-confirmation feedback relationship was established. Preconception led to confirmation; confirmation strengthened the underlying preconception; the strengthened preconception increased the likelihood of further confirmation which was, indeed, forthcoming.[36]

Cytologists were conscious of the influence of preconception but claimed they had avoided it. Thus Painter noted he had first drawn chromosomes "without a thought as to the number" and then counted them. Similarly, Winiwarter and Oguma emphasized that, in their collaboration, they had first, independently, drawn preparations and then, independently, counted them. Subsequently they found they were always in complete accord as to the presence of 47 chromosomes. Furthermore, Winiwarter and Oguma insisted that it was not because they had retained, as good preparations, only those cells with 47 chromosomes that they always arrived at the same count. Rather they had first chosen those preparations that were most favorable for counting and afterwards always discovered that all had the same number (47) of chromosomes. But R. Matthey, aware of the purely psychological elements in chromosome counting, observed that even this procedure was not proof against the effects of preconception.

> As soon as the researcher knows the haploid number of chromosomes, he no longer makes use of his critical powers. In vain will he take great care not to count the chromosomes as he draws them. He will try to force himself to think of something else, to whistle or to sing; by all these means he will do his best to be only a recording device immune to any preconceived idea. . . . But whatever he does, the total mounts unconsciously and stops at the precise moment when twice the haploid number is reached![37]

Since Winiwarter and Painter differed only with respect to the sex chromosome type in human cells, it is interesting to compare the origins of their two beliefs. Winiwarter made his first human chromosome counts in 1912 when there was little knowledge of the sex chromosome type in mammals. Only three years before (1909) he had been among the first to identify sex chromosomes in vertebrate cells. At the time the most common sex chromosome type, known for about a decade, was the XO/XX in insects. This fact may have influenced Winiwarter in his observations. Once he found this type in human cells he also found it in the cells of other mammalian species. To the cytologist Matthey (1936), "it [was] very natural that Winiwarter who, for so long, was right while others were wrong, [remained] convinced of his original opinions." [38] Painter, on the other hand, had the good fortune to begin his study of mammalian chromosomes with the opossum in which the XY/XX sex chromosome type was indisputable. Properly prepared, Painter readily identified X and Y chromosomes in human cells, especially primary spermatocytes. In fact, "in . . . placing the full diploid number at 48, [Painter] was influenced, not so much by spermatogonia counts, as by the conditions [sex chromosomes] observed in the first maturation division." [39]

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[36] The preconception extended to counts of chromosomes in giant germ cells. From approximate counts, these cells were apparently tetraploid. Thus cytologists expected to find $2 \times 48 = 96$ chromosomes in giant spermatogonia and $2 \times 24 = 48$ bivalents in giant spermatocytes. Indeed they counted 96 and 48 chromosomes in giant germ cells and took these tetraploid counts to be further confirmation of the correctness of their diploid and haploid counts of 48 and 24 chromosomes. A. Andres, "Die Riesenzellen in der Spermatogenese des Menschen," *Ztschr. f. Zellforschung u. mikroskopische Anatomie,* 1933, *18*: 420-424, 432; Heberer, *op. cit.* (n. 1 above), 125-126.

[37] Painter, *op. cit.* (n. 24 above), 440; de Winiwarter and Oguma, *op. cit.* (n. 15 above), 109; H. de Winiwarter and K. Oguma, "Recherches sur quelques points controversés de la spermatogenèse humaine," *Comptes Rendus de Association des Anatomistes,* 1925 (Turin), 408; Matthey, *op. cit.* (1949, n. 1 above), 27.

Since the belief in the presence of X and Y chromosomes in mammalian cells proved to be right, it has generally been assumed that the identifications of those chromosomes in the 1920s and 1930s were correct. In addition, if those who counted 47 chromosomes just overlooked the small Y, then all cytologists should have agreed in the identification of the X. However, in the 1920s and 1930s, there was considerable disagreement over which chromosomes were the X and Y. Those who counted 48 chromosomes agreed the X was of medium length but then disagreed with respect to its form—rodlike or U-shaped. Furthermore, Minouchi and Ohta (whom Matthey believed were the first to have seen the actual X and Y in human cells) disputed Painter's identification of the Y chromosome. There was no small unpaired chromosome in their preparations.[40] Those who counted 47 chromosomes believed the X was large but then disagreed whether or not it was the largest chromosome. Initially Oguma argued the X was the largest but later, in collaboration with Winiwarter, he revised his karyotypes and came to the new conclusion that the X was only one of the larger chromosomes. Nevertheless, a few years afterwards Oguma returned to his original position that the X was, after all, the largest chromosome.[41] Clearly the identification of the sex chromosome(s), like the counting of chromosomes, was a subjective matter. Agreement as to the number and sizes of sex chromosomes was as much the result of preconception as objective observation.[42]

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[38] Matthey, *op. cit.* (n. 18 above), 353; R. Matthey, *op. cit.* (1949, n. 1 above), 109.

[39] Painter, *op. cit.* (n. 21 above), 507. It is instructive to compare Painter's observations on sex chromosomes in a less favorable species. When Painter began his study of lizard chromosomes in the late 1910s, the most common sex chromosome type in vertebrates was the XX O /XX XX (which, incidentally, Painter had already identified in the spider). Since sex chromosomes are obscure in reptiles, it is not surprising that Painter found this compound X sex chromosome type that, at the time, appeared general in all vertebrates and that he had already seen in the spider, in the lizard. T. Painter, "Studies in reptilian spermatogenesis. I. The spermatogenesis of lizards," *J. Exper. Zool.* 1921, *34*: 308-309; Matthey, *op. cit.* (1949, n. 1 above), 48-49, 85-97.

[40] Minouchi and Ohta, *op. cit.* (n. 29 above), 486-487; Matthey, *op. cit.* (n. 18 above), 340-341, 374-376; T. C. Hsu, "Mammalian chromosomes *in vitro* I. The karyotype of man," *J. Heredity,* 1952, *43*: 172. We now know that the Y chromosome is especially variable in

With the discovery in 1959 that certain abnormal individuals do possess 48 chromosomes, the suggestion was made that perhaps Painter's chromosome counts had been correct since his material, at least, had come from patients at a mental institution.[43] However, with our present knowledge, this possibility can be eliminated. Spermatogenesis in an individual with 48 chromosomes would proceed abnormally. Since one or both of the extra chromosomes would undoubtedly be a sex chromosome, a trivalent or quadrivalent of the sex chromosomes would form and spermatogenesis would eventually break down. But Painter described just normal spermatogenesis with a normal XY bivalent.

Another attempt to account for the counts of 48 chromosomes in a systematic fashion by some "persistent error" deserves discussion. Ford and Hamerton have made three such suggestions: (1) the precocious disjunction of the XY bivalent in primary spermatocytes (which definitely occurs) would leave 22 autosomal bivalents and two univalents that might be mistaken for bivalents to give a total of 24 bivalents; (2) the terminal loop of a large bivalent with several chiasmata could be mistaken for a 24th bivalent; and (3) the arms of longer chromosomes with their elongated centric regions could be interpreted as separate chromosomes. One such error would give a count of 47 chromosomes; two errors a count of 48.[44] There seems to be no doubt that

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length. It is indeed longest in Japanese populations. Hamerton, *op. cit.* (n. 1 above), I, 92-94.

[41] Oguma and Kihara, *op. cit.* (n. 25 above), 498; de Winiwarter and Oguma, *op. cit.* (n. 15 above), 111-112; Oguma, *op. cit.* (1930, n. 25 above), 208-211

[42] Examination of *photomicrographs* of the presumed XY bivalent reinforces this feeling. See Minouchi and Ohta, *op. cit.* (n 29 above), fig. 15b; Shiwago and Andres, *op. cit* (n. 21 above), fig. 13; King and Beams, *op. cit.* (n. 29 above), Pl. 1; Koller, *op. cit.* (n. 29 above), Pls. 1 and 2.

[43] Chu, *op. cit.* (n. 1 above), 97; Hamerton, *op. cit.* (n. 1 above), I, 4.

[44] Ford and Hamerton, *op. cit.* (n. 1 above), 1021-1023.

the third suggestion does apply to T. C. Hsu's interesting chromosome counts in the early 1950s (see below p. 492), while the precocious disjunction of a small autosomal bivalent might explain M. Kodani's counts of 24 bivalents *after* Tjio and Levan's discovery. But these suggestions are only applicable to otherwise clear and distinct chromosome preparations characteristic of the 1950s. In the 1920s and 1930s, in contrast, the preparations were anything but clear and distinct. One does not have to look beyond the crowded, overlapping, sometimes cut chromosomes and the effects of preconception to account for the counts of 47 and 48 chromosomes.

The certainty of cytologists by the late 1930s that human cells possessed 48 chromosomes is reflected in the paucity of published chromosome counts in the 1940s and early 1950s. In this period there was not one study of the number of human chromosomes comparable in scope to the numerous studies of the 1920s and 1930s.[45] Cytologists simply had no reason to devote all the time necessary for making any more, decisive counts. The few incidental counts that were made all confirmed the accepted existence of 48 chromosomes in human cells.[46]

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[45] A possible exception might be the work on somatic inconstancy or aneuploidy. In the 1920s and 1930s, some cytologists felt that the number of chromosomes might not be absolutely constant in *somatic* cells. Around 1950 S. Timonen took up this question again: "A survey of the studies made on the somatic chromosomes in man provides a good example of how obvious facts may be overlooked in a case in which the technique is not satisfactory. Questions of authority, and even pure chance, have instead of facts, settled various questions for a long period, and thus led investigators astray. Finally, the erroneous results have been established in text books as unquestioned facts doubted by nobody." Using the new squash technique, Timonen and E. Therman found a huge range of variability. However, they, too, were not entirely free from preconception as they found peaks in their counts at 24 and 48 chromosomes. Furthermore, their counts were the result of unsatisfactory technique, too heavy squashing. Present opinion is that in all somatic tissues, aneuploidy amounts to only about 10%. S. Timonen, "Mitosis in normal endometrium and genital cancer," *Acta Obstetrica et Gynecologica Scandinavica,* 1950, *31,* Suppl. 1: 9; E. Therman and S. Timonen, "Inconstancy of the human somatic chromosome complement," *Hereditas,* 1951, *37*: 266-279; Beatty, *op. cit.* (n. 1 above), 182-183; T. C. Hsu and C Pomerat, "Mammalian chromosomes *in vitro* III. On somatic aneuploidy," *J. Morphology,* 1953, *93*: 316-317; H. Kava and H. Klinger, "Chromosomes of human endometrial cells I. *In vitro* studies from normal tissues of healthy women," *Cytogenetics,* 1970, *9*: 199-211.

[46] L. LaCour, "Mitosis and cell differentiation in the blood," *Proceedings of the Royal Society of Edinburgh,* 1944, *62B*: 75; B. Slizynski, "Human chromosomes," *Nature,* 1945, *155*: 427 (see Beatty, *op. cit.* (n. 1 above), 182); C. Barigozzi, "Les chromosomes humains dans quelques états pathologiques," *Arch. d. Julius Klaus-Stiftung f. Vererbungsforschung Sozialanthropologie u. Rassenhygiene,* 1947, *22*: 343; A. Severinghaus, "Sex chromosomes in a human intersex," *Am. J. Anatomy,* 1942, *70*: 75-82; L. Sachs, "The chromosome constancy of the normal mammalian uterus," *Heredity,* 1954, *8*: 119; G. Manna, "Chromosome number of human endometrium," *Nature,* 1954, *173*: 271; A. Gopal-Ayengar in C. D.

The Technical Revolution in Mammalian Cytology and the Discovery of 46 Chromosomes in Human Cells, Early 1950s

Mammalian cytology underwent a technical revolution in the early 1950s. The Tjio and Levan discovery was the direct and inevitable result of the *combined* use of four new preparative techniques in mammalian cytology. Each of the new techniques—tissue culture, hypotonic pretreatment, colchicine pretreatment, and squashes—contributed to the dispersal of chromosomes and the reduction of chromosome overlap. The net effect of their combined use was to overcome the inherent difficulty in human chromosome counting. I have already argued that section methods accentuated that difficulty. In addition, due to the two new techniques of tissue culture and colchicine pretreatment, cytologists now possessed a wealth of material for counts as compared to the scarcity of material in the previous period.

The new technique, replacing the former, elaborate and tedious section technique, was simple. Cells were grown in tissue culture—that is, outside the body (*in vitro*) on a suitable nutrient medium. The growing cells were pretreated with a dilute colchicine solution several hours before fixation. Several minutes before fixation, they were also pretreated with a hypotonic solution. The cells were then fixed, with a simple fixative such as a mixture of alcohol and acetic acid, and stained. Immediately thereafter the cells were squashed with a coverslip and a thumb.

Cells in tissue culture are unusually large, providing a substantial volume for the dispersal of chromosomes by the other new techniques. They are also actively dividing, providing many opportunities for chromosome counting. Hypotonic pretreatment swells cells and so, when applied to tissue culture cells, increases the already enlarged cell volume for maximum chromosome dispersal. More importantly, hypotonic pretreatment "dissolves" the cell spindle to which the chromosomes are attached, thereby liberating them into the very large volume simultaneously created. Colchicine pretreatment inhibits the formation of a spindle, freeing chromosomes to spread throughout the whole cell. In the process it necessarily blocks the completion of mitosis and accumulates many cells at metaphase for chromosome counting. Colchicine

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Darlington, *The Facts of Life* (New York: Macmillan, 1953), Pl. IX; P. Koller. "Chromosome behavior in tumors: readjustments to Boveri's theory," in *Cell Physiology of Neoplasia* (Austin: University of Texas, 1960), 27. For the counts of Hsu, C. D. Darlington and A. Haque, and M. Kodani, see below.

pretreatment also contracts chromosomes, decreasing considerably the chance of chromosome overlap. Moreover it both sharpens chromosome outline and accentuates the centromere's position, making possible the precise karyotyping of cells. Lastly, squashes have the obvious effect of separating crowded chromosomes.

Two things must be stressed about the new techniques—the necessity for their *combined* use and the fact that they were not new at all in the early 1950s but only new to the vast majority of mammalian cytologists. I shall discuss this second fact in the following section. The combined use of the four new techniques was critical because only when all were used together were chromosome preparations so clear that the preconception that human cells possessed 48 chromosomes could be overcome. Before all four techniques were combined they were used singly or in *partial* combination with fascinating results. As early as the 1920s and 1930s some cytologists were dissatisfied with sections, especially for somatic cell counts, and turned to unsectioned tissue culture and membrane preparations. Yet Kemp, and Andres and Navaschin still counted 48 chromosomes in human cells (see below p. 498). Squashes were first used on human cells in the 1940s but, as with the first use of tissue culture, 48 chromosomes were still counted. As late as 1954, C. D. Darlington and A. Haque relied on simple squashes for spermatogonial counts in the rhesus monkey and man. They succeeded in correcting Painter's inaccurate count of 48 chromosomes in the monkey; but they still found Painter's 48 chromosomes in man (see below n. 50).[47] Though squashes were much more convenient than sections, by themselves they did not represent a sufficient improvement to overcome the preconception in favor of 48 chromosomes.

The use of partial combinations of the new techniques had the same result. The most interesting such case was the 1952 study of somatic cell chromosomes by T. C. Hsu. Hsu revived the tissue culture technique occasionally employed in the 1920s and 1930s. Quite accidentally, he introduced the hypotonic pretreatment of cells. He had intended to wash the cells prior to fixation with an isotonic solution. Apparently the balance was misread and a hypotonic solution was prepared and used instead. Hsu found the chromosomes were beautifully dispersed. Eventually he was able to trace the well-spread metaphases to the accidental use of the hypotonic pretreatment which prompted Hsu to

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[47] C. D. Darlington and A. Haque, "Chromosomes of monkeys and men," *Nature,* 1955, *175*: 32.

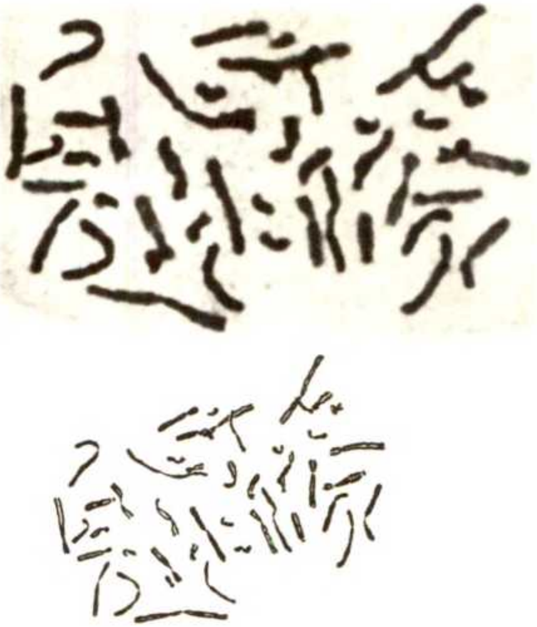

Fig. 6. Photomicrograph and drawing of preparation of hypotonic pretreated spleen cell chromosomes in tissue culture. In 1952, T. C. Hsu counted 48 chromosomes, but, in 1956, J. H. Tjio and A. Levan claimed a count of 46 chromosomes in this preparation more likely. Prior to 1956, this was considered the finest published human chromosome preparation. (T. C. Hsu, *op. cit.* [n. 40], Figs. 11c and 13c, photomicrograph taken from C. D. Darlington, *op. cit.* [n. 46], Pl. 8; J. H. Tjio and A. Levan, *op. cit.* [n. 51], 5.)

later remark, "it only shows that to achieve some discovery, sloppy technicians are occasionally helpful."[48] Prior to 1956 it was commonly felt that Hsu's human chromosome preparations were the best (Fig. 6). Nevertheless in over 70% of the cells counted, Hsu reported 48 chromosomes. Hsu has admitted he firmly believed human cells possessed 48 chromosomes and tried very hard to find 48 chromosomes in each cell. If necessary, he reinterpreted long chromosomes as two shorter chromosomes aligned end-to-end in order to raise lower counts to the expected 48. The fact that Hsu was working at the time at the University of Texas, where none other than Painter was then President, did not help to free Hsu from his preconception. Hsu has claimed that had he not approached his preparations with a preconception of how many chromosomes he should find, he would have discovered, in 1952, that human cells possess 46 chromosomes.[49] This is believable since in about 10% of the cells he did find (and report) 46 chromosomes which simply could not be reinterpreted as 48. Furthermore, after Tjio and Levan broke the hold of the preconception, Hsu made new preparations, in 1957, *using exactly the same technique he had in 1952,* and now counted 46 chromosomes. Thus his procedure of tissue culture plus hypotonic pretreatment was sufficient, though not ideal, *in 1957* to count 46 chromosomes but, using only two of the four new techniques, it was not strong enough, *in 1952,* to overcome the preconception that human cells possessed 48 chromosomes.[50]

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[48] T. C. Hsu, "Chromosomal evolution in cell populations," *Internat. Rev. Cytology*, 1961, *12*: 81; Hsu, *op. cit.* (n. 40 above), 172.

[49] T. C. Hsu, personal communication; Hsu, *op. cit.* (n. 40 above), 171.

There is one more illuminating example of the remarkable strength of the reigning preconception. In 1954, three colleagues of Tjio and Levan in Sweden undertook chromosome counts in embryonic liver cells, using just simple squashes. Much to their amazement, they found that the cells possessed only 46 chromosomes, and chose to abandon their study just because they had failed to find the expected, normal number of 48 chromosomes.[51]

To my knowledge the first account of the application of both hypotonic and colchicine pretreatments to mammalian cells was published in 1955 by D. Hungerford who employed both pretreatments after simple squashes had failed to disperse mouse chromosomes sufficiently.[52] During 1955, Ford and Hamerton developed a colchicine, hypotonic, squash sequence for the preparation of mammalian chromosomes which they applied to mouse cells in order to detect the presence or absence of a marker chromosome and make precise chromosome counts.[53] They personally communicated this new technique to Tjio. Independently, Levan, while visiting at the Sloan Kettering Institute in the summer of 1955, combined all four new techniques. In the early 1950s there was a renewed interest in the long-held suspicion that cancer was the consequence of an abnormal chromosome constitution. In 1955, to study the chromosomes in tissue-cultured human cancer cells, Levan first applied the simple squash technique. The chromosomes were clumped. On the advice of J. Biesele, on the next day Levan applied hypotonic pretreatment before squashing the cells and found some metaphases that were "almost analyzable." The following day he pretreated cells with both colchicine and hypotonic solutions and found many cells with good and analyzable chromosomes.[54] Before his return to Sweden at the end of the summer, Levan also applied these new techniques to some tissue culture lines of normal human cells. In nearly all cases the number of chromosomes was very high, which is not surprising in view of the old age of the cell lines. In one case Levan counted 48 chromosomes which, of course, he took to be the unchanged diploid number.[55] In order to make a control karyotype it was apparently necessary to work with normal cells that had been in tissue culture for just a short time.

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[50] Similarly, in 1962, Darlington and Haque, using the same simple squash technique as they had in 1954, *now* counted 23 bivalents in primary spermatocytes. T. C. Hsu, C. Pomerat, and P. Moorhead, "Mammalian chromosomes *in vitro*. VIII. Heteroploid transformation in the human cell strain Mayes," *J. National Cancer Institute,* 1957, *19*: 868; C. D. Darlington and A. Haque, "Polyploidy with chromosome breakage at meiosis in the human male," *Cytogenetics,* 1962, *1*: 196.

[51] A. Levan, personal communication. J. H. Tjio and A. Levan, "The chromosome number of man," *Hereditas,* 1956, *42*: 5.

[52] D. Hungerford, "Chromosome numbers in ten-day fetal mouse cells," *J. Morphology,* 1955, *97*: 499-501.

[53] C. Ford and J. Hamerton, "A colchicine, hypotonic citrate, squash sequence for mammalian chromosomes," *Stain Technology,* 1956, *31*: 247-251.

In December 1955 Tjio and Levan had the opportunity to study such cells. It must be remembered that they were as convinced as everyone else that human cells possessed 48 chromosomes. Their study was not motivated by even the slightest doubt about the accepted number of chromosomes in human cells. They were interested in the morphology, not the number, of chromosomes. Though Tjio and Levan expected to find 48 chromosomes and were therefore surprised to find just 46, in retrospect it is no real surprise that when all four new techniques were first applied to normal human cells, 46 chromosomes were immediately and unequivocally counted. Under these new technical circumstances, there was no need for interpretation and therefore no chance for preconception to exert its long-felt effect; 46 chromosomes *had* to be counted (Fig. 7).

Tjio and Levan considered, but rejected as very unlikely, the possibility that in the formation of lung tissue two chromosomes were systematically excluded. Tjio and Levan concluded their short but momentous 1956 paper, "before a renewed careful control has been made of the chromosome number in spermatogonial mitoses of man we do not wish to generalize our present findings into a statement that the chromosome number of man is $2n = 46$, but it is hard to avoid the conclusion that this would be the most natural explanation of our observations."[56]

Since all four techniques were applicable only to somatic cells (tissue culture and colchicine pretreatment cannot be applied to human germ

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[54] A. Levan, personal communication.

[55] A. Levan, "Chromosome studies on some human tumors and tissues of normal origin, grown *in vivo* and *in vitro* at the Sloan-Kettering Institute," *Cancer,* 1956, *9*: 657 and fig. 5b.

[56] Tjio and Levan, *op. cit.* (n. 51 above), 5.

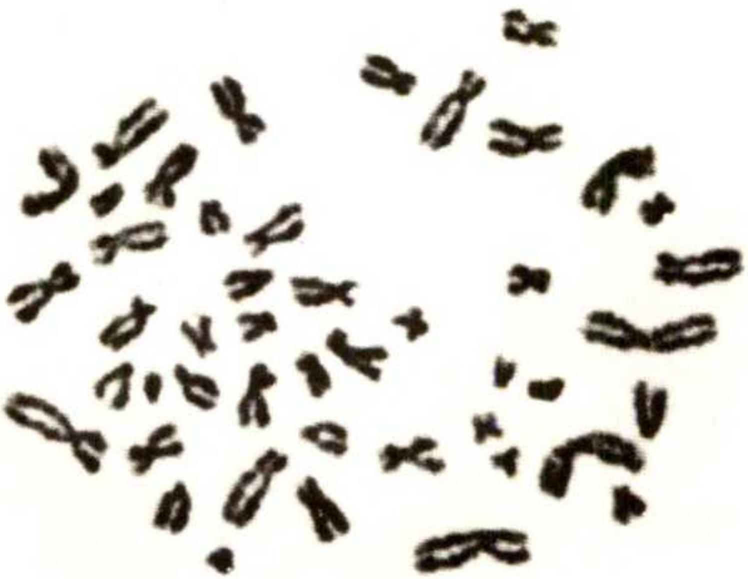

Fig. 7. Photomicrograph of squash preparation of colchicine and hypotonic pretreated lung cell chromosomes in tissue culture. J. H. Tjio and A. Levan conclusively counted 46 chromosomes. (J. H. Tjio and A. Levan, *op. cit.* [n. 51], Fig. 1a.)

cells), it is hardly accidental that the discovery of the actual number of chromosomes in man was made with somatic cells. Other cytologists, despite their total surprise at Tjio and Levan's "very unexpected finding," were able to quickly confirm those counts in somatic cells when they also used Tjio and Levan methods.[57] It was more difficult to count germ cell chromosomes. When they learned of Tjio and Levan's counts, Ford and Hamerton undertook the necessary study of the number of chromosomes in spermatogonia and spermatocytes and confirmed the new counts (Fig. 8).[58] But before they could report this confirmation (November 1956) M. Kodani announced his counts, based on several years of study, of 48 chromosomes in spermatogonia and 24 bivalents in primary spermatocytes (September 1956). These counts were made without any knowledge of Tjio and Levan's results. Having learned of those counts, Kodani returned to germ cells to test them. *Now* he

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[57] I have found at least thirteen papers, published in 1957-1959, based on material from over one hundred individuals, which contain approximately five thousand counts of 46 chromosomes.

[58] Ford and Hamerton, *op. cit.* (n. 1 above), 1020-1023.

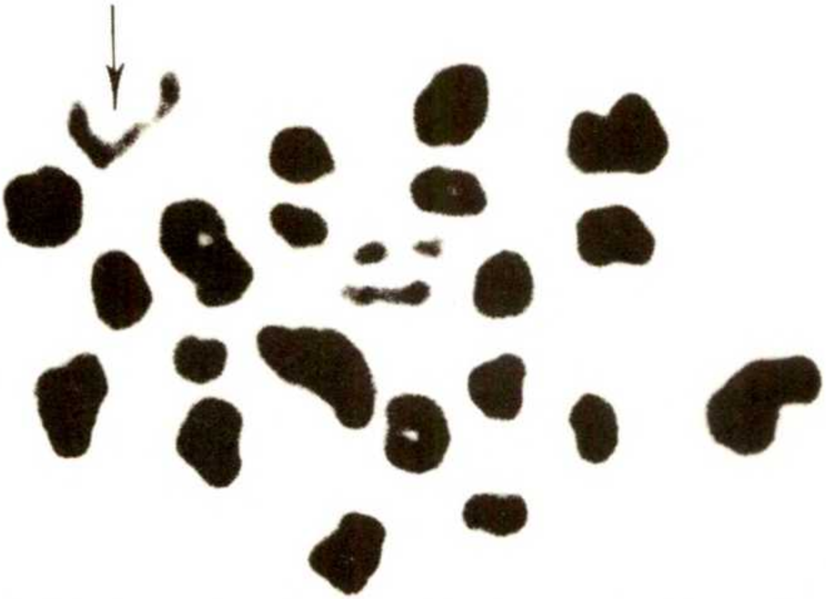

Fig. 8. Photomicrograph of squash preparation of hypotonic pretreated primary spermatocyte chromosomes. Arrow indicates the unequal XY bivalent. C. Ford and J. Hamerton counted 23 bivalents, confirming Tjio and Levan's discovery. (C. Ford and J. Hamerton, *op. cit.* [n. 1], Fig. 1.)

found both individuals with 48 and 46 chromosomes. Kodani realized there should, then, be other individuals with 47 (24 + 23) chromosomes and eventually he found one of them, too. Judging from Kodani's photomicrographs, his counts do represent the most convincing evidence of the presence of 48 chromosomes in man. Therefore it is ironic that Kodani's counts were reported *after* Tjio and Levan's discovery and the abandonment of the old belief in 48 chromosomes by all other cytologists. Kodani's counts in germ cells have never been confirmed.[59]

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[59] M. Kodani, "The karyotype of man with the diploid chromosome number of 48," *Proceedings of the International Genetics Symposia,* 1956 (Tokyo), 103; M. Kodani, "The supernumerary chromosomes of man," *Am. J. Human Genetics,* 1958, *10*: 125-139. In an acknowledgment appended to this 1958 paper, Kodani wrote, "Professor Theophilus S. Painter of the University of Texas, kindly communicated to the author [Kodani] his opinion regarding the cause of the reduction of the chromosome number from 48 to the lower numbers in man, for which the author wishes to express sincere appreciation." I do not know that opinion but it would appear that Painter continued to believe for some time after 1956 that some individuals possessed 48 chromosomes. Kodani still feels his counts of 47 and 48 chromosomes were correct. M. Kodani, personal communication.

The Background to the Technical Revolution in Mammalian Cytology.

Reflecting back on the period before 1951, Hsu has observed, "karyology of mammals was one of the dark alleys in nuclear cytology. . . . Most cytologists shied away from this field and considered it unexplorable."[60] The technical revolution in mammalian cytology in the early 1950s immediately changed this situation and created a field fruitful for exploration. As I have already noted, however, the new techniques which brought about this transformation were not new at all. All were actually old techniques which could have been combined many years before they were. But for reasons of ignorance, misconception, and self-satisfaction with familiar, trusted methods, they were not.

The role of ignorance is illustrated by the history of hypotonic pretreatment. From about 1910 there was continuous interest in the effect of hypotonicity on mitosis. Working in this tradition, the cytologists E. Slifer, M. Lewis, and E. Aisenberg observed, in the 1930s, the dispersal or scattering effect of hypotonicity on chromosomes and noted its potential value for chromosome counting. But since these cytologists were not chromosome counters, their observations were contained in papers that were understandably overlooked by the cytologists who were counting human chromosomes. The dispersal effect of hypotonicity on chromosomes was suddenly rediscovered, simultaneously and independently, in the early 1950s by Hsu, S. Makino and I. Nishimura, A. Hughes, and O. Venge. In at least three of these cases the rediscovery was completely accidental. This accidental (re)discovery was the single most important of the four new techniques in the transformation of mammalian cytology.[61]

The history of colchicine pretreatment reveals the role of misconception. The disruptive effect of colchicine on mitosis was well known by the 1930s. The colchicine boom began in the late 1930s with the discovery (the subject of a full-fledged priority dispute) that colchicine could induce polyploidy in plant cells.[62] Levan was among the first to exploit the effects of colchicine on plant chromosomes. However, most mammalian cytologists believed colchicine clumped, rather than dispersed, mammalian chromosomes. Thus they consciously avoided using it. Their misconception was not without empirical foundation. High concentrations of colchicine are necessary to block mitosis in plant cells, whereas low concentrations suffice in mammalian cells. Since cytologists first discovered the great value of colchicine for chromosome study in plants, they naturally tended to use the same high concentrations for mammals. At high colchicine concentrations, chromosomes in mammalian cells do indeed clump. Hsu, for instance, did not combine colchicine with his accidentally-discovered hypotonic pretreatment because he believed that colchicine would clump human chromosomes.[63] Had he combined the two pretreatments in 1952 he probably would have become convinced that human cells possess only 46 chromosomes. During the 1940s and early 1950s, colchicine had been commonly and successfully applied to mammalian ascites tumor cells. But this work was in the mainstream of cancer research, rather than mammalian cytology, hence it was unfamiliar to many mammalian cytologists. However, because of their interest in the relationship between chromosomes and cancer, Tjio and Levan were familiar with this research and naturally used colchicine in their study of mouse and rat tumor cells, then human tumor cells, and lastly normal human cells.[64]

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[60] Hsu, *op. cit.* (n. 48 above), 80-81.

[61] E. Slifer, "Insect development VI. The behavior of grasshopper embryos in anisotonic, balanced salt solutions," *J. Exper. Zool.*, 1934, *67*: 153; M. Lewis, "Reversible solation of the mitotic spindle of living chick embryo cells studied *in vitro*," *Arch. f. experimentelle Zellforschung besonders Gewebezüchtung (Explantation)*, 1934, *16*: 161-162; E. Aisenberg, "De l'effet de l'hypo et de l'hypertonie sur les mitoses," *Bull. d'histologie appliquée à la physiologie et à la pathologie et de technique microscopique*, 1935, *12*: 100-102, 106-107; Hsu, *op. cit.* (n. 40 above), 172; S. Makino and I. Nishimura, "Water-pretreatment squash technic: A new and simple practical method for the chromosome study of animals," *Stain Technology*, 1952, *27*: 1-7; A. Hughes, "Some effects of abnormal tonicity on dividing cells in chick tissue cultures," *Quart. J. Microscopical Science*, 1952, *93*: 207-220; O. Venge, "A simplified method for spreading the chromosomes in the rabbit blastocyst," *Nature*, 1954, *174*: 608; O. Venge, personal communication.

Finally, the self-satisfaction with familiar methods is apparent from the histories of tissue culture and squashes. The unsatisfactory nature of sections was first clear from the study of somatic cells, which are smaller than germ cells and consequently have especially crowded chromosomes. Kemp could only find about 24 chromosomes in sections of somatic cells. Seeking a better method to prepare somatic cell chromosomes, Kemp turned to tissue culture, a technique which R. Hance had recently employed to study chicken chromosomes. Kemp recognized that tissue culture possessed several advantages. Somatic cells divide infrequently in the adult living body but divide actively *in vitro*. Somatic cells in tissue culture are much larger than normal. There was no doubt that all chromosomes were present since none could be lost or cut as with sections. Chromosome overlap was reduced because tissue culture cells grow in a single, flattened layer. Though Kemp still counted 48 chromosomes, his preparations were a definite advance. Andres and Navaschin followed Kemp's lead. They, too, continued to count 48 chromosomes; but, due to the clearness of their tissue culture preparations, they were able to describe chromosome morphology in greater detail than anyone else. In fact, their descriptions of the ten largest pairs of chromosomes were virtually identical to presently accepted ones. They even identified "satellites" on some chromosomes (Fig. 9). Despite this success of Kemp, and of Andres and Navaschin, the use of tissue culture for human chromosome study was abandoned for over fifteen years.[65] Hsu revived the tissue culture technique in the early 1950s in order to obtain a complete idiogram of all (24) pairs of human chromosomes.

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[62] J. Krythe and S. Wellensiek, "Five years of colchicine research," *Bibliographia Genetica,* 1942, *14*: 3-5; H. Dermen, "Colchicine polyploidy and technique," *Botanical Review,* 1940, *6*: 600-603; L. Havas, "A colchicine chronology," *J. Heredity,* 1940, *31*: 115-117.

[63] Hsu and Pomerat, *op. cit.* (n. 45 above), 304.

[64] A. Levan, "Colchicine-induced C-mitosis in two mouse ascites tumours," *Hereditas,* 1954, *40*: 1-64; J. H. Tjio and A. Levan, "Chromosome analysis of three hyperdiploid ascites tumors of the mouse," *Lunds Universitets Årsskrift,* N.F., Avd. 2, *50*: 1-38.

In the 1920s J. Belling introduced the aceto-carmine squash method to plant cytologists, for whom it soon became a standard technique. Squashes were known to cytologists counting human chromosomes in the 1920s and 1930s, but they were rejected untried. This reaction reflected absolute confidence in the accuracy of counts based on the use of tried-and-true sections. Belling himself had applied the squash technique to human tumor cells. I suspect his admission of uncertainty in chromosome counts (between 40 and 50) was considered reason enough by other cytologists for rejecting the squash method sight unseen and untried.[66] Fortunately this rejection did not last for long. Squashes were

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[65] Kemp, *op. cit.* (n. 29 above), 4-7; Andres and Navaschin, *op. cit.* (n. 29 above), 415-420. G. Chrustschoff used tissue-cultured leucocytes in the 1930s for chromosome study. He even made one count of 52 chromosomes. Since 1960, leucocytes from peripheral blood have become a common source of mitoses for human chromosome study. Normally, such cells rarely divide; but the addition of phytohemagglutinin to leucocytes in tissue culture initiates active mitosis. Thus it is interesting to observe that the discovery of this stimulus effect was accidental. P. Nowell initially used phytohemagglutinin simply to separate leucocytes from whole blood (by coagulating erythrocytes) in order to prepare cultures. After a few days he noticed the many, unexpected mitoses which he was able to attribute to the action of phytohemagglutinin. G. Chrustschoff, "Cytological investigations on cultures of normal human blood," *J. Genetics*, 1935, *31*: 255-259; P. Nowell, "Phytohemagglutinin: An initiator of mitosis in cultures of normal human leukocytes," *Cancer Research*, 1960, *20*: 462.

[66] J. Belling, "On counting chromosomes in pollen mother cells," *Am. Naturalist*, 1921, *55*: 573-574; J. Belling, "The iron-acetocarmine method of fixing and staining chromosomes,"

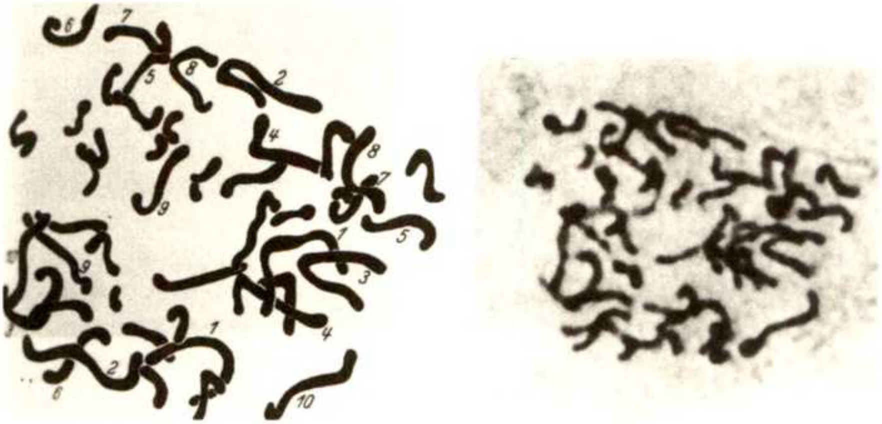

Fig. 9. Photomicrograph and drawing of preparation of lung cell chromosomes in tissue culture. This is perhaps the clearest published photomicrograph of human chromosomes from the 1920s and 1930s; it enabled A. Andres and M. Navaschin to precisely describe the ten largest homologous chromosomes (numbered) and identify the satellite on number 9. (A. Andres and M. Navaschin, *op. cit.* [n. 29], Figs. 1 and 2.)

adopted in the 1930s by cytogeneticists to study the giant salivary gland chromosomes of insect larvae to demonstrate cytologically the chromosome substructure they had predicted on genetic grounds. Painter "was fond of relating the story of how he came to make the successful preparations of the giant [salivary gland] chromosomes—a bit of serendipity that involved enforced meditation under a cedar tree at his deer stand while he waited for a buck to wander into view one gray November day, and a sudden insight that led him to jump up, drive to the laboratory, and take down the fixative-stain acetocarmine, which softened the larval tissue and allowed him to squash the glands and spread their stained chromosomes out flat on a microscope slide. Thus the chromosomes in their continuity could be studied, as conventional paraffin sections had never permitted."[67]

In 1939 Painter suggested the application of squashes to bird and mammalian material. Koller in 1941 was the first since Belling to make

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Biological Bull. 1926, *50*: 160; J. Belling, "The number of chromosomes in the cells of cancerous and other human tumors," *J.A.M.A.*, 1927, *88*: 396; Kemp, *op. cit.* (n. 29 above), 4.

[67] Oliver, *op. cit.* (n. 17 above), 8-9; Painter, *op. cit.* (1971, n. 17 above), 35-36; T. Painter, "A new method for the study of chromosome aberrations and the plotting of chromosome maps in *Drosophila melanogaster*," Genetics, 1934, *19*: 176. E. Heitz, simultaneously and independently, applied the squash technique to insect larvae salivary gland chromosomes.

squashes of human (tumor) cells for chromosome counts, and during the 1940s squashes gradually gained favor among mammalian cytologists. By the early 1950s most cytologists felt they had progressed about as far as they could with sections and turned to squashes exclusively for their chromosome counts. As late as 1949 Matthey, in his masterful treatise on vertebrate chromosomes, did not consider squashes as anything more than a convenient preliminary to the use of sections in cytological technique. But his attitude changed completely after the addition of hypotonic pretreatment to the squash method.[68] Tjio and Levan naturally adopted squashes because of their backgrounds as plant cytologists and their familiarity with the work on chromosomes in ascites tumors which, being cell suspensions, responded very well to squashing. They chose the fixative-stain aceto-orcein, introduced by L. LaCour in 1941, in preference to aceto-carmine because of the greater selectivity of the stain orcein.[69]

Epilogue

It was not long after the discovery of Tjio and Levan that cytologists realized its full significance. Knowledge of the number (and form) of chromosomes in human cells was much more important than that of other biological constants, such as the number of teeth. As early as the 1930s, suggestions were made that certain congenital human abnormalities—including Mongolism (Down's syndrome)—had chromosomal bases. Chromosomal aberrations were well known in *Drosophila*. In mice, chromosomal aberrations had been correlated with neurological abnormalities, similar to those in man.[70] U. Mittwoch in the early 1950s studied the chromosomes of an individual with Down's syndrome. Using simple squashes she counted 24 bivalents in primary spermatocytes and concluded that in gross morphology and number the chromosome complement was normal.[71] The squash technique by itself was insufficient to reveal a difference of just one chromosome between an abnormal and a normal complement, much less permit the identification of the extra or missing chromosome. In 1958 and 1959, correlations between abnormal chromosome constitutions and many congenital defects, including the long known syndromes of Down, Turner, and Klinefelter, were independently discovered by a number of cytologists.[72] The discoveries were made as soon as the new cytological techniques were applied. It was now possible not only to distinguish a difference of one in the total number of chromosomes but also to determine which chromosomes were extra or missing, since some chromosomes could be individually identified.[73] The new cytological techniques inaugurated the new discipline of human cytogenetics, entirely unforeseen as well as unforeseeable less than a decade before because it was impossible as long as the cytological techniques were still lacking.

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[68] T. Painter, "An aceto-carmine method for bird and mammalian chromosomes," *Science,* 1939, *90*: 307-308; P. Koller, "A new technique for mitosis in tumours," *Nature,* 1942, *149*: 193; L. Sachs, "Simple methods for mammalian chromosomes," *Stain Technology,* 1953, *28*: 169; Makino and Nishimura, *op. cit.* (n. 61 above), 1-2; Matthey, *op. cit.* (1949, n. 1 above), 23; R. Matthey, "Les chromosomes des muridae. Revision critique et materiaux nouveaux pour servir à l'histoire de l'evolution chromosomique chez ces rongeurs," *Revue Suisse de Zoologie,* 1953, *60*: 228-231; R. Matthey, *op. cit.* (1963, n. 1 above), 294-295.

[69] A. Levan, personal communication; L. LaCour, "A new stain-fixative for chromosomes," *Stain Technology,* 1941, *16*: 169-172.

[70] P. Waardenburg, "Das menschliche Auge und seine Erbanlagen," *Bibliographia Genetica,* 1932, *7*: 47-48; A. Bleyer, "Indications that mongoloid imbecility is a gametic mutation of degressive type," *Am. J. Diseases of Children,* 1934, *47*: 345; L. Penrose, "Maternal age, order of birth and developmental abnormalities," *J. Mental Science,* 1939, *85*: 1149-1150; R. Turpin, A. Caratzali, and H. Rogier, "Etude étiologique de 104 cas de mongolisme et considérations sur la pathogénie de cette maladie," cited in J. Lejeune, R. Turpin, and M. Gautier, "Le mongolisme premier exemple d'aberration autosomique humaine," *Annales de*

Conclusion

The cytologist and historian of cytology A. Hughes (see above p. 496) has written:

> In the development of the sciences, the historian can trace in each two lines of research. One is concerned with the necessary technical basis of laboratory methods and practice; the other with discoveries which result from the application of this experience to the problems of natural phenomena. When both of these have been followed through the past centuries, we can begin to assess how far the limitations on the pace

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génétique, 1959, *1*: 41; G. Snell and D. Picken, "Abnormal development in the mouse caused by chromosome unbalance," *J. Genetics,* 1935, *31*: 213-235.

[71] U. Mittwoch, "The chromosome complement in a mongolian imbecile," *Ann. Eugenics,* 1952-1953, *17*: 37.

[72] J. Lejeune, M. Gautier, and R. Turpin, "Les chromosomes humains en culture de tissus," *Comptes Rendus de l'Academie des Sciences,* 1959, *248*: 602; C. Ford, K. Jones, P. Polani, J. De Almeida, and J. Briggs, "A sex-chromosome anomaly in a case of gonadal dysgenesis (Turner's Syndrome)," *Lancet,* 1959, *1*: 711-713; P. Jacobs, A. Baikie, W. Court Brown, T. MacGregor, N. Maclean, and D. Harnden, "Evidence for the existence of the human 'super female'," *Lancet,* 1959, *2*: 423; P. Jacobs and J. Strong, "A case of human intersexuality having a possible XXY sex-determining mechanism," *Nature,* 1959, *183*: 302.

[73] In the past five years, mammalian cytology has undergone another technical revolution with the introduction of new staining methods which reveal the longitudinal differentiation or banding of chromosomes and thereby make possible the precise identification of *each* chromosome, T. C. Hsu, "Longitudinal differentiation of chromosomes," *Annual Rev. Genetics,* 1974, *7*: 153-176.

> of discovery at each period have been due to imperfect apparatus, or to what extent other factors, such as misleading theoretical notions, have diverted men of science from making observations or have hindered the understanding of what they have been able to see.[74]

Historians of cytology have tended to neglect the history of cytological technique. In this paper I have focused on the technical side of cytology and its dominant role in the history of human chromosome counting. For two lengthy periods, cytologists were convinced that human cells possessed what proved to be the wrong number of chromosomes. Until about 1920, human cells were supposed to possess 24 chromosomes; from the 1920s to 1956, 48 chromosomes. The "erroneous" chromosome counts which gave rise to these beliefs were, initially, the result of inadequate cytological technique. Under the technical circumstances, the counts were actually quite accurate, particularly during the second period of human chromosome counting. Cytologists in the 1920s and 1930s should not be faulted for their incorrect counts of 47 or 48 chromosomes. However, they did fail to appreciate the uncertainty attached to those counts resulting from their imperfect technique. Convinced of the sufficiency of their technique and the sureness of their counts, cytologists fell victim to preconception. They expected to find a certain number of chromosomes, and they found them. Only with the improvement of cytological technique was such preconception finally overcome.

Acknowledgments

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[74] A. Hughes, "I. Studies in the history of microscopy 1. The influence of achromatism," *J. Royal Microscopical Society*, 1955, *75*: 1.

THE FIELDING H. GARRISON LECTURE *

BLINDERS OF THE MIND: HISTORICAL REFLECTIONS ON FUNCTIONAL IMPAIRMENT OF VISION

ILZA VEITH

The invitation to present the Fielding H. Garrison Lecture at the annual meeting of the American Association for the History of Medicine is a great honor that scarcely comes more than once to a member of our Association. I am indeed very grateful for having received this invitation.

Study of previous Garrison lectures which have been published in the *Bulletin of the History of Medicine* makes it evident that they generally reflect the various speakers' major subject of research; and it is for this reason that I have chosen to speak about the history of functional disorders of vision, because that topic is a natural extension of my ongoing study of the history of hysteria. Moreover, the topic of today's talk is also closely related to one important aspect of the work of the man whose memory is being honored with this lecture.

Fielding H. Garrison's *Introduction to the History of Medicine* has been the "Bible" of medical historians since it first appeared in print in 1913. At that time, however, it was evident that Dr. Garrison—encyclopedic though his medico-historial knowledge appeared to be—was not greatly interested in the history of neurology or psychiatry. These subjects played a very minor part in the early editions of his vast *History,* while the fourth and last edition of his great book has many more entries on their history.

This addition is due in large measure to the fact that Dr. Garrison had published, in 1925, a brief chapter on the history of neurology upon the invitation of Charles L. Dana, the famous neurologist, who wished to celebrate the publication of the tenth edition of his *Textbook of Nervous Diseases* with a historical preface. At that time scarcely anything had been written in this field, and by his short (forty-nine page) introduction to Dana's book, Garrison had become a pioneer in the history of neurology.

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* Given at the 47th annual meeting of the American Association for the History of Medicine, Charleston, South Carolina, May 2, 1974.

Although in 1929 neuro-psychiatry was still a more or less unified subject, we do find in the vast index of the fourth and last edition of the *Introduction to the History of Medicine* 45 entries on various aspects of neurology and 13 on psychiatric topics, few of which entries had been contained in earlier editions. In fact, in Garrison's description of the work of Albrecht von Graefe, the leading ophthoneurologist of nineteenth-century Germany, Garrison emphasized that von Graefe had utilized the ophthalmoscope with extraordinary success for the study of amblyopias, *i.e.*, dimness of vision in functional disorders.[1]

And it is indeed functional amblyopia which is the actual subject of my talk today, because this impairment can be traced back to the very beginnings of medical history.

Some diseases may remain essentially the same throughout history, varying somewhat only in severity and frequency; others, however, appear to change, because their symptomatology reflects the ever-changing social milieu. And they may also appear to change, because physicians display different attitudes and insights towards these disease manifestations.

One such syndrome is composed of the visual manifestations of hysteria, of which we have descriptions over a period of four thousand years. This description is contained in the oldest Egyptian medical document, known as the *Kahun Papyrus*. The case history is rather undifferentiated so far as the nature of the symptoms is concerned. There we read of a woman who is "ill in seeing" and does not rise from her bed. The illness in seeing is implied to be functional and temporary, as is her refusal to rise from her bed. This case is but one of several in that papyrus which deal with diseases of functional origin.

The *Kahun Papyrus* stems from the nineteenth century B.C., and for more than a millennium the Egyptian papyri remained the only medical documents of the Western World. Consequently, this first indication of functional impairment of vision was not followed by other writings on that subject until some 1400 years later, in the all-encompassing *Corpus hippocraticum*. It was Hippocrates, the so-called "father of medicine," who first divested illness of its supernatural connotation and attributed the functions or dysfunctions of the senses to the brain. In doing so, he presented a superb description of the mode of action of the brain which had hitherto remained unrecognized, or ignored by other physicians.

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[1] Fielding H. Garrison, *An Introduction to the History of Medicine*, 4th ed., revised and enlarged (Philadelphia and London, W. B. Saunders, 1929) p. 609.

> Men ought to know that from the brain, and from the brain only, arise our pleasures, joys, laughter and jests, as well as our sorrows, pains, griefs and tears. Through it, in particular, we think, see, hear, and distinguish the ugly from the beautiful, the bad from the good, the pleasant from the unpleasant, in some cases using custom as a test, in others perceiving them from their utility.[2]

Hippocrates concluded that the brain was the most important organ of the human body and that the eyes, the ears, tongue, hands and feet, indeed the entire body, act in accordance with the discernment of the brain; and he arrived finally at the classic definition in which he characterized the brain as "the interpreter of consciousness."

The physicians, and even the philosophers of the Hippocratic era, accepted the rational explanation of disease and of medicine in general, because it was thoroughly compatible with their all-inclusive concepts of nature. However, not all Greeks were rationally and scientifically inclined, and many looked to the supernatural for explanation of disease and its treatment. Moreover the Greek world at large was dream-conscious; political and personal decisions were often based on dreams. So a healing cult, utilizing dreams and dedicated to a healing god, combined these two elements dearest to all—divine worship and dream cult. Such a source of healing, particularly effective in functional diseases, was provided by the temples which were dedicated to Asclepios, the god of medicine.

The temples of Asclepios were generally situated in localities which provided a pleasant environment. After an initial period of sacrifices, ablutions, and fasting, the patient was placed on a couch in one of the many small rooms that surrounded the main center of worship and was told to rest; cure was to be wrought by the god Asclepios, who would appear in a dream. A number of votive tablets, excavated at the temple sites, tell of the dreams and visions the patients had experienced during their temple sleep. In most cases, it seems the god Asclepios appeared to them, and brought about their recovery by words alone, or by performing some manipulation, even including sham surgery, at the site of the malady. The god, as seen in the dream, carried a staff and was often accompanied by one or several of the large sacred snakes which abounded in the temples. Sometimes it appeared to the dreaming patients that they were touched or licked by these snakes. According to the tablets, *cure* was the almost invariable result of these dreams.

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[2] "The Sacred Disease," in *Hippocrates*, with an English translation by W. H. S. Jones (The Loeb Classical Library), vol. II (London, New York G. P. Putnam's Sons, 1923), p. 175.

The case histories, taken from the inscriptions which were excavated at the site of the great Asclepian temple in Epidaurus, are examples that confirm the impression of the various therapeutic approaches. Even if the cause and nature of the illness are not explained, it appears that many patients suffered from transitory blindness, probably of hysterical origin.

> Inscription 4. Ambrosia of Athens, *blind* of one eye. She came as a supplicant to the god. As she walked about in the Temple, she laughed at some of the cures as incredible and impossible that the lame and the blind should be healed by merely seeing a dream. In her sleep she had a vision. It seemed to her that the god stood by her, and said that he would cure her, but that in payment for her cure and disbelief he would ask her to dedicate to the Temple a silver pig, as a memorial of her ignorance. After saying this, he cut the diseased eyeball, and poured in some drug. When she awoke on the following day her sight in both eyes was fully restored.[3]

The following inscription deals with another case of this nature:

> Inscription 18. Alketas of Halika. This blind man saw a vision. He thought that the god came up to him and opened his eyes with his fingers. The first things he saw were the trees of the Temple. At daybreak he left the Temple restored to health.

The subject of fees or gifts to the temple is frequently mentioned in these inscriptions; and it appears that thank-offerings of sacrificial animals, objects of art, votive tablets or precious metals were usually expected. That they were essential in the healing ritual is confirmed by the inscriptions recording the histories of two blind men whose sight was restored, but who lost it again after refusing to make the prearranged donations to the temple.

These cases illustrate the special character of Asclepian medicine and its differences from religious healing in the commonly known sense. The healing occurred primarily by suggestion, and some patients came to the temple not as believers but with avowed skepticism, despite which they were cured. Actually, Asclepian temple healing persisted into the early centuries of the Middle Ages.

Another venerable source for case histories of functional amblyopia is the Bible. Perhaps the most impressive examples of transitory blindness of emotional causation are found in the Acts of the Apostles, where

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[3] This and the following inscriptions are quoted from Emma J. Edelstein and Ludwig Edelstein, *Asclepius: A Collection and Interpretation of the Testimonies* (Baltimore: The Johns Hopkins Press, 1945), vol 1, pp. 230-233, 235.

Saul, who later became Paul, is said to have lost his sight and had it restored at the hands of Ananias upon the command of Jesus.

Subsequently Saul himself, cured of his affliction through the Lord, struck with blindness Elymas the magician who had interfered with his missionary efforts. So forceful was the curse of "Saul, who was also called Paul" that we should quote it verbatim: "You monster of underhandedness and cunning! You son of the devil! You enemy of all that is right! Will you never stop trying to make the Lord's straight paths crooked? The Lord's hand is right upon you, and you will be blind and unable to see even the sun for a time." And indeed "instantly a mist of darkness fell upon him [Elymas, the magician] and he groped about for someone to lead him by the hand." [4]

A further source of narratives concerning blindness of an emotional causation comes to us from the study of hagiology when we learn of saints who suffered the trauma of blindness in the course of achieving martyrdom—unlike St. Lucy, whose martyrdom consisted in being permanently blinded by having her eyes removed by her tormentors. St. Agnes, another Virgin Martyr, was sentenced to enter a house of ill fame, but "a young man who looked upon her lustfully was striken blind." [5] This incident somewhat resembles that of "Peeping Tom," which will be discussed somewhat later.

In the Middle Ages and the Renaissance such proliferation of blindness gives rise to many questions: Was the belief in it as an act of superhuman punishment so powerful that persons who had accidentally or intentionally observed forbidden deeds and committed moral transgressions fairly expected to be punished by being struck blind and perhaps later made seeing again by divine grace? [6]

On the other hand, if cataract, injuries, glaucoma, trachoma, and the various medieval epidemics, such as smallpox, leprosy and syphilis had been responsible for the many losses of vision, would not the impairment have been permanent?

However, as it was, numerous such cases of blindness, which have been recorded, gave way to prayers and other acts of religious devotion and repentance; and therefore we may assume that such instances of so-called blindness had been cured by suggestion, possibly by auto-suggestion. The reports about impairment of vision and transitory blindness generally had to be subjective on the part of the patient; prior to the invention of the ophthalmoscope in the nineteenth century, physicians were unable to diagnose visual defects except for those caused by cataracts, trachoma, and external tumors. Therefore we do not know whether the cases of so-called blindness truly represent amaurosis, i.e., total, permanent loss of vision, or temporary impairments. In some cases we gather from the description in medieval books on witchcraft that blindness was caused by blepharospasm, or inability to separate the eyelids. In one of these books we read: "The old woman had pressed his eyelids so firmly together that the victim could not pull them apart to open them by natural means, thus he was blinded."

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[4] See also Acts 9: 7, 13, 17, 18.

[5] *Lives of Saints, with Excerpts from Their Writings.* Selected and Illustrated. Introduction by Father Thomas Plassmann, O.F.M., editorial supervision by Father Joseph Vann, O.F.M. (New York: John S. Crawley, 1954) p. 33.

[6] Acts, 13: 10, 11, 12.

The belief in supernatural causation and cure of deterioration of the eyesight persisted beyond the Renaissance and the period of Enlightenment well into the end of the eighteenth century. Not until the second half of the nineteenth century was the proposition offered that this affliction could not be *blindness* in the literal, i.e., organic, sense of the word, but only of a temporary, functional nature. This view was expressed by Pierre Janet (1859-1947), one of the disciples of the great French neurologist Jean-Martin Charcot, and one of his closest and most favorite students. Janet, like his teacher, devoted most of his professional life to the study of neuropsychiatry. Among the many books he wrote, several deal with various aspects of hysteria, which he had learned to investigate under the tutelage of his great teacher. In this respect, Janet moved on a parallel course with Sigmund Freud, another disciple of Charcot. The question of priority between Freud and Janet has been extensively studied by the distinguished neuroscientist, the late Percival Bailey of the University of Illinois,[7] who did not fail to stress its scientific insignificance.

Particularly pertinent to my lecture is Janet's essay on "Disturbances of Vision" which is contained in *Major Symptoms of Hysteria.*[8] Because of Janet's unique contribution to the history of psychiatry and to the subject I have chosen, I shall spend some time in discussing this work.

The first, detailed, and most important statement we find in Janet's *Major Symptoms of Hysteria* is his description of hysterical blindness as a dissociation of the totality of vision. However, he says that hysterical blindness is in fact rarely complete, as some vision is nearly always retained and the patient keeps the essential function of his sight. This opinion was shared by Sigmund Freud a few years later when he wrote about "Psychogenic Disturbance of Vision." [9]

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[7] Percival Bailey, "Janet and Freud," *Arch. Neurol. & Psychiat.,* July 1956, *76*: 76-89. See also: Academic Lecture, American Psychiatric Association, Chicago, May 1, 1956.

[8] Pierre Janet, *The Major Symptoms of Hysteria,* 2nd ed. (New York: Macmillan, 1920). This superb volume is based on Janet's series of lectures at Harvard University in 1906.

According to Janet, psychogenic sightlessness, if total, is generally the result of accidents and belongs to the phenomena of "traumatic hysteria." To illustrate this phenomenon, Janet furnished several case histories. The first deals with a 38-year old man who was cleaning a machine: A rag saturated with grease and petroleum caught in a gear and lashed into his face. The face was only dirtied. The patient was not concerned about this accident, but had much difficulty in cleaning his chin and eyelids of those fatty substances when he washed himself. It is important to note that nothing penetrated into his eyes and that he felt no pain in them. However, after an hour it seemed to him as if there were a veil before him; this veil grew thicker and two hours later he could no longer see at all. His vision fluctuated a little in the next morning and the following days. From time to time he could see a little, chiefly in his right eye. The fluctuation lasted for a month and then stopped, and for four years he remained entirely blind. Evidently Janet remained in professional contact with this patient throughout these four years and until he regained his sight.

The second case is similar and equally illuminating. It deals with a woman of 31 years of age. Employed in a laundry she experienced the explosion of a boiler and through this accident some water mixed with soap and lime was thrown into her face. Her skin was but slightly burnt and her lids swollen. She was in her menstrual period when the accident occurred. Like Charcot, Janet felt that all pathological, and especially psychopathological, conditions tended to be aggravated during menstruation. The patient felt agitated and dizzy. During the first days after the accident she hardly dared to open her eyes; soon afterwards she realized that she could no longer see. The amaurosis was complete for two years. When Janet reexamined the patient (he does not say when this occurred) there was already a slight restoration of the vision, which was easily and rapidly completed.

Janet's third such case history deals with another woman patient who also suffered from traumatically induced transitory blindness. Although one in this series of Janet's hysterical patients was a man, we find that in his case histories he constantly followed the old tradition of using the feminine pronoun when speaking about hysterical patients. Years later, another typical case of apparent functional amaurosis appeared in the annals of medical history in the person of Adolf Hitler. His blindness at the end of World War I would seem to have been a prime example of "traumatic hysteria," caused by an accident, *i.e.*, by his allegedly having been gassed in battle, more probably, though never admitted, by battle fatigue after years of combat duty. He regained his full vision within a few months without any subsequent impairment, which, considered with all other accompanying symptoms, appears to typify the case as one of hysterical causation.

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[9] Sigmund Freud, "The Psycho-Analytic View of Psychogenic Disturbance of Vision," (1910) in *The Standard Edition of the Complete Psychological Works of Sigmund Freud,* vol. XI (London: The Hogarth Press, and the Institute of Psycho-Analysis, 1957), pp. 211-218.

Like other disciples of Charcot, such as Guinon,[10] Janet states that much more frequent than complete amaurosis is *unilateral amaurosis*. He called it the highest degree of hysterical anesthesia which, in those days, was often noticed quite accidentally by the physician or even the patient on the occasion of a physical examination prior to induction into the armed forces. Janet then described a very complicated test for the discovery of such malingerers of unilateral amaurosis.

It is significant to observe that Dr. S. Weir Mitchell of Philadelphia, the American contemporary of Janet and Freud, and a distinguished neurologist in the American Civil War, encountered unilateral amaurosis even more frequently than his French colleagues among Civil War recruits who attempted to avoid military service. To uncover these malingerers, Mitchell developed a test much simpler than that of Janet. This test consisted in covering the good eye so that only the supposedly blind eye was exposed. The testing physician—in this case, Dr. Mitchell—extended two glasses of liquid, one with water, the other with whiskey, and invited the recruit to select and drink from one of them. When the examinee selected and drank the whiskey instead of the water, he could generally be unveiled as a malingerer. Nowadays, it would be interesting to know how sufferers of functional unilateral amaurosis react during eye tests for the driver's license, that is, in a situation where the incentive for malingering would be decidedly negative.

Bernheim of the so-called School of Nancy, designated the "suppression of vision" in one eye as *suggestion*,[11] while Janet called it a

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[10] Georges Guinon, *Les agents provocateurs de l'hysterie* (Paris: au Bureaux Progrès Médical, 1889).

[11] Hippolyte Bernheim, *De la suggestion et ces applications thérapeutique* (Paris, 1886).

"subconscious sensation," which was peculiar to humans and did not afflict animals. Janet differentiated the nature of human binocular vision from that of animals, inasmuch as that of humans is on one plane in contrast to that of most animals who have alternative binocular vision on each side of the face. However, when the human patient suffers from hysteria he—or, as in Janet's words—*she,* can dissociate the two visions by actually dissociating one eye—although the patient has never previously known that such a deactivation was possible.

Unconscious manipulation of vision can also be the case in the narrowing of the visual field—and here we come to the focal point of this lecture—"Blinders of the Mind." What has this unconscious manipulation to do with the narrowing of the field of vision? It is well known that human sight, owing to the dimension of the retina, extends over a certain space. In hystericals, however, frequently the field of vision is narrowed concentrically and becomes "tunnel vision."

The space an eye can see simultaneously without moving, is called the "visual field." It is not quite certain that all points in the field of vision are seen simultaneously in a single act of attention; nevertheless this brief definition is practically sufficient. However, in the case of certain dysfunctions, the patient becomes aware that his field of vision has narrowed often to a tunnel in the center of his sight. This can happen not only in hysteria but in certain cases of organic illness. Nothing is quite so distressing as a somatogenic contraction of the visual field. Patients afflicted with glaucoma invariably complain of no longer being able to glance over an entire page of a book, or especially a newspaper; they see only one word, or even one syllable at a time. Such patients who suffer from glaucoma see very well in the center, but may no longer be able to find their way in the street.

Hysterics, on the other hand, who have an exceedingly restricted visual field, go about the streets without the slightest trace of apprehension or concern, nor do they run into fences or other obstacles. It was just this difference in behavior between patients afflicted with glaucoma and those with hysteria which Janet brought to Charcot's attention; and he was gratified by the reaction of astonishment and even admiration which was shown to him by his usually aloof chief who rarely reacted with much emotion to the findings and successes of his disciples.

To support his point, Janet showed Charcot two young patients who were playing ball in the courtyard of the Sâlpetrière. He then brought them indoors to show his chief that the boys' visual field was reduced to a mere point through certain frightening experiences.

These deliberations led Janet to wonder whether hysterical tunnel vision must always be concentric, or whether it could ever be hemianopic. Hemianopsia, that is to say the vision of only one half of the visual field, is a frequent consequence of lesions of the brain such as cerebral vascular accidents, *i.e.*, of strokes. Because of intensive studies on the anatomy and physiology of optical nerves, especially by Gilles de la Tourette, the existence of hysterical hemianopsia was then categorically denied. Janet, however, could not see any reason why hysterical disturbances should not produce the same symptoms as the organic destruction of the nerve center. Every function has its organic center, and in certain cases the effect of functional and organic disturbances may be alike. This fact is most convincingly demonstrated in the study of hemiplegia. There is no disturbance more symptomatic of a major brain lesion than motor hemiplegia, but it is a well-known fact that the latter also often occurs in cases of hysteria. Consequently, Janet concluded it must be similar to hemianopsia, *i.e.*, the loss of vision in one-half of the visual field; and in spite of the contrary view of contemporary neurologists, Janet was convinced—and succeeded in proving it—that it does exist. In Janet's terms, "Subconscious sensations" can blot out part of the visual field.

Although this review can only cover a small part of the "Troubles of Vision" in Janet's *Major Symptoms of Hysteria,* I shall conclude it with his chapter about one more form of visual disturbance. Beyond purely neuro-psychiatric aspects, Janet's "Troubles of Vision" contains interesting data for the social and medical historian, because it adds an unusual descriptive facet to the Sâlpetrière and the patient population of that famous institution. The disturbance discussed by Janet is "dyschromatopsia," or the distortion of color perception. Pastel colors generally disappear first and almost entirely, while in the end, red remains the only color that can be discerned by these patients.

With red as the only persistent color in an otherwise colorless mental world, it had always been assumed that the hysterical patients had a special predilection for dressing in red. The hysterical patients in the Sâlpetrière wore red dresses—which Janet described as "showy"—they put red ribbons in their hair, as red was in fact the only color they continued to see. When Janet used the feminine pronoun in describing dyschromatopsia of his hysterical patients thus affected, he was entirely justified, as in this case: they were indeed all women.

There is one point, however, that Janet failed to consider in his amazement at the selection of red by the hysterical women patients of the Sâlpetrière; namely, the fact that many of them had been prostitutes who brought with them to the hospital the attire of their previous "professional" life. Therefore the patients' garb in the historical color of their trade most probably was all they owned when they were admitted to the hospital, rather than a preference because of the disappearance of all colors but red from their visual field.

In general, visual field studies of color-loss and color-perception had been made with great care and accuracy. Janet was aware that the perception of colors at the periphery of the visual field changes very much even in normal persons, and is influenced by many external conditions and, in particular, by the intensity of the light. But beyond these natural effects he said, "in hystericals the influence of the association of ideas plays an enormous part in the perception of colours." [12] As an example he reported about a young woman who saw someone put a bouquet of red flowers on her father's coffin. She became very angry, because to her the red color had a purely political connotation and was an adverse political symbol. From that time on she held red in abhorrence.

Janet's findings were the earliest to acquaint the contemporary medical world with the theory that many forms of visual impairment have a psychogenic, or rather, a hysterical substrate. Although he freely used a psychiatric vocabulary of modern terms, such as "unconscious," "subconscious," "divided, dual, or split personality," he did not attempt to explain those mental processes that cause the emotional trauma or functional impairment of vision, except when he attributed the love of the red color to "moral causes," as in the case of the inmates of the Sâlpetrière, or the hatred of it to flowers at a father's funeral.

Far more searching psychological explanations for functional impairment of vision were offered by Janet's fellow student, Sigmund Freud, who first associated these disturbances with *scopophilia,* as meaning the "love of seeing," or in modern terms "voyeurism," *i.e.,* in Freud's words "the sexual pleasure in looking." It was according to Freud not the unimpeded abandon to this tendency towards voyeurism that caused the psychogenic disturbances of vision, but rather the individual's repression from giving in to that instinct and his ability to remain unconscious of the forbidden sights. By invoking the repression

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[12] Janet, *Major Symptoms of Hysteria,* p 205

"the ego will have lost its dominance over the eyes . . . which will now be wholly at the disposal of the repressed sexual instinct and hence unable to function properly."[13] Freud continued:

"As regards the eye, we are in the habit of translating the obscure psychical processes concerned in the repression of sexual scopophilia and in the development of the psychogenic disturbance of vision as though a punishing voice was speaking from within the subject and saying: 'Because you sought to misuse your organ of sight for evil sexual pleasures it is fitting that you should not see anything at all anymore'. . . ." The idea of such instant punishment is borne out in many myths and legends.

The most graphic example quoted by Freud is the legend of Lady Godiva which "tells how all the town's inhabitants hid behind their shuttered windows, so as to make easier that noble lady's task of riding naked through the streets in broad daylight, and how the only man who peeped through the shutters at her revealed loveliness was punished by going blind."[14] Freud's theory on the psychogenic disturbances of vision was further shared by his student and colleague, the German psychoanalyst Karl Abraham after whom the Berlin Psychoanalytical Institute is named. Dr. Abraham wrote an essay on scopophilia in psychoneurotic patients, which has become one of his most famous publications and a classic in psychoanalytical literature.

Inexplicably, Dr. Abraham entitled, and used throughout his paper, the word "Scoptophilia" either through inattention to Freud's work or incomplete knowledge of Greek for the derivation of the word "scopophilia" (from *scopeō* to view and *philos* to be fond). Abraham shared Freud's understanding that "pleasure in looking" was frequently derived from the looking at forbidden objects, and consequently had to be repressed.

The transgression of this repression itself was believed to be the cause of hysterical blindness, the most far-reaching of visual disturbances. Indeed hysterical blindness is only one form—though a particularly striking one—of neurotic disturbances associated with the instincts of looking. There also exists a fear of looking, as well as of being looked at, known as scopophobia. It may finally be transformed into photophobia, the fear of light, and as such it was related by Abraham to the accidental or intentional sight of parental nudity. This dreadful punishment, photophobia, he explained in the following somewhat tortuous fashion:

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[13] Freud, "Psychogenic Disturbance of Vision," p. 217.

[14] "Restriction and Transformation of Scoptophilia in Psycho-Neurotics; with Remarks on Analogous Phenomena in Folk-Psychology" (1913), in *Selected Papers of Karl Abraham.* The International Psycho-Analytical Library, No. 13 (London: Hogarth Press, 1948).

As the sun was the symbol of the father, *i.e.*, the parents, Abraham reasons in his sensational essay, that to most persons the father—and also the mother—represented the sun. It is of linguistic interest that in German (as one of the few languages) the sun (*Sonne*) is a feminine noun, and the moon (*Mond*) is masculine; and hence, as a punishment for having seen their father or mother unclothed, the patients no longer permitted themselves to look towards the sun and towards their parents, and thus developed persistent photophobia from which they could be cured only—according to Abraham—if they could be reconciled with their early transgression giving in to their love of seeing; that is, they be cured of photophobia by psychoanalytically working through their voyeuristic tendencies. Whether or not the self-inflicted blindness of the unfortunate King Oedipus represents a drastic acting out of scopophobia must remain a subject of speculation for psychoanalysts, who have used this mythological personality, though in another context, from the very beginning of their discipline.

At the present time a rash of voyeurism seems to have befallen the patrons of pornographic films and other so-called artistic representations of social taboos by the pictorial and performing arts. The evaluation of the psychological impact of these phenomena and their interpretation will remain the task of future medical historians.

In comparison, a marked difference between the work of the three contemporary psychotherapists, Janet, Freud and Abraham, becomes unmistakably evident. Janet stated observed facts with scarcely any attempt to analyse the psychological reasons for the disturbance of vision—except in the cases of dyschromatopsia when his conclusions were somewhat simplistic. Freud and Abraham, however, probed into the psychoneurotic background of the defects of their hysterical patients who were predominantly, but by no means exclusively, members of the tender and delicate sex whose extreme sensibilities had been further sensitized by the repressions and neuroses of the Victorian era.

The discussion of my subject would be incomplete if I restricted it to the Western—especially the Victorian—conditioning which is indeed not the only one to be observed. It seems necessary to me to add a few words about a Japanese neurosis which has, in fact, an effect similar to scopophobia. This neurosis is called *Taijin Kyofu* and is a pathological fear of meeting people. Specifically, the patient fears that his gaze will intrude upon, disturb, and injure others, and he therefore strenuously avoids looking at them.[15] In a civilization accustomed to communal nude bathing, the existence of such an ailment seems astonishing, yet it does exist.

Social anxiety of offending others with too intensive a gaze also exists in Western society, but it usually does not go so far as to inhibit people completely from looking at others or to be looked at. Nevertheless the current fashion of wearing tinted eyeglasses, especially among women, may be covering up the need for such psychological protection. Even today, many women who are still content to grope their way through life in a myopic haze, rather than wear corrective lenses, believe that they can cover up their ocular defect by sporting what appears to be sunglasses. So it would seem that the wearers of tinted lenses have made themselves immune to Dorothy Parker's now somewhat shop-worn quip: "Men seldom make passes at girls who wear glasses."

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[15] Cited by special permission of the author from "Culture-bound Neurosis in Japan," by Masafumi Nakakuki, M.D. Presentation No. 70 (Session VI, Transcultural Psychiatry), Annual Meeting of the American Psychiatric Association, Honolulu, May 1973.

HIDEYO NOGUCHI LECTURE

GEORGE CHEYNE, MIRROR OF EIGHTEENTH CENTURY MEDICINE *

LESTER S. KING

Long ago when I first studied elementary physics in high school, our textbook contained a question that somehow has remained in my memory these many years, and now leads to the title of my paper. The question had to do with that branch of physics called optics, and went something like this—obviously I give only the general sense, and not the precise wording: "When you enter a room, which are you more likely to see—a mirror or a sheet of white blotting paper?" The "correct" answer—i.e., what the author had in mind—was, the blotting paper. This would scatter the light rays in diffuse fashion so that some rays would be likely to impinge on your eye no matter where you were standing. A mirror, however, instead of scattering the rays, reflects them in a regular fashion so that, if you notice the mirror at all, the reflection might blend into the field of vision and hence not stand out in any way.

A moment's thought makes us appreciate this. Certainly we have all had the experience of entering, say, a restaurant, and being uncertain whether we are in a single large room or in a small room with a mirrored wall. At first we do not see the mirror itself, but only what it reflects. But once we are aware of the mirror, we can, by changing our position or point of view, examine first one part of the room and then another. The mirror, itself inconspicuous, can help us to see what the room contains. The sheet of white blotting paper, however, calls attention to itself, stands out from its surroundings, forms an immediate focus for our vision, but does not help us see the remainder of the room.

I suggest that a metaphor such as this can help clarify 18th-century medicine. When we enter the room that we call the 18th century, we immediately perceive certain striking figures—Boerhaave, Stahl, Hoffmann, Haller, Hunter—who, like the white blotting paper, catch our eye no matter where we stand. But a physician like George Cheyne is, by himself, quite inconspicuous. We are scarcely aware of him unless we attend rather closely. Yet we find that he reflects a great deal of the 18th century and enables us to see much of its intellectual furnishings.

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* Delivered at the Johns Hopkins University, School of Medicine, April 12, 1973.

George Cheyne was not an outstanding physician, he did not change the course of medical history, but his voluminous writings represent well the intellectual activity of his era. He permits us to appreciate the currents and cross-currents, the conflicts and contradictions that harrowed early 18th-century thinking. It is this aspect to which I would devote my analysis.

I

The life of Cheyne I will touch on only briefly, to provide a background for his doctrines and their relation to his era. While there is no full length biography of Cheyne, several shorter studies[1] clarify many aspects of his life; many others, however, remain obscure.

Cheyne was a native of Aberdeenshire in Scotland. The year of his birth is still a matter of dispute. It is generally given as 1671 but Viets has produced evidence that it should be 1673. Intending to enter the ministry, Cheyne enjoyed a sound liberal education and, in addition, showed a substantial interest and proficiency in mathematics. For reasons that are not entirely clear, he changed his mind about the ministry and studied medicine at Edinburgh under Pitcairn, who exerted a profound influence over his intellectual development. Interestingly enough Pitcairn (1652-1713) had also originally intended to enter the ministry and also was greatly interested in mathematics.

When Cheyne completed his studies in 1701 the University of Edinburgh did not as yet confer any medical degree[2] and he received his degree from the University of Aberdeen. At this time he also published a small book on fevers that expressed the iatromechanical philosophy and was the first of his prolific writings.

Like so many promising young men, Cheyne went to London. There he was promptly elected Fellow of the Royal Society and established an excellent practice, while his professional activities were complemented by his reputation as a wit, a bon-vivant, and, unfortunately, a gourmet.

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[1] Henry R. Viets, "George Cheyne, 1673-1743," *Bull. Hist. Med.*, 1949, *23*: 435-452; Charles F. Mullett, ed., *The Letters of George Cheyne to the Countess of Huntingdon* (San Marino, Cal.: Huntington Library, 1940); *idem, The Letters of Doctor George Cheyne to Samuel Richardson* (*1733-1743*) (Columbia, Mo.: University of Missouri, 1943); R. S. Siddall, "George Cheyne, M.D., eighteenth century clinician and medical author," *Ann. Med. Hist.*, Ser. 3, 1942, *4*:95-109.

[2] John D. Comrie, *History of Scottish Medicine*, 2nd ed., 2 vols. (London: Wellcome Historical Museum, 1932), vol. I, p. 300.

His health began to suffer, forcing him to go on a rigid diet. Some recovery ensued but he relapsed, became markedly obese, and reached a weight of 32 stone (about 475 pounds). He again went on a rigid diet and became the outstanding proponent of the so-called extremely "low" diet, emphasizing vegetables, fruit, and milk. He completely eliminated wine and spirituous liquors and avoided red meat. Under a Spartan regime he recovered his health and lived until 1743. His life thus covers that crucial period in medical history when strongly conflicting philosophies were struggling for some sort of resolution.

Cheyne not only had an illustrious practice and a wide circle of friends, but he wrote voluminously. His writings enjoyed great popularity, going into repeated editions and numerous translations. As Mullet indicated,[3] he was highly articulate and represented that large body of physicians whose place in the development of medicine historians have slighted in favor of the "great names." Much of Cheyne's practice, especially his therapeutic concern with a "low" diet, was dictated by his own personal experience. But even more significant is the way that his theories reflect the intellectual movements and conflicts of the period. Study of his writings gives us considerable insight into these conflicts.

II

To understand early 18th-century medical thought we must keep in mind certain problems in philosophy and religion that greatly exercised the best thinkers of the period. The mechanical philosophy had quite transformed medical theory and led directly to the iatrophysical doctrines, somewhat indirectly to much chemical theorizing. But the mechanical philosophy, successful in the sciences and attractive in medicine, threatened the religious traditions and the establishments erected thereon. These mechanistic concepts, strengthened by concrete advances in astronomy, physics, and chemistry, all combined to cast doubts on the supremacy of God.

The daily social and cultural life was rooted in a tradition not too friendly to the new ideas. Scientific achievements had little effect on the people; traditional religion, however, affected their lives quite directly. The new philosophy seemed too close to materialism. Matter in motion could very well explain the material world, but what about the world of spirit, the realm of the immaterial? If we want to enter into the core of 18th-century thought, we must appreciate the relation of the material and the immaterial.

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[3] Mullett, *Letters of Cheyne to the Countess of Huntington*, p. ix.

Science and religion could not be separated. The world of matter had to be harmonized, somehow, with the world of spirit. In medicine and the biological sciences soul and mind, as immaterial entities, had to find a place in the philosophical explanations and systems. The traditional medicine of the 16th and 17th century—which I like to call neo-Galenism—gave an important place to the Aristotelian concepts of Qualities and Forms (Substantial Forms), and these, certainly, were immaterial. But the new philosophy, emphasizing matter in motion, tried to put these older concepts into a subordinate position, secondary to the *configuration* of particles which alone enjoyed status as really real. For the mechanical philosophy of the late 17th century the immaterial became dependent on the material, and lost the primary rank it had earlier enjoyed.[4]

But even if qualities and forms lost status, still the mind and soul and spirit remained important for medical philosophers. In this connection I would point out that many of the leading physicans at the turn of the century had either originally intended to enter the ministry or had a deep religious bias. Pitcairn, Boerhaave, and Stahl come immediately to mind. Cheyne himself, as we have seen, belongs to this group having a strong initial religious trend. His writings reflect the problems of the immaterial and its relation to the material world and the corpuscular philosophy.

We see this particularly in his *Philosophic Principles of Religion,* a work published originally in 1705. A second edition[5] ten years later included an additional part dealing with mathematical topics, but presented in a particularly difficult fashion. My own copy of the 1715 edition shows on the title page of Part II a note in an 18th-century hand, "This portion is very abstruse, and not easy to be understood by the most learned." With this comment the 20th-century reader can heartily agree. Fortunately, this Part II has nothing to do with our analysis. I have used the 1715 edition, but only Part I is relevant and this expresses the ideas of 1705. Many of the ideas in this early work are restated or expanded in his later writings.

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[4] Lester S. King, "Medical theory and practice at the beginning of the eighteenth century," *Bull. Hist. Med.,* 1972, *46*:1-15.

[5] George Cheyne, *Philosophical Principles of Religion, Natural and Reveal'd,* Part I, 2nd ed., Part II, now first published (London: Strahan, 1715) (hereafter referred to as *Phil. Prin.*).

Let us take up first the concept of God and then the concept of mind, soul, and spirit.

Cheyne regarded nature as a vast machine, subject to natural law. Bodies are composed of particles, and particulate matter obeys the laws of motion and inertia as expressed by Newton.[6] Cheyne calls these the laws of nature and we can state them in shorthand form: (1) bodies persist in a state of motion or rest unless acted on by an external force; (2) changes in motion are proportional to the impressed force; and (3) reaction is always equal to action. These laws in their beautiful simplicity, could indeed serve as first principles for the new mechanical philosophy which Cheyne heartily embraced.

From the laws of motion, especially the first law, we must infer that matter is passive. It cannot move by itself nor, once in motion, does it have the power to alter the course in which it is put.[7] Moreover, if material bodies obeyed merely the laws of motion, they would proceed only in straight lines. But celestial bodies, and terrestial bodies under certain conditions, move in curved lines. How to account for curvilineal motion? To explain it the primary laws of motion are not enough. Some additional principle is necessary. After we eliminate the Cartesian notion of vortices, the correct answer is, clearly, through gravitation as expounded by Newton.

To account for gravitation Cheyne performs some neat acrobatics, during which he refutes atheism and materialism, brings purpose and God back into natural philosophy and reconciles science and religion. His reasoning is worth detailed examination.

Gravity, the source of celestial motion, is not an essential property of matter, for, by the first law of nature, "no kind of motion is essential to matter." Furthermore, gravity acts at a distance without direct contact. But this cannot be "a result from the nature of matter, because the efficacy of matter is communicated by immediate contact, and it can by no means act at a distance."[8] Moreover, gravity cannot be derived from the laws of motion and therefore it is not mechanical.

For Cheyne, the three laws of motion and the corollaries deduced therefrom serve to define mechanism. Said Cheyne, the three laws of motion "do virtually comprehend all the rules of mechanism, and consequently, if any appearance contradicts these laws, or their necessary consequences, it is not to be mechanically accounted for."[9] To prove that anything is "immechanical" we need only to show that it does not follow from the laws of nature, i.e., the laws of motion. Gravity, according to these concepts, is not mechanical.

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[6] *Phil. Prin.*, p. 7.

[7] *Ibid.*, p. 10.

[8] *Ibid.*, p. 39. In all verbatim quotations I have provided modern spellings and eliminated the capricious capitalizations and use of italics.

If gravitation is not an intrinsic property of matter, and if it cannot be explained by mechanical principles, then it "must be a principle impressed on matter by the Creator of the world."[10] "We must of necessity [i.e., logical necessity] refer it to the power and influence of the First Cause of all things."[11] Gravity represents some additional property not essential to the *existence* of matter, not originally belonging to *"materia prima"* but "an original impress which continues in it, by virtue of the Omnipotent Activity, in the Divine Nature of which it is a copy or image."[12] Gravitation is implanted by some power superior to matter and *is independent of the laws of mechanism.*[13]

Cheyne uses the same type of argument to refute materialism of the epicurean type, that the world arose from the blind concourse of atoms in motion, interacting by chance. If gravitation is not an intrinsic property of matter, then the blind atoms could not form the world unless some Divine power impressed gravitation on matter. If gravity is not an intrinsic property of matter and if it cannot be explained by mechanical principles, and if, at the same time, its presence is essential to the universe, then blind materialism cannot stand, mechanism is insufficient, and the role of God is justified and restored to philosophy. The concept of gravitation served to integrate science and religion, physics and metaphysics, natural philosophy and theology.

In trying to prove the existence of God from the existence of gravity, Cheyne was not in any sense original; he reflected concepts previously expressed. Richard Bentley (1662-1742), foremost scholar of his era and deeply concerned with the religious implications of the new science, had entered the controversy through the posthumous efforts of Robert Boyle (1627-1691). Boyle, in his will, had left a sum of money "to settle an annual salary for some divine or preaching minister, . . . to preach eight sermons in the year, for proving the Christian religion against notorious infidels, viz. Atheists, Deists, Pagans, Jews, and Mahometans; not descending to any controversies that are among

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[9] *Ibid.*, p. 24. See also p. 42.

[10] *Ibid.*, p. 39, p. 41.

[11] *Ibid.*, p. 44.

[12] *Ibid.*, p. 41.

[13] *Ibid.*, p. 121, italics mine.

Christians themselves."[14] Bentley, the first appointee under Boyle's bequest, delivered a series of eight sermons in 1692, directed against atheists. As Hall states, in so doing, he established the tradition that much of the evidence for religion should be drawn from the study of nature. Following his introductory series, the lectures became a series of treatises on natural religion.[15]

Declared Bentley, "We can easily attain the knowledge of the deity by the sole use of our natural reason."[16] In the course of his demonstrations he discussed the problem of gravitation. Gravity is the "cement" which holds the world together, without which no body can "subsist and continue." In bodies, he declared, "the power of tending toward a center" [i.e., gravitation] does not arise "either from other bodies or from themselves." Gravity, "the great basis of all mechanism, is not itself mechanical, but the immediate *fiat* and finger of God, and the execution of the divine law."[17] He had anticipated Cheyne in the claim that gravity is neither a natural property of matter nor mechanical in origin, but the special *fiat* of God.

The sermon in which these ideas were embodied he delivered on 6 June 1692. Later he engaged in correspondence with Isaac Newton and four of Newton's letters, dated from December 1692 to February 1693, bear on this topic. These letters were not printed until 1756, so that Cheyne could not have used them, but they represent ideas that, presumably, were at least "in the air."

Regarding the question whether gravity was "essential and inherent to matter," Newton had declared, "Pray, do not ascribe that notion to me."[18] In his next letter he again requested that Bentley should not "ascribe innate [i.e., innate to matter] gravity to me," and he branded as an "absurdity" the concept that "gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum* without the mediation of anything else. . . ."[19] Action at a distance is not an ordinary property of matter, for it is "inconceivable" that "inanimate brute matter" should "affect other matter without mutual contact." [20]

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[14] Richard Bentley, *Sermons Preached at Boyle's Lecture,* edited by Rev. Alexander Dyce (London, Macpherson, 1838 [facsimile edition, New York: AMS Press, Inc., 1966]), p. xv; see also Mitchell Salem Fisher, *Robert Boyle, Devout Naturalist* (Philadelphia: Oshiver Studio Press, 1945), p. 23 and passim.

[15] Marie Boas Hall, *Robert Boyle on Natural Philosophy* (Bloomington, Indiana: University Press, 1965), p. 32.

[16] Bentley, *Sermons,* p. 53.

[17] *Ibid.,* p. 75.

[18] Isaac Newton, Letter to Bentley in Bentley, *Sermons,* p. 210.

[19] *Ibid.,* p. 212.

Once "brute matter" and gravitation are separated, we must invoke God to put them together again. This is what Bentley and Cheyne expressly favored, and what Newton implied.

III

Gravitation provides us a reason for believing in God, a reason derived from the inanimate world. It is easier to prove God from the existence of living creatures, for the familiar argument from design carried great weight. Adaptation of means to ends, and the functions and uses of bodily parts, imply purpose and design; they "clearly argue" an infinite wisdom and "a Being absolutely perfect." [21] This argument was convincing to the 18th century, but we need not pursue it further.

More significant to our purposes is the distinction between matter and spirit. Cheyne's philosophy rests on the concept that matter is passive and inert, and can undergo change only through some external force. And this, he declared, "can never be effected but by a spirit, or spiritual substance: which . . . is essentially self-active and self-motive." Force and motion derive from the action of spiritual substances. "Where matter, mechanism and its powers and laws end, there spiritual agency and energy begin." [22]

Wherever there is life there is some degree of soul or spirit. Life is "matter organized and actuated by a spirit or soul." We may conceive of life as "a nice, delicate, finely contrived machine, of a vast variety of organs set in motion by the first cause, and continued by an internal self-motive spring, which spring is this spiritual substance." [23]

God is the supreme spirit; finite spirits bear to him the relation of "miniatures . . . infinitely small sparkles," endowed with God's essential "attributes of life, activity, and intelligence." Spirits as "diminutive deities" have intelligence and freedom, as well as immortality.[24]

What spiritual substance *is*, we cannot know directly (until, that is, we "drop this coarse earthy tabernacle" and enter directly into the world of spirit).[25] However, spirit has "no sensible parts," [26] and is not divisible, although he admits we can form some notion of it if we "divide, refine, sublime and exalt body and matter *ad infinitum,* so that we be sure it can have very little of the sensible qualities." [27] This is indirect knowledge. In this world the only way we can get to know spiritual substance is by analogy.

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[20] *Ibid.*, p. 211.

[21] Cheyne, *Phil. Prin.*, p. 295.

[22] George Cheyne, *An Essay on Regimen, Together with Five Discourses, Medical, Moral, and Philosophical: Serving to Illustrate the Principles and Theory of Philosophical Medicin* [sic] (London: Rivington, 1740) [hereafter referred to as *Regimen*], p. 148; see also p. 124.

[23] *Ibid.*, p. 126.

[24] *Ibid.*, p. 120.

Cheyne shows us some of the problems that engaged thoughtful physicians. Originality and depth he did not have, and he contentedly echoed prevailing concepts. But his concern with the philosophical aspects of matter and spirit reveals the importance that philosophy held for the practicing physician at this time. In this conjunction we may note briefly the teachings of Stahl on this subject.

The need to connect the world of spirit with the world of matter formed a major concern for George Ernst Stahl (1660-1734). He had a strong Pietist background and combined a religious outlook with a physiology that essentially was mechanistic. The body, he emphasized, moved according to mechanical principles, to which were applicable such concepts as configuration, magnitude, position, and mobility; and to these must be added *motion.*[28] Motion, however, is an incorporeal entity, distinct from matter—an immaterial entity added to bodies (*res incorporea corporibus adiuncta*).[29] Elsewhere, after enumerating some attributes of matter, such as magnitude, extension, configuration, impenetrable solidity, he declared that motion is something added to the inner essential properties of matter (*motum . . . reliquis essentialibus materiae internis proprietatibus adiungere*).[30] Motion, as immaterial, presupposes a cause of similar nature (*motus, ut res incorporea, omnino supponat caussam etiam parem*),[31] and this he finds in the immaterial *anima.* Motion is of the same nature as the *anima* itself—both are immaterial.

In this way Stahl tried to connect the immaterial world, in which the *anima* plays a leading part, with the world of matter—the realm of physics and physical law. This connection he made through the concept of motion which, as immaterial, has kinship with the animating principle and at the same time can become a property of bodies. Motion thus acts as a link between mind and body, a sort of *tertium quid* that shares in the properties of each.

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[25] *Ibid.,* p. 123.

[26] *Ibid.,* p. 124.

[27] *Ibid.,* p. 125.

[28] Georg Ernest Stahl, "Disquisitio de mechanismi et organismi diversitate," in *Theoria medica vera* (Halle, 1737), p. 12 (par 29).

[29] Stahl, "Brevis repetitio summorum capitum medicae physiologiae," *Theoria medica vera,* pp. 428-429 (par. 20); see also "Disquisitio," *op. cit.,* p. 36 (par. 86).

[30] Stahl, "Demonstratio de mixti et vivi corporis vera diversitate," *Theoria medica vera,* p. 103 (par. 88).

[31] *Ibid.,* p. 118 (par. 124).

For Stahl the body was the instrument of the *anima*. Cheyne held comparable views but instead of "anima" preferred the terminology "spiritual substance." This, he declared, "uses material organs, of one kind of matter or another, in its operations; and it is highly probable, they are the nervous glands. . . ." [32] In varying the terminology, he also adopted such words as "mind" or "soul," the "self-moving, self-active, sentient, and intelligent principle" that has the power of "beginning motion" in bodies.[33]

IV

Cheyne had to prove the majesty and power of God, the reality and importance of spiritual substance. But however intense his religious feelings, he nevertheless was above all a physician, concerned with the physical ailments of patients. To explain these diseases he followed the general teachings of the mechanical philosophy. At the same time two other major currents were influencing medicine—chemistry, inexorably in the ascendant; and the neo-Galenic teachings, rapidly declining but yet not without influence.

As an iatromechanist, he tried to explain the phenomena of disease by "mechanick principles." The body is a machine, "nothing but an infinity of branching and winding canals, filled with liquor of different natures." [34] Diseases arise from a "vitiation of the quantity, quality, or motion of the fluids, or to a bad disposition and texture . . . of their conduits." [35] To understand the workings of the body we must apply "the doctrine of quantity, i.e. geometry and numbers." [36] Natural philosophy, unless supported by geometry, "is but a pleasant romance."[37] With this epithet he characterized the doctrine of "the philosophic physicians" who invoked not geometry but fanciful notions such as substantial forms, sympathies, and antipathies.

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[32] George Cheyne, *The Natural Method of Cureing the Diseases of the Body, and the Disorders of the Mind Depending on the Body* (London: Strahan, 1742) [hereafter referred to as *Natural Method*], p. 34.

[33] *Ibid.*, p. 94.

[34] Cheyne, *A New Theory of Acute and Slow Continued Fevers . . . To which is prefix'd An Essay Concerning the Improvements of the Theory of Medicine,* 4th ed. (London, Strahan, 1742), preface [unpaginated].

[35] *Ibid.*, pp. 5, 6.

[36] *Ibid.*, p. 6.

Equally unsatisfactory were the teachings of the chemical physicians, who did not distinguish what took place in the laboratory from what took place in the body. "The heat of our bodies," he declared, "is in no way able to produce the same effect with their furnaces" (in modern terminology, *in vitro* results do not necessarily apply *in vivo*). And furthermore, the claims of the chemists are "nonsense, unless they first shew their . . . chemical effects to be necessary corollaries from the known laws of motion."[38] In this way Cheyne neatly begs the question: the principles of mechanics, expressed in mathematical formulae, ruled nature; therefore, whatever did not conform to these principles could receive no serious attention.

This dogmatism, however, he tempered with some critical insight. Medicine, he admitted, was still deficient in many ways. He mentioned some aspects that awaited improvement, and his listing shows sound thinking. We are deficient, he declared, in our knowledge of finer anatomy. Arteries, veins, and nerves are grossly manifest, but their "continuation" has not been traced and their ramifications extend below the limits of visibility. Cheyne wanted empirical demonstration as well as theoretical derivation: ". . . unless our theories and observations confirm one another, they will be still little more than the most probable conjectures."[39] The modern idiom would say that while the mechanical theories give rise to certain inferences regarding the minute structures of the body, we lack any concrete verification of these inferences. Reason can make extrapolations regarding the finer structure, but these are not valid without empirical evidence.

He continued in the same enlightened vein. The mechanical philosophy rests on geometrical principles, and Newton had given us the key for understanding the major phenomena. But Cheyne pointed out that these principles "have not as yet been applied to the lesser, less obvious, less constant, and less uniform effects of nature . . . so absolutely necessary to a true theory of medicine." And while Cheyne had the faith that these would apply to all aspects of nature, or, as he said, "that from the same principles the grand appearances of nature have been accounted for, these more minute ones may be so too," he nevertheless wanted to have a concrete demonstration that it actually was so. This demonstration had not as yet been performed—"it . . . has not actually been done, and without which we shall be still straitened in our theories." [40]

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[37] *Ibid.*, p. 9.
[38] *Ibid.*, p. 11.
[39] *Ibid.*, p. 22.

This reveals the important insight that theory without verification is mere speculation rather than science. Theory alone cannot guarantee truth. Yet this concern with critical thinking was honored more in the breach than in the observance. Many authors of the early 18th century gave such lip service to the need for experimental verification, and then completely disregarded the principles involved. Pitcairn had offered striking examples of this defect, and Cheyne tended to follow the footsteps of his master.

Cheyne listed many areas where more knowledge was sorely needed. Newtonian concepts, he felt convinced, would be able to explain physiological processes; nevertheless he realized that conjecture and extrapolation, however probable, were not the same as concrete demonstration. This praiseworthy attitude, unfortunately, he did not carry out in practice.

V

Cheyne was in no sense a scientist; rather was he a clinician who could claim considerable success in treating patients. However, it was not enough merely to prescribe favorite remedies and condemn others; to maintain status as a physician he had to give sound reasons for the treatment he employed. His therapy had to have rational justification. If he could not provide sound explanation for what he did and what he prescribed, if he could not offer a sound rationale, then he would be merely an "empirick"—an epithet obnoxious to all educated physicians. A suitable rationale could be found only in "natural philosophy."

In this he came particularly under the influence of Pitcairn and the iatromechanists who strongly opposed the special medical doctrines of the iatrochemists. Yet despite the opposition, some chemical concepts had become firmly established and had to be taken into account. Cheyne, in offering a "philosophical" basis for his clinical procedures, fell back on certain chemical terms, and interwove these into the fabric of his mechanical explanations. The interplay, the attempt on the one hand to utilize chemical concepts and, on the other hand, to discredit them, forms an interesting aspect of 18th-century medical thought.

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[40] *Ibid.*, p. 25.

As indicated earlier, Cheyne did not pursue any original investigations into nature's secrets. He took his doctrines from the writings of others, sometimes, however, adding an emphasis or an extrapolation or an inference of his own. Let us examine some of his "natural philosophy" as it applies to medical theory, with particular attention to the relations of physics, chemistry, and medicine.

Cheyne's various chemical concepts, mentioned in writings extending over some four decades, reveal great complexity. He has expressly acknowledged his debt to Newton[41] but, as Debus had pointed out,[42] there is a remarkable degree of eclecticism. His chemical ideas he had derived at second hand from his readings or discussions and his concepts show kinship to a wide range of different authors. Individual aspects can, perhaps, be traced to Paracelsian and Helmontian doctrines, to Willis, Boyle, Lemery, and, of course, Newton, as well as Newtonian followers of the late 17th or early 18th century such as Pitcairn, Keill, and Freind.

Chemical notions, however derived, play an essential role in Cheyne's medical concepts. It is not possible for me to trace the separate strands back to their sources. Fortunately, new work in the history of chemistry is bringing fresh viewpoints, and some recent books provide indispensible perspectives.[43]

When we examine the actual *things* found in the material world and subject them to analysis, we find, according to Cheyne, that they are composed of five elements—sulfur (or "oil or spirit, or the matter of fire"); salt (hard particles soluble in water); air (a "thin elastic dry fluid"); water (a fluid more dense and inelastic); and earth (gross, unalterable, and the "basis and cement" of the others).[44]

But these elements seem to exist in three states, or, in Cheyne's words, "three degrees of the size of particles in bodies."[45] The elements

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[41] *Regimen*, pp. 91-92.

[42] Allen G. Debus, personal communication. I am indebted to Dr. Debus for comments and suggestions about early 18th-century chemistry. The responsibility for all statements in this paper, however, is exclusively mine.

[43] Allen G. Debus, *The English Paracelsians* (London: Oldbourne, 1965); Robert E. Schofield, *Mechanism and Materialism: British Natural Philosophy in an Age of Reason* (Princeton, N. J.: Princeton University Press, 1970); Arnold Thackray, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass.: Harvard University Press, 1970). J. R. Partington, *A History of Chemistry*, vol. 2 (London: Macmillan, 1961), represents an older but still valuable tradition.

[44] Cheyne, *Natural Method*, pp. 6-7. See also *Regimen*, p. 5.

[45] *Natural Method*, p. 15.

"are of various sizes, densities and figures."[46] The most minute particles, "the first and least," he identified as solid, compact, and indivisible, at least by any finite or natural force or art. These, he went on to say, "seem to be the particles that make the primitive elements," which he identified as the "original" salt, sulfur, air, water and earth. These least particles remain always the same, and he speculated regarding their configuration. As the simplest of bodies they should have the simplest structure. Presumably they were shaped as triangular prisms, spheres, and cubes, which are the most simple solids.[47]

At this point, Cheyne's thought becomes a little obscure. The concept that matter exists in three sizes of particles can, perhaps, be traced back to Descartes, but Cheyne was also influenced by contemporary chemical theory. The least particles, which we can think of as atoms, combine into larger aggregates of the same kind—which we might, perhaps, think of as molecules. For Cheyne the second category of matter consists of "the simplest composition and combination of these elementary particles." These composites are not found as such in nature but are produced by "art," that is, by operations in the chemical laboratory. They are the "chemical depurated and rectified salts, sulfur, water, air, and earth." However, no art can produce these in absolutely pure state, but they are always mixed with other elements.[48]

Elsewhere,[49] he described the first order of particles as "almost infinitely small, hard and elastic," and he considered them as "etherial or Newtonian fluid." These, aggregated, comprise the next larger class of particles; and these, in turn, go to make up the bodies of the third order. Or, differently phrased, the third category is "the common mixture or product of the least integral particles, as we find them in nature, and without art, in animal and vegetable substances, where these elements are combined in different proportions."[50]

The term "element" Cheyne used rather indiscriminately to apply to any or all of the three orders. Thus, he spoke of "the several elements, or the several aggregates or masses of the original particles, now commonly called elements."[51]

In this latter reference, when enumerating the elements, he substituted "light" for "sulfur." Light, we must recall, was deemed to be particulate, with "activity, velocity, tenuity of its particles."[52] And in the treatise on gout he indicated that the "parts" of sulfur and those of light and of fire "act mutually upon one another," and bodies owe to sulfur their "reflecting and refracting virtues," and from the sulfur proceeds their inflammability. "The particles of sulfurous bodies receive and retain those of light and fire, more strongly than any other bodies: And the rays of light and particles of fire, separate, move and turn the parts of sulfurous bodies into fire and flame."[53]

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[46] *Regimen,* p. 92.

[47] *Natural Method,* pp. 15, 16.

[48] *Ibid.,* p. 16.

[49] *Regimen,* p. 92.

[50] *Natural Method,* p. 16.

[51] *Regimen,* p. 4.

The properties of light, heat, and sulfur were thus intermingled in a rather glorious jumble. Cheyne was trying to account for manifest properties through confused concepts drawn from various chemical and physical theories, without any great concern with consistency or precision of detail, and without any attempt to provide specific evidence for his assertions.

In this jumble, his views on the nature of fire are especially relevant. The elements of the second category, we recall, are the product not of nature but of the chemical furnace, and this involves the application of fire. Substances produced by chemical art are, he believed, entirely unfit to serve as medicines. All "chemical medicines . . . everything that has passed through the tortures of the fire, is so ignited, and the matter and active particles of light or fire are so transubstantiated into it, that it is ever afterwards unfit to be admitted into an animal body. . . ."[54] Elsewhere, he spoke of chemical procedures having the effect of impregnating material "with the substance and matter of fire,"[55] and thus rendering them unfit for "animal bodies."

Fire, heat, light, sulfur, and the properties of combustibility as well as volatility, seem inextricably confused. Fire seemed to play at least three roles. First of all, it related to the ancient doctrine of qualities—the primordial "hot." Although the primary qualities no longer enjoyed the metaphysical status that they did for the neo-Galenists, the notion of qualities still exerted influence in medical thinking. Casual sentences reveal this persistence—e.g., "Medicines act principally by their most eminent and sensible qualities."[56]

Second, fire was construed as particulate. The material substance of actual fire[57] thus formed part of the corpuscular philosophy. In a sense the neo-Galenic quality of "hot" had been turned into "particles of hot"—more a change of orientation and nomenclature than of anything else.[58] A material entity had been created as an *ad hoc* procedure, a quality was hypostasized to accommodate a new metaphysics.

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[52] *Ibid.*, p. 5.

[53] Cheyne, *An Essay of the True Nature and Due Method of Treating the Gout*, 6th ed. (London: Strahan, 1724) [hereafter called *Gout*], pp. 36, 37.

[54] *Natural Method*, pp. 43-44.

[55] *Ibid.*, p. 17.

[56] *Ibid.*, p. 42.

But more significantly, heat or fire became an activity. Cheyne ranked fire with certain dynamic functions. He listed what he called the principles of action—a term that served to identify certain modes of interaction rather than anything substantial. These principles are "1. Attraction or repulsion, and its several laws. 2. Elasticity or reaction. 3. Fermentation. 4. Heat, fire, or its matter and cause." He admitted ignorance of the "primary causes" of these, but considered them to be "the secondary causes of actions in all bodies."[59]

All of these Cheyne tried hard to reduce to mechanical principles but with dubious success. The laws of attraction and repulsion had not been adequately determined by observation or experiment.[60] Analogous to gravity in the great bodies of the universe, the smallest particles exhibit a principle of attraction and repulsion, a power or force which "in small particles of matter, increases as the size of the particles is diminished."[61]

Particles may be either elastic or inelastic. If elastic, they will collide and "resile"—i.e., rebound. But if not elastic, they will coalesce into masses. When elastic particles collide and get reflected, in a series of actions and reactions, there is produced "a fermentation, effervescence and ebullition."[62]

Fermentation implies a gaseous component. For Cheyne "air," composed of particles with repulsive force, was "fixed" in animal, vegetable, and mineral substances. This air can be "set at liberty, through the action of fire, fermentation, putrefaction, dissolution, or any other intestine action."[63] Release of "air" could be accomplished through several agencies, of which fermentation is one.[64] Fermentation was thus considered a force comparable to fire, acting through resilience and elasticity, and separating out the components of bodies.

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[57] *Ibid.*, pp. 44, 45; *Gout*, p. 37.

[58] In this connection see *Regimen*, pp. 34-35.

[59] *Natural Method*, p. 7.

[60] *Regimen*, pp. 94-95.

[61] *Ibid.*, p. 97.

[62] *Ibid.*, p. 98; see also *Natural Method*, p. 15.

[63] *Regimen*, p. 101.

[64] *Ibid.*, p. 99.

The "release" of "air" through mechanical factors he described in some detail. There is no food without wind—i.e., without elastic (or inelastic) air enclosed in its substance. When ingested food is sufficiently "ground and comminuted, and the wind set free," it passes "by perspiration, like smoke out of a chimney"[65]; but when digestion is bad or perspiratory ducts are stopped, the air accumulates as in a room choked with smoke because of a foul chimney.

But a purely mechanical explanation of digestion caused some lurking doubts. Pitcairn had been extremely rigid in his doctrines. Digestion, said Pitcairn, is not carried out by the assistance of any ferment. The "concoction" that takes place in the stomach divides the food "into integral parts, not differing from what they were before," but of lesser bulk, in the same way that coral might be ground with water "and reduced into an impalpable powder, whose parts are only small pieces of coral, and not any principle into which coral is resolved."[66] Such a formulation, however, which reduced digestion in the stomach to mere trituration, did not fully satisfy Cheyne. Food, softened through the juices of digestive glands, was, to be sure, broken down by muscular action of stomach and abdomen, through which the "parts are broken, and their intimate cohesion dissolved." But then he added cautiously, "and possibly from other causes, never to be known."[67]

Elsewhere,[68] he briefly discussed specific secretion, and he noted the nature of "runnet," from the calf's stomach that turns milk into curds and whey. Various observations, he said "make it more than probable, that in digestion there is something besides trituration." He thought that glands could contain "an originally specifically attractive zest." "This original zest," together with gentle heat and gentle trituration from muscular motion, "are sufficient to explain all the appearance of concoction."[69] The nature of this "zest" receives little more than bare mention, but these cautious remarks are still a far distance from Pitcairn's dogmatism. The concern with some "specific" factor above and beyond the general laws of mechanics, reveals a kinship with the old neo-Galenic specific qualities, and also a compatibility with the specific ferments that chemists had invoked.

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[65] *Natural Method,* pp. 218-219.

[66] Archibald Pitcairn, *The Philosophical and Mathematical Elements of Physick* (London, 1718), p. 36.

[67] *Phil. Prin.,* p. 295.

[68] *Natural Method,* p. 23.

[69] *Ibid.,* p. 24.

VI

If we want to understand the bodily economy, the elements obviously play an important part. Of special importance, however, are the salts. These, although never obtained in an absolutely pure state, exist in three kinds or categories: acid, alkali, and "urinous or volatile." The acid salts are sharp and seem to be "angular, with plain surfaces." Alkalis, "porous and obtuse, or calcarious," are like "sponges" and hence can undergo a "fermentation with acids."[70] Since the acid particles are "probably triangular and highly attractive, and the alkali particles are porous calxes," Cheyne believes that "the sharp points of the acids will be sheathed or blunted in the pores of the alkalis."[71]

Most important is the third category, peculiar to the animate body. These salts, however, although also porous and obtuse, are "intimately united with a light volatile oil or sulfur." They are the products which "all animal substances, and most vegetables yield, when chemically prepared, or much divided and sublimed by putrefaction, fermentation, or distillation."[72]

Thus did Cheyne distinguish inorganic from organic substances. Salts of the latter category exist in great variety, for they can combine with one another and also with variable quantities of "sulfur, oil, or inflammable spirit," and thus are "capable of infinite volatility and sublimity, and so of infinite force or activity."[73]

These organic salts, because of their infinite capacity for combination, show a wide range of properties. Some of these "urinous and volatile" salts Cheyne considered to be active agents of disease—of "contagion and pestilential distempers." The great variety of such diseases he tried to correlate, vaguely, with the properties of the salts under different circumstances. Thus, epidemics have a variety of remote causes, such as putrefying animal bodies, or insects, or clothes or other objects saturated with harmful particles, or rotten food abounding with these particles, or mineral vapors and exhalations. These, however, exert their harmful effect through the "urinous animal salts," either by introducing new salts into the body or, it would seem, by modifying those already present. Cheyne's exposition is worth quoting at some length.

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[70] *Ibid.*, p. 100.

[71] *Ibid.*, p. 216. In connection with the discussions on salts, see also Cheyne's *The English Malady, or, a Treatise of Nervous Diseases of all Kinds* (London: Strahan, 1733), chapter 5.

[72] *Natural Method*, p. 100.

[73] *Loc. cit.*

Pestilential distempers, he said, "can only arise from an high exaltation, volatilisation and sublimation of these urinous animal salts, and mineral steams, which being hard, porous, and alkaline, but fitted with a light caustic and ethereal oil, or sulfur, whereby their velocity and elasticity is increased, they become small, volatile, and highly attractive, by their acid sulfur, when taken into animal bodies in great quantities, by the lungs, or vasa inhalantia." They act by "effervescence" and "rend, tear and putrefy the solids, and break the cohesion of the fluids and so destroy animal life." [74]

Epidemic or infectious diseases exist in varying grades of severity, and these he would correlate with a weakening or dilution of these animal salts. Smallpox is the "lowest degree of the plague . . . and the great-pox is the same, more condensed and concentrated." And then, in quite an off-hand way, he declared, "And thus, by an easy and natural piece of philosophy, all epidemical infections, and pestilential distempers, may be accounted for." [75]

Cheyne did not systematically expound the pathogenesis of various diseases. As a practitioner he was concerned chiefly with therapy, and his comments on pathogenesis derive mostly from *obiter dicta* that support and justify his therapy. Such comments reveal his thinking.

His views on syphilis (the great pox) are interesting. The "essence" of venereal disease lies with the "particular animal salt which abounds in the spermatic organs." This he considered "a subtle, active and caustic alkali, less sublimed and volatile than that which is the cause of the pestilence, and infinitely more fine and volatile than our most perfect common volatile, animal salts." [76] These caustic salts are "enveloped and sheathed with a soft mild jelly, when inocuous" and in this state provide only a "stimulus, and titillation" which result in lechery and sensuality. Indeed, volatile alkalis are "great promoters of salacity." But under the influence of various external factors—and he mentioned especially lechery, climate, "nastiness," a hot diet, and laziness—"these salts turn acrid and caustic, and so produce what is called the great pox." The specific cause of this disease is thus "the caustic, active animal salts." [77]

In general, diseases arise from the vitiation of normal specific secretions. The mucus, phlegm, and serum secreted by various glands of the

[74] *Ibid.*, p. 102.
[75] *Ibid.*, p. 104.
[76] *Ibid.*, p. 198.
[77] *Ibid.*, p. 199.

respiratory or digestive tracts, were "designed by Nature to sheath, lubricate and preserve the elasticity of these organs," [78] but these secretions, "when viscous, saltish, or morbid," he called "the seminium or principle" of various diseases. Among others he mentioned coughs, asthmas, pleurisies, consumption, scurvy, colic, "hot and inflammatory ulcers, and the cancer." For syphilis, when the "specific animal salt" responsible for "lust and lechery" becomes "high, hot and corrosive" from the various factors already mentioned, there result the "pains, corrosions, inflammations, ulcers, and phagedenous tumors on the tender and delicate parts of generation first; and at last spreads all over the habit like a cancer or plague, and consumes both flesh, skin and bones." [79]

The great variety of animal salts Cheyne divided into three major classes, depending on the degree of fineness. He distinguished the "common animal salt or spirit," like hartshorn or sal armoniac; second, salts producing the plague, which are "the highest exaltation, sublimation and . . . subtilization of these animal salts"; and third, those that produce the pox [great pox] and cancer which are "more coarse and fixed, and, as it were, a medium between the other two." [80]

VII

Cheyne described many specific diseases caused by salts and modified by various environmental or internal factors. Rather than discuss this aspect generally, I will consider briefly the concept of obstruction that played such an important part in 18th-century medicine.[81] Cheyne's concept of disease reflected the then current thinking in physiology—that bodily processes depend on the free passage of fluids (or humours) through vessels of various types. Once this initial premise is granted, then any obstruction to the free vascular flow will cause disease. Other factors, however, would also play a part.

Gout offers a fine example to help us understand Cheyne's theories of pathology. Gout was extremely prevalent in 18th-century England. Physicians and laity alike realized that in this disease a rich diet and "high living" played an important part. As for the clinical aspects,

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[78] *Ibid.*, pp. 204-205.

[79] *Ibid.*, pp. 205-206.

[80] *Ibid.*, p. 206.

[81] Lester S. King, *The Medical World of the Eighteenth Century* (University of Chicago Press, 1958), chapter 2, especially pp. 83-88; Friedrich Hoffmann, *Fundamenta Medicinae,* Introduced and translated by Lester S. King (London: MacDonald, 1971), *passim,* especially Book II, "Pathology."

Sydenham had already given a splendid clinical description, but the underlying causes and their modes of action remained a subject of dispute.

Cheyne's book on gout enjoyed great popularity. Its title and subtitle indicate its contents and intent: *An Essay of the True Nature and Due Method of Treating the Gout, Written for the Use of Richard Tennison, Esq.* (note 53 above). In this book the primary interest rests on treatment, but the details of the treatment receive their justification and rationale from theories.

The underlying cause of gout, said Cheyne, is a special configuration of the very smallest vessels. These, in the gouty person, are "in their natural conformation narrower and more stiff" than in persons free from the disease.[82] An additional cause—what we may call the precipitating cause—rests with the food, namely, an excess of "tartarous, urinous, or other salts, introduced into the blood by the food." These salts, if not properly broken down by the digestive process, nor eliminated by proper exercise, can unite in "clusters," and then "must necessarily form obstructions, and give pain, when by the force of the circulation, they are thrust through narrower and stiffer small vessels." [83] Highly seasoned rich foods supply the urinous salts, while strong liquors furnish the acid salts.

This theory explains many clinical features: some persons have a predisposition to the disease because their vessels are too narrow; gout customarily begins in the older age bracket when the fibers have become stiffer and less yielding, and thus the disposition to obstruction is intensified by the loss of flexibility; bad diet brings on attacks by introducing material (salts) that might pass through normal channels and get eliminated but which obstruct the narrower channels. Why does the disease attack the joints? Cheyne's answer, "Because, in the joints, the smallest vessels are more compressed by the larger heads or protuberancy of the bones, and are thereby rendered narrower, and more readily obstructed." [84]

The treatment follows logically from the pathogenesis: relieve the obstruction, break up obstructing aggregates, and eliminate foods that contain the responsible salts. Experience had already shown that certain regimens or therapy had beneficial results. The theory gave reasons why.

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[82] *Gout,* p. 2.
[83] *Ibid.,* pp. 4-5.
[84] *Ibid.,* p. 7.

Moderate exercise was recommended, for it increased the motion of fluids, so that the pathogenetic salts strike against one another and are broken up into smaller masses. Furthermore, exercise increases the force of the blood, thus helping to relieve obstruction and to promote the natural perspiration that normally helps to eliminate the salts. But moderation in eating and drinking are also important. A wholly vegetable diet weakens the digestive powers. A proper diet will include fish and the white meats (such as poultry). Wine must be taken in very small amounts and diluted.

Evacuations help to eliminate salts. Cheyne held strongly to this type of therapy—at least in the form of gentle sweats and purges. Mineral waters and tea act as diluents which thin the blood and "dissolve and break the salts and keep them . . . from running into clusters." [85]

Mercury had great merit in relieving obstruction. It is heavy—fourteen times as heavy as water—and thus has great force in "opening" obstructions. It divides into very minute globules which are smooth and soft, and can enter the finest capillary tubes. Mercury may be administered in its crude form or else in "preparations." [86] Mercury also promotes salivation, and while this may sometimes keep patients free from gouty symptoms, it is a potentially dangerous procedure. Sometimes "a full and free salivation does so break, rend and tear all the smallest, tenderest, and finest vessels and fibers, that the body becomes in a worse state." [87] The mercury has, to be sure, a purifying action on the blood but the "great weight, and the excessive force it adds to the blood" can sometimes tear the vessels.

Some gouty patients improved with administration of mercury; others, pushed to salivation, became worse. Cheyne created—or rather adapted—a theory which "explains" both sets of facts in verbal fashion without supporting evidence. His statements show how crude empirical observations were able to masquerade under the cloak of theory. Proof, as we understand the term today, was neither necessary nor important. Logical consistency was enough.

I could continue at considerable length, discussing the various details of treatment in various diseases, and the rationale by which Cheyne justified his methods. Vomits (which "are in disease what bombs are in besieging forts"),[88] purges, and bloodlettings all had their place.

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[85] *Ibid.*, p. 23.

[86] *Natural Method*, pp. 119-126.

[87] *Gout*, p. 25.

[88] *Natural Method*, p. 144.

Much of Cheyne's voluminous writings was devoted to clinical and therapeutic aspects of medicine, the practical methods of caring for patients, the recommended treatments.[89] These aspects, although intrinsically interesting, hold less interest in the present context, and I do not want to consider them further in this place. Our concern is more with theory.

Any improvement of the patient Cheyne attributed to treatment and gave detailed reasons why and how the improvement came about. But if a patient got worse Cheyne could also provide an explanation, using exactly the same concepts but adding enough additional or subsidiary factors to account for the divergent result. Whether these additional factors existed in reality or only in his imagination was quite irrelevant.

Theory was highly systematized. A small number of primitive propositions, drawn chiefly from mechanics, formed a base that could, by deductive reasoning, explain all clinical and therapeutic phenomena. The base of the system rested vaguely on some observed phenomena of mechanics, but there was no further empirical support relating these to the phenomena of the living body, no additional factual evidence to support the inferences of the theory.

The first half of the 18th century, and much of the second half, continued the tradition that had long dominated academic medicine, namely that logic was more important than observation, and that theory derived its force more from internal consistency than from empirical verification. Progress toward a more modern viewpoint came slowly, only after medicine accepted new standards of evidence, new criteria for validity, new evaluation of cogency. How these standards and criteria arose and gradually permeated the scientific and medical communities form a quite distinct chapter in the history of ideas. Meanwhile the writings of George Cheyne serve to epitomize a viewpoint that dominated medical thinking for half a century.

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[89] See, for example, *The English Malady,* note 71 above, *passim.* Also George Cheyne, *An Essay of Health and Long Life* (London: Strahan, 1724).

A HISTORICAL-PHENOMENOLOGICAL STUDY OF BODILY PAIN IN WESTERN MAN *

DANIEL DE MOULIN

> "But pain is perfect miserie, the worst
> Of evils, and excessive, overturnes
> All patience."—John Milton,
> *Paradise Lost*, VI, 462.

Much has been written about the anatomical, physiological, biochemical, pharmacological, anthropological, moral, theological or philosophical aspects of physical pain.

In his well-known book, Professor Buytendijk states that in modern society the problem of pain is almost entirely confined to the question of its relief, but that nevertheless ". . . the pain-*experience* itself compels us to ask questions." [1] The practical management of pain in everyday practice and a more fundamental approach to the various problems relating to the experience of pain are greatly hampered by our inability to assess or measure its nature and intensity objectively. In evaluating the pain of our patients we are entirely dependent on their statements and behaviour as well as on our imagination, which may sometimes be quickened by subjective experience.[2] The phenomenological clinical picture, however, can never be an accurate reflection of the anthropological sense of being ill, and an incorrect evaluation of the existential situation of the patient might easily ensue. However conscientiously the physician proceeds in his sedative treatment, however carefully he tries to adapt its modality and its dose to the needs of the individual patient, he will repeatedly find that the patient feels himself wronged. Buytendijk remarks in this connection:

Modern man takes offence at many things that used to be accepted with resignation.

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* This study was begun in 1967, when the author was temporarily attached to the Johns Hopkins Institute of the History of Medicine as a visiting lecturer.

[1] F. J. J. Buytendijk, *Over de pijn*, 2nd ed. (Utrecht and Antwerp: Het Spectrum, 1957), p. 19. A German translation by Helmuth Plessner, of the first edition, appeared in Bern under the title of *Ueber den Schmerz* (H. Huber, 1948).

[2] It was already recognized by Hippocrates that pain, experienced as it is as a fundamentally personal experience, is coloured by all kinds of attendant factors: "How must one value pain? Look at fear, patience, timidity," *Epidemics* II, sect 2 § 10, *Oeuvres complètes d'Hippocrate,* with a French transl. by E. Littré, 10 vols. (Paris: J. B Baillière, 1839-61 Cited hereafter as ed. Littré), vol. V, p. 10.

> He takes offence at growing old, at a long sick-bed, frequently even at death, but certainly at pain. Its occurrence is inacceptible. The demand to do away with pain becomes progressively stronger, as every doctor, dentist, surgeon or obstetrician knows from daily experience. It has led to the development of an algophobia which, unlimited as it is, becomes in itself a torture and which, moreover, produces a pusillanimity that, once it is present, must also display itself when faced with other experiences.[3]

The enormous and ever-increasing turnover of analgesics confirms the clinical impression.

How can we explain this? Can modern algophobia be attributed purely and simply to a decreased willingness to endure pain, or are we conceivably observing an increase in the pain experience as such, an increase in the painfulness of pain? When we are convinced that it is only a matter of a changed mental attitude, then we must also recognize that modern medicine with its powerful means of pain-control is responsible for this in no small measure (Buytendijk).

Let us take a closer look at the question, whether sensivity to pain may have increased in the course of time. A well-known investigator who answered the question in the affirmative was the American neurologist Silas Weir Mitchell. It was not much more, though, than a supposition which he formulated:

> Civilized man has of will ceased to torture, but in our process of being civilized we have won, I suspect, intensified capacity to suffer. The savage does not feel pain as we do: nor as we examine the descending scale of life do animals seem to have the acuteness of pain-sense at which we have arrived.[4]

It is important for our investigation to observe that these words were written in 1892, seven years before acetylsalicylic acid was introduced into medical practice by Dreser and Wohlgemuth. Mitchell does not give any opinion upon the point in time at which the heightened sensitivity to pain might have arisen.[5]

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[3] Buytendijk, *Over de pijn,* pp. 14 and 15.

[4] S. Weir Mitchell, "Civilization and pain," *J.A.M A.,* 1892, *18*: 108. Mitchell was not the only one to assume that cultural and intellectual development are accompanied by an increased sensitivity. Ten years before, Ch. Eloy had written that pain perception can sometimes be "en quelque sorte émoussée." "Ce fait est d'observation vulgaire chez les personnes d'éducation grossière, . . et chez les individus incultes. . . ." *Dictionnaire encyclopédique des sciences médicales* (Paris: P. Asselin and G. Masson, 1884), vol. 30, p. 482. Similarly German philosophers of the last century accepted as almost self evident that the grade of human sensitivity is directly related to the level of intelligence (A. Schopenhauer, *Die Welt als Wille und Vorstellung,* I, book 4, § 56, 1st ed. 1844) or the degree of civilisation (F. Nietzsche, *Genealogie der Moral,* II, 7, 1st ed. 1887).

[5] The fact that Mitchell brackets the abolition of the rack together with the intensification

More explicit in this respect was the French surgeon René Leriche (1879-1955), one of the most original figures in modern surgery. In his book *La chirurgie de la douleur* he discussed the problem in great detail. "Sans conteste," he concludes, modern man is more sensitive to pain than even his immediate ancestors. Leriche does not want to be misunderstood: he emphatically explains that he does not mean the "résistance morale," but the sensitivity to pain as a physiological quality. The arguments with which he supports his assertion are the following:

A. Men has indeed at all times suffered much pain and has always searched for methods of relief, but until recently he did not possess such methods, so that he has hardened himself against his painful situation, as a consequence of which he has felt the pain less;

B. Modern man attaches exaggerated importance to the slightest irritation of his sensitivity, and tries, much more than his ancestors, to eliminate even the most trivial pain, now that he knows that there are means to do so. But because of this he habituates himself to experience pain more easily and more intensely.[6]

These arguments, however, can at best support a change of mentality, certainly no physiological increase in sensitivity.

Leriche based his general pronouncement on the fact that two great historical figures, namely King Louis XIV of France and Cardinal de Richelieu, suffered from morbid processes which supposedly were very painful, without withdrawing from their heavy obligations as sovereign and statesman, respectively. He also pointed out that Jean-Dominique Larrey, Napoleon's famous army-surgeon, used to perform disarticulations of the shoulder in patients who, seated on a drum, only made grimaces without letting themselves be heard.

As to the resignation of the French king, however, it should be remembered that history also provides examples of royal patients with less patience: the lamentations that in 1560 escaped from an earlier king of France, Francis II, when he suffered a sudden aggravation of a chronic, purulent otitis media, were heard in the streets.[7] It seems, moreover, somewhat doubtful whether the lesion from which Louis XIV suffered actually was as distressing as Leriche took it to be, at any rate in its later, chronic stages. The extraction of a tooth—unfortunately of

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of pain, suggests that he sees some connection between these two events, which would mean that the increased capacity for pain set in at the end of the eighteenth century.

[6] R. Leriche, *La chirurgie de la douleur,* 2nd ed. (Paris: Masson & Cie, 1940), pp. 66-67.

[7] F. K. Kessel, "Neurochirurgische Bemerkungen über einige französische Könige," *Münch. med. Wschr.,* 1959, *101*: 1585-1587, 1624-1627 (reprint p. 6).

a part of the maxilla and of the hard palate as well—had left the monarch with a chronic osteomyelitis and a fistula.

In the *Journal de la Santé du Roi* it is merely recorded that occasionally a purulent discharge occurred and that food or drink taken into the mouth were liable to come out through the nose,[8] obviously a serious inconvenience, but not necessarily very painful. Leriche had better have referred to another incident from the life of the Sun King in support of his theory, viz., the operation that he underwent in 1686 for an anal fistula. Nobody will doubt the painfulness of such an intervention, which, of course, was carried out without anaesthesia. It is said that the king made no complaint during the torment, which lasted some hours, and that very same evening he attended a session of the council.[9] Can this be because he felt but little pain? It seems more likely that the monarch to whom the words "l'état c'est moi" are attributed, had identified himself so strongly with France, that he simply could not afford to cast reflections on the glory of the country by screaming like a common mortal.

Leriche believed that the increase of human sensitivity in all respects must have come about more than a century ago, strongly promoted by the introduction of surgical anaesthesia and of aspirin, agents which have disaccustomed Western man to certain pains.

As far as aspirin is concerned, it should be remembered that Silas Weir Mitchell had observed an increase in pain several years before this analgesic became available: the introduction of aspirin—and of other easily obtainable drugs—can therefore at best only have accelerated a process already in progress. To the subject of surgical anaesthesia we shall return.

Leriche is not the only author who suggests the first half of the last century as the period during which the intensification of pain began. He is supported by Professor J. H. van den Berg of Leyden.[10] Van den Berg goes back to the eighteenth century when Friedrich Anton Mesmer, first in Vienna and later in Paris, introduced hypnosis as a method of medical treatment. It became apparent as early as 1778 that hypnotic trance may be accompanied by insensitivity to pain, but this phenomenon was not put to use on a fairly wide scale in operative surgery until 1829. In 1800, Van den Berg recalls, Humphry Davy discovered the narcotic properties of nitrous oxide and even suggested the use of this gas in surgical operations. In 1818 Michael Faraday made a similar discovery with regard to diethyl ether, which means—still following Van den Berg—that doctors in 1800 and again in 1818, were offered the possibility of using chemical means for the suppression of pain during operations. The doctors did not yet employ such agents, however, in 1800 or in 1818: the first surgical anaesthesia—ether narcosis for a tooth extraction—was given in 1842.[11] Why not earlier? Because, Van den Berg expounds, the patients still had little need of it. "But why," and now we are quoting the Leyden professor literally, "had the patients of about 1845 so much more need of analgesics than their fellow sufferers of 1780? The answer is: because in the period between 1780 and 1845 pain sensitivity had increased." [12] A surprising pronouncement which, supported as it is by exact dates, deserves a closer examination. We will not go into the explanation which Van den Berg offers for the increase of pain sensation during the period 1780-1845; it is more relevant here to verify the veracity of the assertion as such. Van den Berg himself refers to the case of a Madame de Bonnac of Paris, who in 1751 was to undergo a breast amputation. The surgeon, in order to comfort her, had assured her that the operation would take no longer than one minute but she had calmly allowed him four minutes, on condition that he would perform this task well and neatly. Van den Berg feels that this account "by its matter of fact and terse expression displays a mentality that we, sensitives, no longer possess." [13]

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[8] E. Seidler, "Louis XIV, Sonnenkonig im Schatten der Medizin," *Die Therapie des Monats*, 1962, *12*: 251-258.

[9] Nancy Mitford, *De Zonnekoning*, Dutch transl. by Beccy de Vries (Amsterdam: H.J.W. Becht's Uitgeversmij, 1966), p 155.

[10] Dr. J. H. van den Berg, contemporary professor of conflict psychology in Leyden, became well known by his theory of *metabletica* His works, written in a brilliant prose, are translated into many languages.

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[11] It is indeed curious that such a long time transpired between the observations of Davy and Faraday, and the practical application of their findings to medicine. Their publications did not remain unnoticed in the scientific world. Davy's book *Researches, chemical and philosophical, chiefly concerning nitrous oxide,* received comprehensive and favourable reviews, in medical journals as well. In none of these reviews, however, is any attention paid to the last remark in the *Researches* which reads: "As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place." F. F. Cartwright, *The English Pioneers of Anaesthesia* (Bristol: John Wright and Sons; London: Simpkin Marshall, 1952), pp. 310-312. This inattention might in part be explained by the casual manner in which the sedative effect of laughing gas was reported by Davy: he himself apparently did not realize the importance of his discovery. The brief communication by Faraday in the *Quarterly Journal of Literature, Science and the Arts* was accompanied by so severe a warning of the dangers of the use of ether that it is hardly surprising that his publication did not lead directly to any practical applications. Another factor which probably contributed to the fact that Davy's communication attracted so little attention is that pain inherent in surgery seemed so much a matter of course, that its prevention—after the many unsuccessful attempts in the past—was considered by many to be a phantasm. "Eviter la douleur dans les incisions est une chimère, que ne poursuit plus personne." A.A.L.M. Velpeau, *Nouveaux éléments de médecine opératoire* (Brussels: H Dumont, 1832), p 10.

[12] J. H. van den Berg, *Leven in Meervoud* (Nijkerk: G. F. Callenbach, 1963), pp. 248-253. Strictly speaking there is a slight difference between the dating of Van den Berg and Leriche. While Van den Berg sees a gradual development over a period of some decades preceding the introduction of narcosis, Leriche seemed to accept a causal relation with the introduction of surgical anaesthesia as such.

This anecdote, however, can be interpreted in a different way. From the fact that—with barely concealed admiration—this event is recorded at all, it could be inferred that the dauntlessness of the patient was an exception rather than the rule. And does not the very shortness of the conversation between patient and surgeon indicate the reality of pain, so clear and unavoidable, that it need not even be put into words?

When we leaf through the *Recueil périodique d'Observations de Médecine, Chirurgie, Pharmacie,* etc. for the years 1754-1759, we read for instance about "douleurs énormes" from which a woman suffered during pregnancy: "les douleurs étoient si vives qu'elles étoient accompagnées de convulsions." We read of another woman with a tumor in the abdominal wall which brought about "souffrances mortelles," and of yet another patient, who was tortured by a "douleur aigue, insupportable" when a septic fever was followed by an inflammation of the foot. There is a description of a woman who was attacked by "les plus cruelles douleurs" in the loins when she, uttering "cris perçans," discharged a piece of sheep bone in her urine.

In the case histories of 1750 or thereabout one can read moreover about fierce pain caused by fractures or incarcerated herniae, by amputations and other operations, about attempted suicide because of trigeminal neuralgia, and it was a bad sign when a gangrenous leg could be amputated without "grande douleur." [14]

The surgeons of the eighteenth century did take the sufferings of their patients to heart, as is apparent from the tearing hurry they displayed in their operations, using well-sharpened knives. William Cheselden (1688-1752), a master of lithotomy, could perform the operation in only a few minutes. Still he felt sick the night before such an intervention, with the thought of the pain he would have to inflict.[15] His French contemporary Pierre Dionis confessed suffering from the same feebleness regarding the amputation of an extremity, which operation he states, cannot be performed without enormous pains that words fail to describe.[16] Somewhat later John Bell wrote in his *Principles of Surgery* that "the surgeon must be especially careful of every thing that may conduce to his patient's comfort, of supporting his strength when it seems to flag with wine and medicines, and of comforting him with anodynes and cordials when he sees him in pain." [17] Opiates were given before and after operation and it is interesting in this connection that Boerhaave, although not a surgeon himself, compounded a laudanum containing "pulvis cardiacus," to use as a preoperative sedation "ad amputationis administrationem." [18]

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[13] *Ibid.,* p. 255.

[14] By consulting only a French source here, I tried to eliminate any disparity in national character or culture which might confuse the comparison with the example of Van den Berg. Most of the case-histories found in the journal mentioned, however, are related to common people: was Madame de Bonnac's self-control perhaps conditioned by her aristocratic upbringing?

The use of opium did not always lead to the desired result, though, as is illustrated by the case of the British admiral Lord Duncan (1731-1804), "one of the bravest hearts that ever beat," who during the removal of a tumour of the back, although "half stupified by opium," proved to be "a most unsteady patient" during the operation.[19] Van den Berg's supposition, then, that the sensitivity to pain in 1842 had increased in comparison to that in 1780, finds no support in contemporary casuistics.

Did the increase in pain experience perhaps commence at an earlier time? Ernest Seifert, until some years ago head of the University Surgical Clinic in Würzburg, thinks that it did, and he has devoted an entire monograph to the subject.[20] Pain only becomes actual pain, according to Professor Seifert, when it is experienced as such. Conscious experience is not possible without ego-consciousness, by which man is distinguished from animals, nor in the absence of certain corticothalamic nerve tracts which end in the frontal cerebral cortex where the affects are roused that are inherent to human pain experience. In his investigation into the pain sensation of our ancestors, Seifert has examined the facial expression of the sufferer in paintings and sculptures of the past, from which he deduced an underdeveloped pain perception in antiquity and in the Middle Ages. This conclusion, which we still have to verify, induced him to put forth that, as late as medieval times, ancient man can be compared with a modern patient who has undergone a frontal leukotomy. In the now rather obsolete operation of frontal leukotomy or lobotomy, the above-mentioned, phylogenetically recent thalamo-frontal connections are cut, following which the patient still feels his pain but tolerates it with more indifference. Seifert declares that it is not possible to state precisely in which period of history European man evolved out of his pseudo-leukotomized state to his present oversensitiveness.[21] Elsewhere in his book, however, he gives evidence of his familiarity with the Dutch seventeenth-century genre painters, who enjoyed depicting on canvas or copper the quack or the village doctor carrying out a minor operation on patients whose disapproving grimaces clearly show that the treatment was not exactly a pleasing experience (Fig. 1). Should not Seifert have inferred from the dissimilarity in facial expression of pained individuals of the seventeenth century and their fellow-sufferers of the Middle Ages, that the development of pain consciousness must have come about, at the latest, in the century of Rembrandt, Jan Steen and David Teniers? Seifert does not make that obvious conclusion, and he is hardly in the position to do so, since he attaches significance to the phylogenetic development of the thalamo-frontal tracks, and for such a development a span of two centuries is really too short, as Van den Berg rightly pointed out. We will not go into Seifert's explanations of the enhancement of pain, but we do want to subject to a closer examination his idea that classical and medieval man, as far as his appreciation of pain is concerned, is comparable with a leukotomized patient of today. To do this, we will go back in history step by step, starting with the seventeenth century, to see whether it is possible to point to a moment at which man has relinquished his pseudo-leukotomized stage, if indeed he has ever lived in such a phase. This will at the same time give us an opportunity to offer a brief sketch of the comparative importance of pain in medical practice in the periods under examination. A seventeenth-century physician who was concerned with the phenomenon of pain was Thomas Sydenham, "medicus in omne

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[15] D. Ottley, "The life of John Hunter," In: *The Works of John Hunter,* ed. by J. F. Palmer (London: Longman & Cie, 1835), p. 9.

[16] Pierre Dionis, *Cours d'opérations de chirurgie* (Paris: L. d'Houry, 1707). The Dutch edition consulted by me is entitled *Verhandeling van alle de Chirurgicale Operatiën* (Rotterdam: J. Hofhout, 1710), pp. 554-555.

[17] Edinburgh: T. Caddell, Jr. & W. Davies, 1801, vol. I, p. 15

[18] *Libellus de materia medica et remediorum formulis,* etc. Sect. 469, N 5.

[19] "Painless operations in surgery." Anonymous article in *North British Review,* 1847, 7 (May): 169-206.

[20] E. Seifert, *Der Wandel im menschlichen Schmerzerleben* (München: J. F Lehmann, 1960).

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[21] *Ibid.*, p. 85.

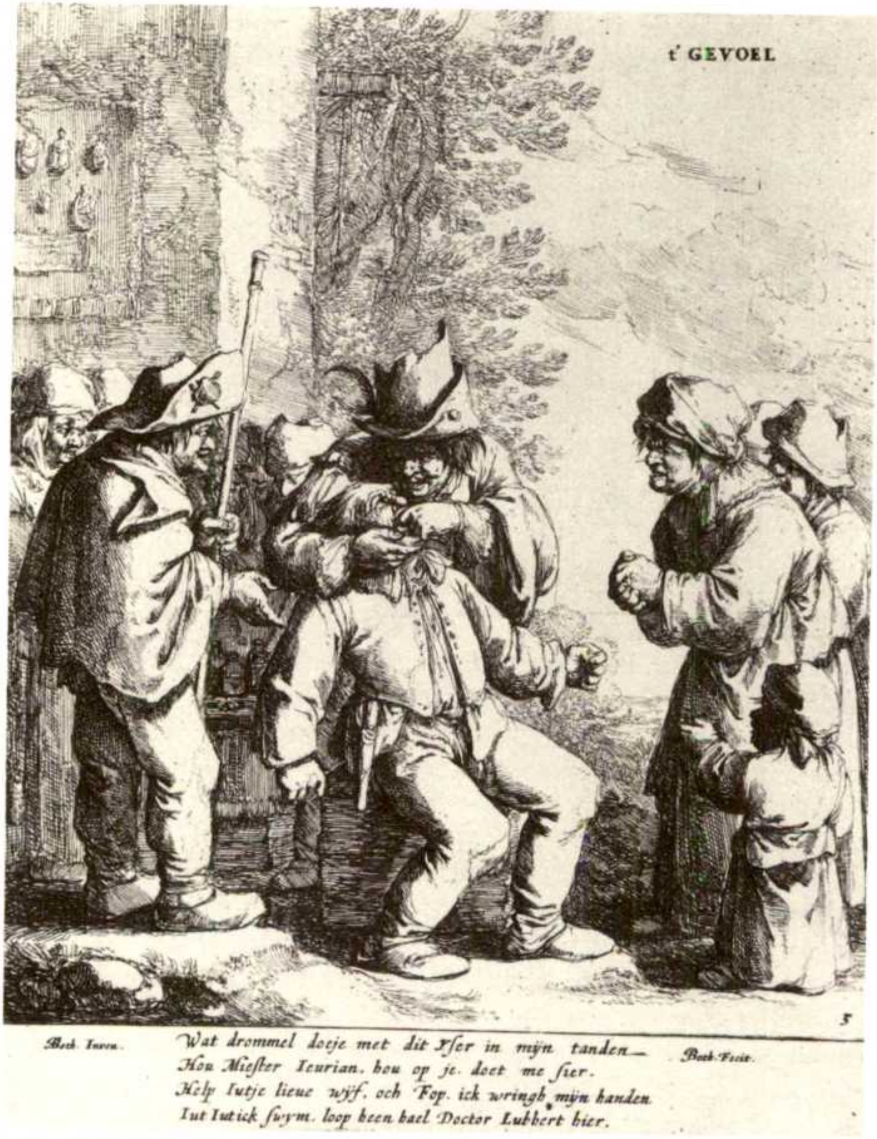
t' GEVOEL
5
Both Inven.
Wat drommel doeje met dit Yſer in mijn tanden—
Hou Mieſter Ieurian. hou op je. doet me ſier.
Help Iutje lieue wijf. och Fop. ick wringh mijn handen
Iut Iutick ſwym. loop heen hael Doctor Lubbert hier.
Both Fecit.

FIG. 1. "Sensation." One of a series of five etchings by Andries Both (about 1608-1641), depicting the five senses. (Collection of the Centraal Museum, Utrecht.)

aevum nobilis," as his tomb-stone in Westminster Abbey commemorates him. Sydenham gave a substantial account of a fierce type of epidemic diarrhoea with cramps and vomiting which occurred in England in the years 1670, 1671 and 1672.[22] After a colourful description of the intermittent but atrocious abdominal pains, "surpassing any accidents to which even the most wretched of mortals is exposed," he sings the praise of his beloved laudanum liquidum, *i.e.* tinctura opii crocata.[23] Without this "sacra anchora," an indispensable tool in the hand of the experienced physician, medicine would be crippled and lame, Sydenham solemnly declared.[24] A much reserved judgment about the value of opiates was expressed by Professor Georg Wolfgang Wendel (1645-1721) a respected academic teacher in Jena:

> There is under the sun not a single medicament that requires more prudence than opium and the opiates; it should not be prescribed wherever an unfavourable outcome is feared. The doctor must be judicious in the use of it, and careful: judicious towards those who refuse to take it because of some futile fear that death might ensue, but also be careful that he does not blot the reputation of the medicament, a blot that even the most cautious can hardly avoid; not a few sad examples of the kind occur in literature, most of these, however, being covered by earth.[25]

J. C. Pezold, who quotes this pronouncement in his academic thesis, remarks that when no remedy is found against their pain, patients do not seldom react in rage and wish themselves dead or even consciously make away with themselves with defiled hands.[26] This assertion is confirmed by several passages in *A Journal of the Plague Year* by Daniel Defoe. Other plague patients in London, "unable to contain themselves, vented their pain by incessant roarings, and such loud and lamentable cries were to be heard as we walked along the streets. . . ."[27]

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[22] *Observationes medicae circa morborum acutorum historiam et curationem,* sectio IV, cap. VII (p. 194 in the *Opera Omnia,* adapted by G. A. Greenhill from the edition of 1685, London: Sydenham Society, 1844).

[23] *Observ. med.,* sect. N, cap. III, *ed. cit.,* p. 174. Whenever he was compounding his tincture, Sydenham felt grateful towards the Lord Almighty, because of the consolation that He has left the human race in the form of opiates.

[24] *Observ. med., ed. cit.,* p. 175.

[25] G. W. Wedel, *Amoenitates materiae medicae* (Jena: 1684), Lib. II, sect. IV, cap. 14.

[26] J. C. Pezold, *Rationes et curationes dolorum in illustri leucorea* (Acad. thesis Wittenberg, 1696), thesis 30.

[27] Ed. Everyman's Library No. 289, (London and New York, 1966), p. 86. Defoe, who must have been born about 1661, could of course not have described the epidemic of 1665 from his own experience. It is evident, though, that he has based his work, which appeared in 1772, on an accurate investigation of written sources and on verbal information of eye witnesses.

In the seventeenth century more theses appeared that had pain for their subject, in fact quite a large number.[28] The authors of these learned treatises seldom acquired any subsequent reputaton on the grounds of further literary production. Therefore they can be considered as examplary of the "rank and file" doctor, who does his work quietly and inconspiciously, and it seems relevant to listen to representatives of this group of practitioners. Another example of a thesis about pain besides Pezold's, is that of Hake.[29] In the foreword, the author wrote that, in his own opinion, nothing makes illness more of a burden than does pain. It is from pain that the patients in the first place want to be relieved, even when they have other symptoms which, taken by themselves, are more serious. It was customary in those days to embellish academic theses with poetry written by a friend's hand. Some of the poems were in praise of the student, others concerned the subject. Thus Hake's thesis contains, as a contribution from one Georg Engelbrect, the touching distich:

Vix poterunt centum linguae numerare dolores
qui cruciant variis corpora nostra modis.[30]

These fine lines would have delighted Madame de Sévigné (1626-1696), whose lively spirit still speaks out of her preserved correspondence.[31] In her frank letters, she comments sharply and often wittyly on the small incidents of daily life in noble circles. Several times illness and medical treatment are commented upon. She had but little appreciation of doctors, calling them "les ignorants," or "les meilleurs ignorants" when they were prominent. Her keen observation could make a clear distinction between real pain and pose, and her description of the colic of one of her friends, who omitted nothing in her attempts to impress those present, is quite admirable:

Madame de Brissac avoit aujourd'hui la colique; elle étoit au lit, belle et coiffée à

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[28] Ch. Eloy quotes more than twenty of these in the bibliography of his article "Douleur" in *Dictionnaire encyclopédique des sciences médicales,* 1884, vol. 30, pp. 465-511. Eloy quips that the authors of such works ". . . exerçaient à l'envie leur plume plus souvent que leur sagacité." In the mostly metaphysical reflections on the phenomenon in the seventeenth and eighteenth centuries, a clear distinction was not yet made between physical pain and mental suffering.

[29] P. Hake, *De doloribus* (Acad. thesis Helmstadt, 1688).

[30] "A hundred tongues will hardly be able to enumerate the pains that torture our bodies in different ways."

[31] Madame de Sévigné, *Lettres,* texte établi et annoté par Gérard-Gailly, 3 vols. (Paris: Gallimard, Bibliothèque de la Pleiade, 1965).

> coiffer tout le monde; je voudrois que vous eussiez vu ce qu'elle faisoit de ses douleurs, et l'usage qu'elle faisoit de ses yeux, et des cris, et des bras, et des mains qui traînoient sur sa couverture, et les situations, et la compassion qu'elle vouloit qu'on eût. . . .[32]

About the piles from which Madame de Coulange suffered she could say, short but to the point, that they "la font mourir de douleur,"[33] and Madame de Nanges had "de ces sortes de coliques jusqu'à s'en évanouir."[34] Her own state of health often left much to be desired too. "Il me semble qu'il est impossible de sentir de plus vives douleurs," she once wrote to her daughter referring to her "grand mal" (biliary colic?), "je tâchois d'avoir de la patience, et je voulois mettre à profit une si bonne pénitence, mais malgré moi je criois souvent de toute ma force."[35] Even this educated woman, who probably more than most of her contemporaries could see the relativity of her own existence and who deliberately cultivated an attitude of resignation and penance, only partly succeeded in coming to an understanding with her ailment.

The pain of Anne, Viscountess of Conway, another very intelligent, even learned woman of the seventeenth century—she was skilled in the liberal arts as well as in literature—is described in detail by Thomas Willis in his comprehensive treatise on headache. From the time she was ten years old, this woman had suffered from a severe migraine that drove her, in search of a cure, through many European countries causing her to try the most diverse remedies and even to subject herself to the most unpleasant and risky treatment, until in the long run she became entirely invalided by "dolores atroces."[36]

The copperplate by Romeijn de Hooghe (1645-1708), representing a breast amputation, speaks for itself (Fig. 2). This illustration is not borrowed from a surgical textbook, but from the emblem-book of Frans van Hoogstraten, *Voorhof der Ziele* (*Forecourt of the Soul*), which appeared in Rotterdam in 1668. The picture is part of the emblem symbolizing Saint Paul's remark in the second epistle to Timothy: "And their word (that of the ungodly) will eat as doth a canker. . . ." And just as a cancerous tumour calls for a painful and radical surgical intervention, the removal of heresy is only feasible by drastic measures: a comparison that the Church Fathers liked to use. The patient in this

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[32] Letter of 21-5-1676, *ed. cit.*, II, p. 99.
[33] Letter of 30-9-1676, *ed. cit.*, II, p. 211.
[34] Letter of 22-9-1687, *ed. cit.*, III, p. 174.
[35] Letter of 15-3-1676, *ed. cit.*, II, p. 54.
[36] Thomas Willis, *Opera omnia* (Geneva: Sam. de Tournes, 1680), vol. II, pp. 169-170.

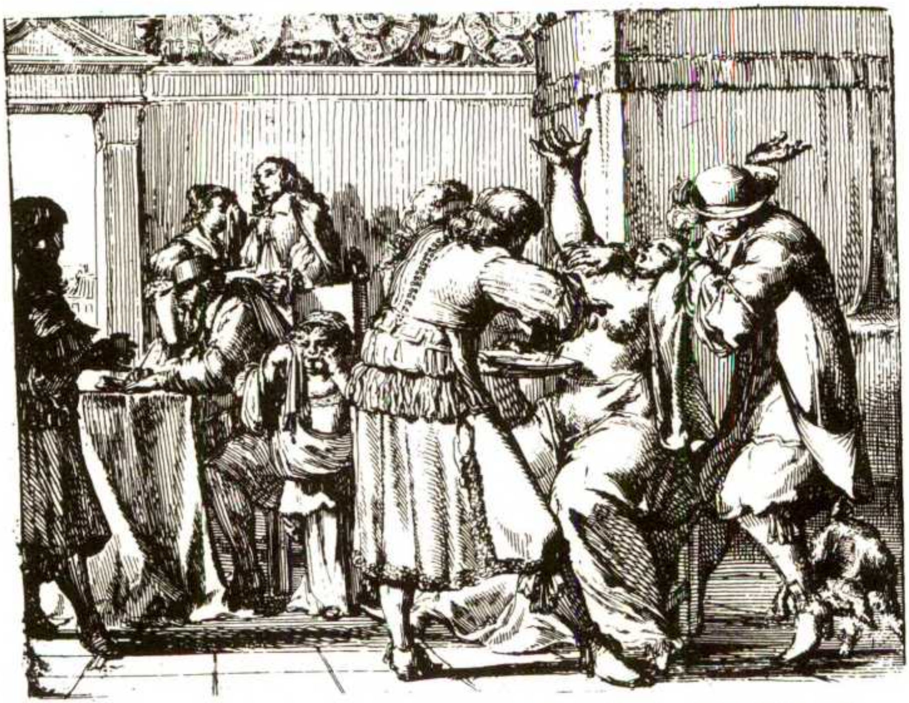

FIG. 2. "Breast amputation." Copper-plate by Romeyn de Hooghe (1645-1708). (From: Frans van Hoogstraten, *Voorhof der Ziele,* Rotterdam, 1668. Emblem 43.)

picture obviously undergoes her operation less resignedly than Madame de Bonnac would one hundred years later.

Breast amputation and colic, migraine, piles and plague sores, all these were apparently experienced as being extremely painful, even in the seventeenth century, and patients did not suffer in silence.[37] To bear pain without complaint one had apparently to possess an extraordinary moral strength in that century too.

This also appears to have been true for the previous century. On the tenth of May, 1521, during the defence of the border fortress of Pamplona against the French army, a young Spanish officer was hit by a cannon ball which shattered his right lower leg. Only after a prolonged convalescence in the castle of his ancestors in Loyola did young Ignatius recover from his injury, albeit with a bony protuberance under the knee. Since in those days he still dreamed of a worldly career, he had this disfiguring deformity removed by the surgeon. Just as in the previous convalescence, the operation was borne by the patient calmly and unswervingly. But when writing his memoirs some forty years later, he clearly recalls the operation and records, with some satisfaction, that he had not allowed himself to be tied,[38] nor had he uttered any sound, but merely clenched his hands into fists.[39] Towards the end of his life, the founder of the Society of Jesus was tortured for years by fierce abdominal pains, interpreted by the doctors as stomach cramps.[40] Although sickness and pain were borne by St. Ignatius with much patience and even with certain disdain, these attacks of stomach cramps forced him more than once to terminate a visit abroad.

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[37] Sometimes they did, though. The thirty-two-year-old Hermentie ten Boom, wife of the private soldier Hendrik Lewis, underwent on the 17th of December 1694, without complaint, a laparotomy which Professor Abraham Cyprianus from Franeker performed on her to remove a dead, fully developed foetus from the right oviduct. This woman had suffered before from such a violent pain, however, that she said she would bear anything with a firm desire to be delivered from the child as soon as possible. J. W. Napjus, "De Hoogleraren in de Geneeskunde aan de Hoogeschool . . . te Franeker (1585-1843) XIII: Abraham Cyprianus," *Nederl. Tijdsch. Geneesk.*, 1939, *83* II: 2625-2631.

A manifestly painful affliction, already known to Hippocrates, which appears to have tortured many historic figures, is gout. Emperor Charles V suffered from this disease to a severe degree, presumably as a result of his intemperate eating habits. Although he was from time to time entirely invalided by attacks of the disease, ". . . it must be remarked that within limits he bore them courageously." [41] Moreover, the emperor was sometimes plagued by haemorrhoids, which meant a real torment, as his aide-de-chambre and secretary, Willem van Male, recorded. Van

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[38] The tying up of patients prior to operation was not unusual from antiquity until the introduction of narcosis. In many illustrations from the Middle Ages as well as from later periods one sees the patient tied to a post or a shelf, or on a table. The Lindau surgeon Caspar Stromayr, who lived in the same period as the founder of the Jesuit-order, embellished his *Practica copiosa* (1559) with a great number of magnificent illustrations. These coloured drawings, apparently from life, were meant to elucidate the surgical treatment of inguinal hernia: the patient often lies tied on a shelf and his facial expression betrays his pain in more than one of the pictures!

[39] W. H. Ullman, "Obduziert wurde: Ignatius von Loyola," *Med. Welt* (*Stuttg.*), 1963, *35*: 1758-1763.

[40] At autopsy after his death in 1556 the stomach was found to be shrivelled. The liver was shrunken, hard and dry. Opinion differs as to the further findings. There is some talk of stones which Matteo Realdo Colombo, the famous anatomist who performed the embalmment, found in all kinds of organs. But it is not clear whether he found them in the body of Ignatius (*De re anatomica,* Venice: Nicolo Bevilacqua, 1559, Lib. XV: *De iis quae raro in anatome reperiuntur*). Ullman holds that of the described localisations only the one in the portal vein applies to Ignatius The clinical picture, though, suggests cholelithiasis.

[41] C. D. O'Malley, "Some episodes in the medical history of Emperor Charles V," *J. Hist. Med. & Allied Sci.,* 1958, *13*: 469-482.

Male himself was once troubled with such a bad nephritis that, as a result of the pain, he was unable either to lie down or to stand up.[42]

No less a person than Francis Bacon, in the *Advancement of Learning*, devotes a chapter to a critical assessment of the medicine of his time: ". . . a science which hath been . . . more professed than laboured, and yet more laboured than advanced. . . ." [43] The doctors do not come out of it too well, the followers of Paracelsus in particular, but no more do any others escape criticism. In this chapter a significant paragraph is spent on the management of pain, in which he states: ". . . I esteem it the office of a physician not only to restore health, but to mitigate pain and dolors; and not only when such mitigation may conduce to recovery, but when it may serve to make a fair and easy passage." [44]

The Middle Ages and classical antiquity must be examined with special attention, since in the opinion of Seifert the patients from these periods may for certain be compared with leukotomized people as far as their subjective pain experience is concerned. Even a superficial glance at the medical and non-medical literature of the Middle Ages proves the untenability of Seifert's thesis for at least this period. There was plenty of pain in the Middle Ages, but, as in our own times, the response of the patients differed markedly. Two examples: a family man of knightly rank, residing in Epfurt, was tortured by so strong a pain in the hips (*coxarum dolore torquebatur*), that because of his incessant screams he was hardly able to put into words the intensity of his internal pain.[45] Louis the German behaved more courageously after he fell from a broken balcony, fracturing a number of ribs so badly that the crepitation could be heard some distance away. But the monarch was so hardened and ambitious that he did not utter a groan or a sigh, a fact that was deemed curious enough to be recorded for posterity by the councillor Regio of Prüm in 870.[46] This contrast in behaviour confirms the assertion of Henri de Mondeville, surgeon to Philip the Fair: "Certains patients sont faibles, d'autres forts, les uns supportent gaillardement et vaillament ce qu'il faut supporter, les autres non"—an observation that has not lost its validity yet. Henri de Mondeville distinguished two kinds of pain: ". . . il y a une douleur tolérable, qui n'empêche pas le sommeil, ni la nourriture, et il y a une intolérable, parce qu'elle ne laisse le patient ni dormir, ni manger. . . ." [47] William the Conqueror must have suffered pain of the latter category after his horse fell, causing the royal belly to collide with the pommel: the king "for four weeks had rent the air with groans and torn the bed with writhings" before at last he expired." [48]

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[42] *Ibid.*, p. 479.

[43] F Bacon, *The Advancement of Learning*, 1605, Bk. II, ch. X (pp. 133 *et seq.* in the edition of W. A. Wright, Oxford: The Clarendon Press, 1873).

[44] *Ed. cit.*, p. 140. On the same page one may read that physicians made it a kind of "scruple and religion" to stay with the patient after the case was despaired of In this respect the doctors of Bacon's time distinguished themselves favourably from their colleagues of antiquity (Celsus, *De medicina*, V, 26) and the Middle Ages Bacon reproaches them, however, with having too passive an attitude at the death bed: they would do better to give all their attendances for the relief of the agonies of death. The opinion of Ackerknecht, that it was only under the influence of the Enlightenment that the notion dawned that even the incurable patient has a claim to medical assistance, needs amplifying in so far that in the eighteenth century the incurables were accorded the right to *active* medical help—as far as active help went in those days E. H. Ackerknecht, "Zur Geschichte der medizinischen Ethik," *Praxis*, 1964, *53*: 578-581.

[45] "Miracula sancti Ottoni babenbergiensis episcopi, auctore monacho S. Michaelis" (end of the 12th century?), *Monumenta Germaniae historica*, Scriptores XII, p. 918.

The mediaeval legislator acknowledged the power of pain by permitting the corpse of a suicide who had come to his desperate action "uyt pijne van siekten," [49] to be buried in consecrated ground, instead of being exhibited on a spike, which was, in those days, the usual punishment for suicides.[50]

The phenomenon of pain played a great part in securing a patient's history: the first question that the doctor must ask when arrived at the bed-side, was whether any pain was felt, next he should enquire whether the pain was strong and continuous and whether it was accompanied by shivering and by disturbed sleep.[51]

Pain appears to be of sufficient importance to Avicenna to have a separate chapter devoted to its management.[52] After a short explanation about the cause of the phenomenon in which he closely follows Galen,[53] he gives the warning that ". . . omnis quidem dolor qui sit fortior interficit." [54] Pain could, according to Avicenna, be controlled either by changing the composition of the body fluids, or by removing whatever disturbs their balance, or by bringing about analgesia. There follows an annotated enumeration of sedatives of increasing strength, with opium as the strongest "stupefaciens." It is curious that Avicenna, like Galen, seemed to prefer external use to administration by mouth.[55] Before any drugs were prescribed at all, the patient had to be examined to see whether the pain was caused by some external, removable cause. He knew that diversion, for instance, by pleasant, soothing songs or joy, could diminish the severity of the pain.[56] Avicenna was clearly a supporter of suitable, but careful treatment of pain. The opinion of this Mohammedan Persian does not seem to differ in this from that of the classical pagan authors or from the Christian surgeons of the later Middle Ages. Arabian textbooks reveal nothing, however, on the reaction of the patients in countries dominated by Islam. The story goes that Rhazes, the well-known clinician from Bagdad, contracted a cataract in his old age but refused to have an operation, because he considered the pain that attends such an intervention too high a price to pay for the short time that he expected to live.[57]

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[46] *Quellen zur karolingischen Reichsgeschichte,* ed. R. Rau, (Darmstadt: Wissenschaftliche Buchgesellschaft, 1969), vol. III, p. 230.

[47] *Chirurgie de Maître Henri de Mondeville,* French transl. by E. Nicaise (Paris: Félix Alcan, 1893), p. 160.

[48] F. T Gardner, "Bells toll for primes," *Surg Gynec. & Obstet.,* 1948, *86*: 377-380.

[49] (Driven) by the pain of his disease.

[50] Ph. Wielant (1440-1520), *Praktycke criminele,* ch. 87 Ed. A. Orts (Gent, 1872), p. 120. Quoted by H. H. Beek, *Waanzin in de Middeleeuwen* (Insanity in the Middle Ages) (Nijkerk: G F. Callenbach; Haarlem: De Toorts. 1969), p. 163

[51] This advice is offered in the famous treatise *Quomodo visitare debes infirmum* which appears in at least seven manuscripts from the tenth until the fifteenth century; it was edited more than once, among others by H. E. Sigerist in "Early mediaeval medical texts," *Bull. Hist. Med.* 1941, *10*, 27-40.

[52] "De sedatione doloris." Avicenna, *Liber canonis,* Lib. I, fen III, cap. XXX. The edition used by us is the Venetian one of 1507, in facsimile reprint. Hildesheim: Georg Olms, 1964.

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[53] Avicenna: "Iam scivisti dolorum causas in duabus comprehendi divisionibus, scilicet mutatione complexionis subito, et solutione continuitatis" Galen: "Caeterum alibi me frequenter dixisse memini, primas doloris species duas esse, repentinam temperamenti alterationem et continuitatis solutionem" *De loc. affect.* II, cap. 5, ed. Kühn VIII, p. 80 (Pain can be the result of sudden change in the composition of the body fluids, or the severance of a coherence).

[54] "Every pain that is too violent, is mortal" This is an observation that our generation, with the potent analgesics at its disposal, will not easily be able to confirm from personal experience. But as late as 1884 the *Dictionnaire encyclopédique des sciences médicales* states: "Une douleur excessive, fût-elle passagère, pourra provoquer . le collapsus, et même parfois la mort," vol. 30, p. 500. The mere fear of a painful operation can lead to a sudden death, in the experience of Benj. Travers (*An Enquiry concerning that Disturbed State of the Vital Functions usually denominated Constitutional Irritation,* London: Longman & Co., 1826, p. 22)

[55] In the list of substances that are suitable for external application, Avicenna mentions snow and cold water. Galen also made use of cold packs to alleviate pain, as is apparent from one of his comments on the aphorisms of Hippocrates (Ed. Kühn, XVII/2, p. 813). In 1646, as is generally known, Marco Aurelio Severino recommended a mixture of snow and ice for surgical anaesthesia

[56] St. Thomas Aquinas writes that the spiritual delight of meditating on divine matters can make one forget bodily pain, *Summa theologica* III, quaest. 15, art. 5. Not only theological contemplation can achieve this: Immanuel Kant forgot the pain of his gout by intensive study. And it is comman experience that pain in the nocturnal hours, when there is little or no distraction, is usually worse than during the day.

Avicenna himself reportedly met his end during an attack of colic. He had suffered from colic earlier in his life, but this last attack was so violent and exhausted him so much that he felt he could not endure it for long. Therefore he ordered discontinuation of all treatment and expired shortly thereafter.[58]

There is another aspect of medieval medicine which is important for our investigation, namely the alleviation of pain at surgical operations, which was attempted by a variety of means. The relative frequency with which surgical anaesthesia is discussed in medical as well as non-medical literature may be taken as indication that, in those centuries too, surgeons were aware of the severity of the pain caused by surgical intervention. The desire to anaesthetize the surgical patient in some way or other had existed in antiquity. Dioscorides [59] and Pliny [60] in the first century of our era described an extract of mandrake roots in wine, mandragora wine, which when taken would render the patient unconscious, making him insensitive to the cutting and burning of the surgeon.[61] Mandragora wine has found its way into the literature of the early Middle Ages, where we meet it, *e.g.*, in Latin translations of Dioscorides, and in the encyclopaedia of Isidorus Hispalensis (570-636).[62] Drinking cups, which in illustrations on folio 2 and 3 of Sloane codex 2839 (eleventh century) are depicted next to the heads of patients to be treated with the thermocautery, suggest that sedative or soporific potions were resorted to at least in he *early* Middle Ages. Non-medical authors in the period under review, amongst whom the monk Jocelin of Furness (twelfth century),[63] the scholastic Abélard (1079-1142),[64] and the chronicler Peter Quinel who gave an account of the church meeting in 1287(?) at Exeter,[65] also appear to have been acquainted with the practice of making a patient insensitive to surgical treatment. With Boccaccio, the surgical sleeping draught is introduced into secular literature,[66] where it continued to be encountered, probably long after its use had been abandoned by surgeons.

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[57] H. Schipperges, "Schmerzbekämpfung in der Arabischen Chirurgie durch schmerzhaften Applikationen," *Therap. Berichte,* 1963, *35*· 89-93.

[58] Avicenna, *Lib. canon.* In the edition of 1507, the un-numbered leaf following the title-page.

[59] Dioscorides, *Materia medica,* lib IV, cap. 75, in the Greek edition in three volumes of M. Wellmann (Berlin: Weidmann, 1906-1914), vol. II, pp. 235-237. In this tract the word anaesthesia is used for the first time in the medical meaning of insensitivity to pain (E T. Morch and R. H. Major, "Anaesthesia, early uses of this word." *Current Researches in Anaesthesia & Analgesia,* 1954, *33*: 64-68)

[60] C. Plinius Secundus, *Historiae naturalis libri XXXVII,* lib. 25, cap. 94. Ed. J. Harduin, Zweibrücken: *Studiis Societatis bipontinae,* vol. IV, 1784, pp 249-250.

[61] ". . . the phenomena repeated themselves with all faithfulness," reported Sir Benjamin Ward Richardson after experiments with a mandragora draught which he had prepared exactly according to Dioscorides (Anon , "The jubilee of anaesthesia." *Brit. Med. J ,* 1896, *5*: 1135-1142).

[62] "Cuius cortex vino mista ad bibendum datur, quorum corpus propter curam secandum est, ut soporati dolorem non sentiant" (of which the bark, mixed with wine, is given to drink to those, whose body for the sake of curing must be cut, so that they, profoundly asleep, will not feel the pain). Isidorus Hispalensis, *Etymologiae,* lib. XVII, cap. 9.

A second variety of surgical anaesthesia, which is mentioned repeatedly from the ninth century onwards, was the well-known soporific sponge.[67] The name of the prepared sponge—Ypnoticon—suggests that it is of Greek origin, but so far no mention of it has been found in the literature extant from antiquity. Unequivocal evidence of actual clinical application of the soporific sponge is nowhere to be discovered in the surviving medieval medical texts; [68] it is indeed hardly conceivable that satisfactory anaesthesia could be brought about by this method.[69] The fact that the procedure is not referred to in non-medical writings makes it even more improbable that it was ever applied on a wide scale.

From the fourteenth century onwards, there is an increasing amount of criticism of the use of pre-operative sleeping draughts by prominent surgeons: Guy de Chauliac,[70] Hans von Gersdorff,[71] Johannes de Vigo.[72] It is striking that in contrast to the fifth decade of the nineteenth century, when the necessity of anaesthesia as such was questioned or even denied by part of the profession, medieval surgeons only rejected the sleeping draught on the grounds of the great dangers to the patient attendant upon its use. According to Dr. Eloy,[73] the adverse response of many surgeons in the late Middle Ages and in the Renaissance is explained by the fact that witches and charlatans, as well as criminals before torture, frequently took drugs that effected loss of consciousness, by which it became less advisable for reputable practitioners to concern themselves with such pharmacons. Still, there were a few surgeons who continued to employ them. As late as 1652, proceedings were instituted against Nicholas Bailly, a surgeon in Troyes, by the college of physicians of that town, amongst other things because, in defiance of a still valid edict of 1372, promulgated by Charles V, he, being a surgeon, did not confine himself to the use of external remedies, but on his own account had also given his patients internal medication, to wit soporifics. In his defence the surgeon stated that "en grandes operations, amputations de membres, contre-ouvertures, cautérisations actuelles et potentielles, bien souvent les malades échappaient à son art, faute de dormir" and that therefore "il s'était étudié dans les secrets de la nature, et enfin avait trouvé un cordial ou essence merveilleuse qui endormait gracieusement les malades et apaisait la sensibilité à la douleur." Of what the "essence merveilleuse" consisted, history is silent.

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[63] [J. Pinkerton], *Ancient Lives of Scottish Saints.* Transl. from Latin by W M Metcalfe (Paisly: Alexander Gardner, 1895), p. 181.

[64] G. Lagneau, "De quelques anaesthésiques anciennement employés en chirurgie." *Bull. Acad. Méd.*, 2e série, 1885, *14*: 864-875.

[65] *Councils & Synods . . relating to the English Church,* edited by F. M. Powicke and C R. Cheney (Oxford: Clarendon Press, 1964), vol. I, part II, p. 1061 Quinel's report contains a remarkable quotation from the Greek church-father Origenes (185-231) in which it is stated that "When the doctors want to cut or burn someone, they give him some draught that makes them sleep deeply so that they do not feel . . ." I have not been able to trace the original text in the writings of Origenes.

[66] Giovanni Boccaccio, *Decamerone,* 10th novella of the 4th day.

[67] For details see among others D. de Moulin, *De Heelkunde in de vroege Middeleeuwen* [Surgery in the Early Middle Ages, with a Summary in English] (Leiden: E. J. Brill, 1964), pp. 121-123.

[68] The last picture but one on fol. 9^{v} of the Roger manuscript in B. M. Sloane 1977 from the thirteenth century, might represent a patient anaesthesized with the help of a soporific sponge. This would be the more remarkable because such a sponge is not mentioned in the text of the *Post mundi fabricam.*

[69] The experiments that Marguérite L. Baur has carried out on guinea pigs in the Therapeutic Laboratory of the University of Geneva with soporific sponges, which were accurately prepared according to a medieval prescription, gave entirely negative results: it did not appear possible to bring the experimental animals to a state of unconsciousness or even of dullness ("Recherches sur l'histoire de l'anaesthésie avant 1846" Suite. *Janus,* 1927, *31*: 170-182).

Francesco Petrarch, "the first humanist," produced between 1358 and 1366 a philosophic treatise, *De remediis utriusque fortunae,* which must have been quite popular, since between 1470 and 1650 it went to press not less than twenty times.[74] The work consists of two parts and is written in dialogue form in imitation of the *De consolatione philosophiae* by Boethius, of which the text here and there is also clearly reminiscent.

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[70] Guy de Chauliac, *La grande chirurgie,* 1363, Sixième traité, doct. I, ch. VII. In the edition of E. Nicaise (Paris: Félix Alcan, 1890), p. 436.

[71] Hans von Gersdorff, *Feldbuch der Wundarznei* (Strassburg: Joh. Schott, 1517), Tr. II, p. lxx⁴.

[72] Joh. de Vigo, *Practica in arte chirurgica copiosa* (Rome: Steph. Guilliretus & Hercules Bononiensis, 1514), lib. IV, tract. VII, cap. VII

[73] [Ch.] Eloy, "Un épisode de l'histoire de l'anesthésie chirurgicale au dix-septième siècle." *Gaz. hebdom. de méd. et de chir.,* 2e série, 1881, *18,* no 1: 1-9; no. 2: 2-12.

[74] F. N. M. Diekstra, *A Dialogue between Reason and Adversity* (Acad. thesis Nijmegen, 1968), p. 23, foot-note 4.

In the first part the relative value of prosperity is explained, in the second, for us the much more important part, it is taught that adversity and sorrow can be salutary. In this section, the problem of bodily pain is treated in three dialogues: one of these occupies as many as fourteen pages, being the longest section of the whole book,[75] which in itself is evidence of the degree to which bodily pain was considered a great evil. The discussions are held between *Dolor* and *Ratio*. The former permits not the least doubt how horrible—an "ingens malum" [76]—the pain of the body can be. *Ratio* does not deny it,[77] but wants to show how such misfortune can be changed for the good. Her words of consolation sound here and there like sophistry,[78] but essentially preach a philosophical resignation—an "ataraxy" one might say—which is reminiscent of the Stoics, Epicurus and Plotinus.

Whilst on these pages the cries and moans of suffering are interrupted by the cool, unaffected voice of reason, in one place only do we read of the answer offered by the physicians. They prescribe absolute rest in bed for *Dolor* because of pains of the lower leg, since standing and walking might cause the spirits and humors to concentrate in the painful limb. The local treatment consisted of fomentations.[79] *Reason* is not in the least impressed by this exclusively somatic management, the sole benefit that could possibly be derived from it being the opportunity it offers the patient confined to bed, to muse quietly on his present condition and to reflect upon death.

We have now passed through the Middle Ages without being able to decide that medieval man bore his pain in any way distinct from that of the patients today; in those ages too, the ill or injured man expressed his pain individually. The attention that medieval medical textbooks have given to the management of pain, as well as the interest which at the time was taken in the problem of surgical anaesthesia, does not make it plausible that pain *perception* was less acute than today.

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[75] Dialog. CXIIII: *De totius corporis dolore*. In the edition of *De remediis utriusque fortunae* of Joh. Le Preux (place of publication not named), 1595, pp. 609-623.

[76] "A terrible evil." *De totius corp. dol., ed. cit.*, p. 610. Dialog. CXIII, *De dolore iliaco, ed. cit.*, p. 604, deals entirely with abdominal pain.

[77] She is even willing to admit that: "Et ego dolorem rem acerbam scio immitem, horridam, amaram, tristem, naturae adversam, sensibus odiosam. . ." (I also know that pain is something unpleasant, cruel, horrible, bitter, sad, unnatural and offensive to the senses) *De totius corp. dol., ed. cit*, p. 612.

[78] "Sperare incipe, quandoquidem quod iam tristius metuas nil est. Sicut autem ad extrema gaudii pervenisse principium est doloris, sic extremitas doloris principium gaudii sit oportet Contrariorum haec est lex. Alterum ex alterius fine oritur" (Start to entertain hope, since what you now fear as something rather sad, does not mean anything. On the contrary, as having reached the ultimate joy means the beginning of sorrow, so the utmost pain necessarily means the commencement of joy. This is the law of contraries, the one develops out of the limit of the other). *De dol. iliac., ed. cit.*, p. 604.

[79] Dialog. XCV, *De aegritudine tibiarum, ed. cit*, p. 577.

So we will have to look further back into history and see how patients and physicians experienced pain in the first centuries of our era. One of the most important extant sources is the *De medicina libri octo* written by A. Cornelius Celsus, probably in the first century A.D. Since it is not certain whether Celsus was a physician himself, we do not know whether a member of the profession or an educated layman is speaking from the pages of this book. However that may be, in Celsus's opinion the surgeon—of all practitioners the one who as a matter of course has to inflict pain on the patient—should be "misericors" but he should not be moved by the patient's cries to go too fast, or to cut less than is necessary, proceeding just as if the cries of pain caused him no emotion.[80] Although Celsus clearly appreciates the painfulness of surgical intervention—the treatment of ozaena for instance is "aliquid tormenti" [81]—it does not appear from his book that pre-operative sedation was ever employed: he makes no more mention than does Galen of the mandragora wine described by Dioscorides and the younger Pliny.

Celsus was generally very reserved regarding the use of sedative drugs,[82] many of which he appears to have been acquainted with. This economy is somewhat astonishing—although we can share Celsus's fear of undesirable side effects—when we compare it with a pronouncement on pain which is quoted by St. Augustine from another, now vanished, work of the same author: "Est autem optimum in animo sapientia, est in corpore pessimum dolor." [83]

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[80] Aulus Cornelius Celsus, *De medicina,* lib. VII, prooemium 4. Lat. text with Eng. transl. by W. G. Spencer, 3 vols. Loeb Classical Library (London W. Heinemann; Cambridge, Mass.: Harvard University Press. 1948-53), vol III, p. 296.

[81] Celsus, lib. VII, *ed cit*, vol. 3, p. 366. Celsus advances as an objection to a certain eye operation, that frequent passing of a needle necessarily means a prolonged torture (lib. VII, *ed. cit.,* vol. III, p. 338). Even without having been touched by the knife of the surgeon, the patient can suffer from violent pain: "inflammations and pains in the ears sometimes even serve to drive the patient to madness and death" lib VI, *ed. cit*, vol. II, p. 229).

[82] Celsus, lib. V, 25, *ed. cit.,* vol. II, p. 59· "unless there is overwhelming necessity, it is improper to use them; for they are . . very active and alien to the stomach." The preference for external use is striking (*ed cit.,* vol II. p. viii), even of the juice of the poppy (*ed cit.,* vol. II, p xlvi). The "cataplasme laudanisé" appears to have still been in use in 1884 (*Dictionnaire encyclopédique des Sciences médicales,* XXX, p. 480).

[83] "Surely the best thing in the soul is wisdom, and the worst thing in the body is pain." Augustine, *Soliloquia,* lib. I, 21

For our investigation, data from the comprehensive work of Galen are better than those from the work of Celsus. It appears that the great Greek physician made a notable study of pain and was well aware of the significance that the phenomenon has for the patient and for the doctor. Elaborate are his descriptions of the different types in which it may be encountered.[84] He knew by experience that suffering patients often commit ill-considered actions, prompted by pain; [85] they may indeed lay violent hands upon themselves to escape from their suffering.[86] Galen insists that the doctor must take into consideration that fear of, or perhaps the very knowledge of what is going to happen, may influence the reaction of the patient. Accurate questioning and observation are not only necessary in order to learn the character, the violence and the localisation of the pain, but are also important in selecting the appropriate treatment. Galen greatly preferred a causal treatment to a symptomatic one: a swollen and painfully throbbing abcess, for instance, should be incised and that is that.[87] He recommends great care with the use of powerful narcotics such as opium.[88] He himself only prescribed such dangerous drugs in colics or other very violent pain. He condemns a too generous liberality as well as a narrow-mindedness on the part of the doctor: the principle of *mēden agan* should be adhered to here as in every other aspect of life. The fact that this Greek proverb is quoted in this connection makes it probably that in these pages we encounter not only the practical wisdom of the experienced physician, but also a decidedly philosophic principle, that of the Golden Mean, which from the time of the Seven Sages—the proverb is ascribed to Solon of Athens—via Socrates and Plato can be traced to Aristotle.

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[84] *Hippocratis epidemiarum II et Galeni in illum commentarius,* lib. II, cap. XI, Galen, *Opera omnia,* ed. C. G. Kühn, 19 v. (Lipsiae: Cnobloch, 1821-30. Cited hereafter as ed. Kühn), XVII/1, pp. 335-336 *De locis affectis,* lib. II, cap. V, ed. Kühn VIII, p. 81.

[85] *Methodi medendi libri XIV,* lib. XII, cap. I, ed. Kühn X, p. 814.

[86] *De symptomatum differentiis,* cap III, ed. Kühn VII, p. 57. The younger Pliny confirms this. one of his friends deliberately resigned his life because he could not bear the violent pain of his gout any longer (*Litterae,* lib. I, litt. XII)

[87] *Method. med.,* lib. XII, cap. I, ed. Kühn X, p. 813. Galen agrees altogether with Hippocrates that any such manual interventions should be carried out rapidly. *Hipp. de medici officina lib. et Gal commentarius,* lib II, ed. Kühn XVIII/2, p. 722.

[88] "Anodyns" are mentioned repeatedly not only in the *Method. med.* lib. XII, cap. I, but also for instance in *De simplicium medicamentorum temperamentis,* lib. V. cap. XIX (ed. Kühn XI, p. 764 *et seq.*), and again in *De compositione medicamentorum secundum locos.* It is interesting that in the latter work (lib. VII, cap V, ed Kühn XIII, p. 89) there occur "colicae confectiones," compounded from *semen hyoscyami, cicuta, succus papaveris* and *cortex radicis mandragorae,* simples which were also the most important ingredients of the medieval soporific sponge, see page 558 above.

A few data found in non-medical literature also throw some light on bodily pain experience during the first centuries of our era. Plutarch describes as a curious example of self-control how the consul Marius let himself be operated upon for varicose veins whilst standing up and without being bound, unflinchingly bearing "extraordinary pains." But when the surgeon had finished one leg, the consul preferred not to have his other leg operated upon as well.[89]

Material for our knowledge of daily medical practice in the early centuries is contained in the patristic literature. Particularly the writings of the Greek church fathers give a clear picture of how the lay public of those remote times looked upon medicine.[90] It appears, then, that therapy not only consisted of medicine or diet; sound and thorough cauterizing and cutting were part and parcel of it. The patients only suffered the latter benefit with lamentations, cries and screams, and some levelled curses at the surgeon; others raised cain even before they felt any pain, as if the clamour would please the Lord.[91] The sedulous surgeon, though, went on, quite unconcerned, his attention being directed to the healing of his patient [92] with whose welfare he was exclusively concerned.[93] The surgeon tried to limit the pain af the intervention as much as possible by the speed of operation [94] and the use of well-sharpened knives. After the operation he applied soothing drugs to the wound and tried, as much as was in his power, to relieve the pain caused by his actions with consoling words and lenitives; [95] and even days afterwards the careful surgeon deliberated on how he could lessen the pain.[96] Opium—for relief—and mandrake—as soporific—rendered good services here.[97]

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[89] Caius Marius Plutarchus, cap. VI, 3. *Plutarch's Lives,* with Eng. transl. by Bernadette Perrin. The Loeb Classical Library (London: W. Heinemann; Cambridge, Mass.: Harvard University Press, 1959), vol. IX, p. 476.

[90] H. J Frings, *Medizin und Arzt bei den griechischen Kirchenvätern bis Chrysostomos* (Acad. thesis Bonn, 1959).

[91] Makarios the Egyptian (died about the year 390), *Homiliae.* Migne, *Patrologia graeca,* XXXIV, 517 (cited hereafter as Migne, *P.G.*).

[92] Joh. Chrysostomos (approximately 344-407), *Enarratio in epistulam ad Hebraeos,* homilia XXX. Migne, *P.G.,* LXIII, 212. We met the surgeon who proceeds with his work impassively, deaf to the cries of the patient, already met in Celsus, see page 561 above.

[93] Joh. Chrysostomos, *Expositio in psalmum VI.* Migne, *P.G.,* LV, 72.

[94] Joh. Chrysostomos, *In illud, vidi Dominum sedentem. . . .* Migne, *P.G.,* LVI, 132

[95] Joh. Chrysostomos, *Enarratio,* homilia XXI and XXX, Migne, P.G., LXIII, 148 and 212.

[96] Joh. Chrysostomos, *In illud, Pater si possibile est. . . .* Migne, *P.G.,* LI, 31.

[97] Basileos of Caesarea (ca 330-379), *Homiliae IX in hexaemeron,* hom. V, 4. Migne, *P.G.,* XXIX, 101. There is hardly any mention in patristic literature of any sedation before or

The righteous doctor suffered together with his patients and carried his share of sorrow,[98] looking upon the suffering of the others as his own concern.[99] One patient might bear his pain with more resignation than another,[100] but one must practically be a saint to bear the violent pain of a surgical operation without complaint. One such saint was possibly Father Stephanus, a Lybian, who was attacked in old age by an ulcerating inflammation of the genitals. While the surgeon removed these parts, the patient calmly carried on a conversation with the bystanders, in the meantime plaiting a mat. When he noticed the astonishment of those present, he begged them not to take offence: "because nothing that God does, He ever does with evil intent, but always with salutary purpose." [101]

In contrast to this, Augustine recalls that in the year 387 he was tortured by a toothache which forced him to interrupt his studies for some time. He readily supported Celsus in his contention that pain is the worst thing that can befall the body. He considered "metus doloris" [102] to be one of the three things that might still disturb his peace

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during surgery. This needs not imply that classical madragora wine was no longer in use. Early Christian writers liked to describe the painful treatment that physically ill people at times must endure in order to be cured, comparing it with the suffering and pain which those who have gone astray from true religion have to undergo while being firmly guided back to the straight path. In such a comparison, the alleviating of pain *during* the procedure is, of course, not appropriate Origenes, for one, seems to have been acquainted with surgical anaesthesia (see note 65). In quite a different part of the then civilized world, Hilary of Poitiers (315-368) too, recorded that it is possible to amputate limbs painlessly with the help of a "medicatus potus" *De trinitate,* lib. X, 14. Migne, *Patrologia latina,* X, 352.

[98] Origenes, *Commentaria in psalmos.* Homil II in psal XXXVII. Migne, P.G., XII, 1386

[99] This passage, borrowed from the pseudo-hippocratic treatise *De flatibus,* occurs in the writings of several Greek fathers (Origenes, Eusebios) H. J Frings, "Aus fremden Leiden eigene Sorgen." *Sudhoffs Archiv,* 1959, *43.* 1-12

[100] Makarios the Egyptian, *Homiliae* Migne, *P.G.,* XXXIV, 517

[101] Palladios, (365?-430?) *Historia ad Lausum, continens vitas sanctorum patrum.* Caput XXI: Vita abbatis Stephani Migne, *P.G.,* XXXIV, 1089 Of course it is not impossible that the ablation of parts that were perhaps already gangrenous, was not so very painful at all.

[102] Augustine, *Soliloquia* I, par. 16 and par 21 In a *spurium* which in the past was sometimes ascribed to Origenes, there is a commentary on the book of Job in which it is described how a toothache, painful eyes or ears, can cause the sufferer to scratch the walls with his nails or even to injure himself (*Anon. in Job,* Migne P.G XVII, 513). It is striking that time and again, throughout history, *toothache* is quoted as an outstanding example of violent pain In Shakespeare Leonato says: "For there was never yet philosopher, that could endure the toothache patiently." (*Much Ado about Nothing,* act V, scene 1) Nearly fifteen centuries after Augustine, Heinrich Heine confessed that, should he ever have to make a choice between suffering from toothache or having a bad conscience, he would rather have

of mind. This confession is reminiscent of Seneca, who depicts "metus doloris," "dolor corporis" and "intermissio voluptatum" as three serious aspects of indisposition.[103] The Stoic Seneca, one of the distinguished representatives of the most influential philosophical system of those centuries during which philosophy was "the mental backbone of the Roman empire," [104] clearly did not reckon physical pain among the matters of indifference: "no man can suffer both severely and for a long time. Nature, who loves us most tenderly, has so constituted us as to make pain either endurable or short." [105] That there are, besides the intensity of the pain itself, still other factors which influence the individual, anthropological response to pain, was well known to Seneca: "young soldiers, although only slightly wounded, cry and fear the hand of the surgeons even more than the sword of the enemy," whereas "veterans, even if they are pierced through, patiently and without complaint endure that their body, as if it were someone else's, is cleaned from dirt." [106] Thus, in the first centuries of our era, pain appears once more to have been experienced in the same way and with the same intensity as in our own days.

So there is nothing for it but to go still further back, to consult the Hippocratic writings. Great prognostic significance was attributed to pain as a symptom of disease, as appears from many places in the book *Prognostic*. The author of this text even believed that pain, as such, could be fatal: "in fact, pains from the bladder alone are enough to cause death. . . ." [107] Although Hippocrates goes much less into detail

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the bad conscience. The German artist Wilhelm Busch has more than once taken toothache as a theme for a rhymed comic strip. See also Fig. 1 It is striking that many of the early instances of surgical anaesthesia were given for tooth extraction as far back as the forties of the last century.

[103] L. Annaeus Seneca, *Epistulae morales ad Lucillium,* with an Eng. transl by R. M. Gummere. Epistle LXXVIII, 6. The Loeb Classical Library. (London: W. Heinemann; New York· G. P. Putnam's Sons., 1920), vol II, p. 184.

[104] J. E. Erdmann, *Grundriss der Geschichte der Philosophie.* Revised by F. Clemens (Berlin-Zürich: Deutsch-Schweizerische Verlagsanstalt, 1930), p. 85.

[105] Seneca, *Epistulae morales.* Epistle LXXVIII, 7.

[106] L. Annaeus Seneca, "Ad Helviam matrem de consolatione III." Dialogorum lib. XII. *Opera quae supersunt,* ed. F. Haase (Leipzig: Teubner Verlag, 1852), vol. I, p. 239 "Sed quemadmodum tirones leviter saucii tamen vociferantur et manus medicorum magis quam ferrum horrent, at veterani, quamvis confossi, patienter ac sine gemitu velut aliena corpora exsaniari patiuntur. . . ."

[107] *Prognostic,* XIX. *Hippocrates.* With an Eng. transl. by W. H. S. Jones. The Loeb Classical Library (Cambridge, Mass , Harvard University Press, London: W. Heinemann, 1952), vol. II, p. 41.

than Galen, he distinguishes "intense pains that cannot be controlled" [108] from "pains, which a proper treatment can allay." [109] In the majority of his well-known sixteen case histories,[110] it is recorded that the patients feel several types of pain which often disturbed their night's rest and could even be accompanied by delirium. The management, recommended Hippocrates, consists in the application of several kinds of fomentations to the painful site, the prescription of a diet or purgatives, and occasionally in the performance of a venesection. As an internal medicament he sometimes prescribed the juice of the red lettuce, taken in water.[111] Hippocrates seems to have made but little use of opium,[112] although the juice of the poppy was familiar to him as a soporific.[113]

Although Hippocrates occasionally talks about severe and intense pain, very little can be concluded from the *Corpus Hippocraticum* about the subjective pain experience of the patient, and the attitude of the physician in relation to it. From the fact that the juice of the red lettuce, which has only a mild soporific effect, is said to be able to control "every pain," one might be tempted to conclude that the pain sensations of Hippocrates' patients were less developed than our own. Have we now, at last, encountered the man whose pain sensations are so indifferent that, as Seifert suggests, he may be compared to modern patients who have undergone a prefrontal leukotomy? Unfortunately, this supposition does not find support in the evidence that we have been able to collect from non-medical literature. The philosopher Epicurus (340-270 B.C.) records that he "had to endure excruciating agony from a diseased bladder and from an ulcerated stomach." [114] Alexander the Great uttered a loud cry, as though he had been struck by a lance, when he was attacked by the sickness that would finally drag him to his grave in

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[108] *Regimen in Acute Diseases,* Appendix, par. 23, ed. Littré, vol. II, p. 509

[109] *On Ailments,* I, par. 5. ed. Littré, vol. VI, p. 149.

[110] *Epidemics,* III, ed. Jones, vol. I, pp. 256-286.

[111] *On Women's Diseases,* I, prescription 101, ed. Littré, 1853, vol. VIII, p. 225.

[112] The doping properties of opium were probably recognized a very long time ago, the drug seems to have played a part in several ancient cults. It is not known at what time the drug was introduced into medical practice. It appears from the *Corpus hippocraticum* that this was already the case in the fourth century B.C. For details see P. G. Kriticos and S. N. Papadaki, "The history of the poppy and of opium and their expansion in antiquity in the eastern Mediterranean area." *Bull. on Narcotics,* 1967, *19* (3): 17-38, and (4): 5-10.

[113] *On Women's Diseases,* III, par. 201, ed. Littré VIII, 1853, p. 387. Hippocrates frequently prescribed the juice or seeds of poppy, solely or in combination with other pharmakons, for gynaecological disorders in particular, although not as a sedative

[114] Seneca, *Epistulae morales.* Epistle LXVI, 47, *ed. cit.,* p. 30.

323 B.C.: the pain in his epigastrium was so violent that the monarch called for a dagger to do away with himself, and merely touching the painful site produced in him the sensation of being wounded.[115] Herodotus, the historian (484-425 B.C.), reports the violent pain that the Persian king Darius (ca. 550-486 B.C.) had to bear for seven days and nights as a result of an injury to his ankle.[116]

Now the time in which Hippocrates lived was one of great cultural prosperity and it is claimed that the process of civilisation is accompanied by an "intensified capacity to suffer." [117] Would this also apply to the time of the Mycenaean civilisation of which the Homeric epics are reminiscent? [118]

On the basis of one hundred and forty-seven injuries recorded in the *Iliad,* we can form a rough idea of the way in which the warriors before the walls of Troy bore the pain of their wounds. It then appears—when the wounds did not prove to be immediately fatal—that many times their painfulness is explicitly described. Agamemnon, for instance, had to withdraw from the battle because of an injury to the elbow, on account of which he was consumed by excruciating pains "as when a woman in childbirth is tortured by the sharp, twinging arrows that are sent to her by the inducers of labour, the Eileithyiae, daughters of Hera and bearers of cutting labour pains." [119] Teucer, who was wounded in the wrist, was carried back to the hollow ships "groaning with pain." [120] Diomedes experienced a smarting pain passing through his body, when he extracted a sharp arrow from his foot.[121] Glaucus was terribly tortured by the wound that Teucer inflicted upon him with an arrow.[122] Several times mention is made of alleviating medicaments and "a physician is worth many other men," since he knows how to apply lenitives to wounds.[123] It was not only war injuries that led to violent pains: abandoned on the isle of Lemnos, poor Philoctetes suffered badly from a wound inflicted by a poisonous water-snake.[124] Not even the immortal gods appear to have been exempt from pain: Aphrodite, her graceful hand wounded by Diomedes, was beside herself and suffered greatly until Paean, the physician of the gods, put soothing medicaments on the wound and healed it; [125] and the formidable god of war himself, murderous Ares, insatiable in war, roared as loud as nine or ten thousand able-bodied men when he was hit in the stomach by a spear cast by Diomedes—who was sneakily supported by Pallas Athene—so that he too fled, lamenting, to Mount Olympus to lodge a complaint with father Zeus and to place himself under the treatment of Paean.[126]

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[115] D. Kerner, "Der Tod Alexanders des Grossen," *Aerztl. Praxis,* 1957, *9,* 47 and 48. Kerner believes that the young monarch suffered from yellow fever, a disease that typically starts with violent epigastric pain.

[116] Herodotus, *Historiae,* lib. III, cap. 129.

[117] S W. Mitchell, see page 541 above.

[118] The Trojan War took place about 1200 B.C., but the *Iliad* and the *Odyssey* probably took shape in the ninth or eighth century, although some parts might be older. In medical matters, which are only casually mentioned in both poems, it is therefore impossible to decide whether they reflect the medical practice of the ninth century or of an even more remote past. Probably there was not much difference between practice in both eras.

[119] *Iliad,* XI, 267. It should also be possible to examine the historical phenomenology of pain from the history of labour pains, which have been said, from the time of Aristotle onwards (*De historia animalium,* lib. 7, cap 9), to be a consequence of culture.

[120] *Iliad,* VIII, 334.

[121] *Iliad,* XI, 398.

[122] *Iliad,* XVI, 510.

With Homer we have almost imperceptibly crossed the border between history and mythology. Even in the prehistorical tradition which we call myth, and which according to Jung proceeds from the "suprapersonal or collective unconscious," [127] physical pain appears to be a significant factor: [128] our Asklepios is praised as a "gentle craftsman who drove pain from the limbs that he healed." [129] Asklepios' teacher, the wise centaur Chiron, "the first physician and the first to understand the use of herbs," suffered from a chronic wound inflicted upon him by Heracles.[130] Because he was not able to cure this wound himself, he was driven by pain to beg Zeus to release him from his immortality. The prayer was answered and Chiron's immortality was transmitted to Prometheus,[131] while he himself has adorned the nocturnal sky as a constellation—Sagittarius or Centaur—ever since. The Titan Prometheus, that sly ally of mankind against the gods, was liberated as a result of this from the chains, with which in punishment of his theft of fire, he lay fettered on the highest top of the Caucasus. Aeschylus makes him find pride in the fact that medicine, with its "soothing mixtures," was among the gifts that he had bestowed upon mankind.[132] So it appears that medicine is closely associated with pain even in the dawn of civilisation, indeed, medicine may have come into existence by the very phenomenon of bodily pain.

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[123] *Iliad,* XI, 515

[124] *Iliad,* II, 721.

[125] *Iliad,* V, 335-430.

[126] *Iliad,* V, 855-900.

[127] C. G. Jung, "The Role of the Unconscious," *Civilization in Transition.* Transl by R. F. C. Hull (London: Routledge & Kegan Paul, 1964), p. 10.

[128] It is of course quite probable that the ancient authors who have recorded the myths in writing have added elements which were derived from their own times.

[129] Pindar, Pythian Ode III, 5. *The Odes of Pindar.* With an Eng. transl. by Sir John Sandys, The Loeb Classical Library (London: W. Heinemann; New York: G. P. Putnam's Sons, 1924), p. 184.

[130] Heracles' arrows were no joking matter: even Hera had to suffer from unbearable sorrows on being struck by one of them in the right breast. Homer, *Iliad,* V, 394.

[131] Apollodorus, *Bibliotheca* II, 5, 4. Edited by R Hercheri (Berlin: Weidmann, 1874), p. 57.

When we look over our collected material we are inclined to formulate the cautious conclusion that, judging by the reaction of the patient and the attitude of the physician, there is no evidence that pain was once less acutely felt that in our own days.[133] Finding no arguments that the sensitivity of our nerve system has increased in recent times, we have to attribute the rapidly dwindling readiness to accept pain to a change in our mental attitude.

In an excellent article, Toellner recently discussed the importance that Cartesianism may have had in the present evaluation of physical pain. He points out that Descartes, by separating the body from the soul, reduced the body to a mechanical apparatus, a conception that eventually paved the way to the modern view that pain is *not* the inevitable and, in essence, unremediable result of the inherent imperfection of creation itself or of original sin, but a sign of a reparable derangement of a mechanism. This material derangement, useful perhaps for the maintenance of the body, is utterly senseless for the subject who has to experience it.[134]

Apart from this general truth which will be felt increasingly stranger according as man loses by and by his metaphysical and religious convictions, there exist striking differences in reaction patterns in various countries, ethnic groups as well as in the individual, probably defined to a great extent by training and tradition: in the Netherlands, for example, women in contrast to their sisters in the U.S.A. still generally deliver their babies without anaesthesia. Even in the same person the threshold of pain may vary from time to time, which shows that actualization of pain, although primarily dependent on concrete, anatomical conditions, cannot be ascribed to a purely anatomically defined level of the threshold. Pain is therefore a subjective way of being an experience as well as an evaluation of the actual situation, and as such the creation of a human being in his sense-giving existence.

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[132] Aeschylus, *Prometheus bound,* Dutch transl by P C. Boutens (Amsterdam: Wereldbibliotheek, no date), toneel VI, p. 27

[133] Let me point out once more, that this survey was limited to Western man It is obvious that in African or Asiatic cultures quite different patterns of reaction will be encountered, which may nevertheless raise similar questions.

[134] R. Toellner, "Die Umbewertung des Schmerzes im 17. Jahrhundert in ihren Voraussetzungen und Folgen." *Med. Hist. J.*, 1971, *6*: 36-44

Our assembled material also shows that the alleviation of pain was a primordial task of the doctor throughout history, a duty which he accomplished not only by providing judiciously the appropriate drugs, but also by offering his sympathy, by removing fear and conspiring confidence as much as he is able to. The powerful drugs at the modern doctor's disposal put him in a far better position than his predecessors to assist his patient, but he would fall short of his traditional task if he would think that analgesics and sleeping-tablets are all he is required to give his suffering patient.

Acknowledgment

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WILLIAM OSLER MEDAL ESSAY

CHANGING CONCEPTS OF THE NOSOLOGY OF GONORRHEA AND SYPHILIS

KENNETH M. FLEGEL

How have we come to regard gonorrhea and syphilis as separate, distinct diseases? This question is different from that asking who first described gonorrhea or where syphilis originated. Much has been assumed on the separation issue and historians frequently make passing reference to the problem. My intention is to make explicit what has been to the present only implicit. I propose to tell the story as it unfolded between 1490 and 1840, and to analyse the events.

The complete answer to such questions is frequently beyond historical resolution as the voluminous literature on the history of syphilis shows.[1] Whether syphilis was a world-wide disease prior to 1492 remains a conjectural matter. Medical and historical journals are replete with reports of diseases, likely to be syphilitic, in early Sumer, in ancient Egypt, in dynastic China, in biblical Israel, in classical Greece and Rome, in medieval England, and in primitive Africa. Although the well-worn discussion over the origin of syphilis persists, it seems certain that only after the epidemics of the 1490s and early 1500s did lues venerea reveal its many forms and wide-spread manifestations as one, unified, contagious disease. However, few contributors have denied that gonorrhea was a disease familiar to most of the ancient civilisations.

It is commonly held that John Hunter confused the two diseases with his famous experiment in 1767, and that not until 1838 did Philippe Ricord rescue the world from the aftermath. I will show how this explanation is too facile. The history of the venereal nosology cannot be so severely condensed.

How gonorrhea and syphilis were confounded is not well appreciated.

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[1] An outline of the history of syphilis is not intended. Several comprehensive works are available. In addition several helpful reference tools should be mentioned. E. Lancereaux, *A Treatise on Syphilis—Historical and Practical,* 2 vols (London: Sydenham Society, 1868-69); W. A. Pusey, *The History and Epidemiology of Syphilis* (Springfield: Charles C Thomas, 1933), H. Goodman, *Notable Contributors to the Knowledge of Syphilis* (New York: Froben Press, 1943). Gonorrhea has not been so thoroughly studied. Its chronicle is puzzling and intriguing because it was confused with syphilis.

Gonorrhea and soft chancre or chancroid (caries) were diseases familiar to pre-Renaissance Europe. The confusion between syphilis and gonorrhea arose only when syphilis became epidemic after 1493 with all its striking and wide-spread effects. The characteristics of the new epidemic were remarkable. Its nature was recognised early and its features described. I include the oft quoted account of Fracastoro in 1546 because it acquaints us with the knowledge of syphilis of that period:

> In some persons, the disease commenced without contagion; in others, and these were the greater number, it was transmitted by contagion. Not every kind of contact sufficed for producing it; it required that two bodies should become heated together, as occurs in the act of coition And it was chiefly by coition that the greatest number became infected. However, a considerable number of children contracted the disease by sucking their diseased mothers or nurses. The disease was not communicable at a distance; it did not show itself immediately, but sometimes at the end of one, two, or even four months; certain signs, however, announced already that the disease was in germ.
>
> Those affected were sad, weary and cast-down; they were pale; most of them had sores on the genital organs, ulcers similar to those which are wont to develop themselves on these organs after coition, and which are called caries, but of a very different nature; they were obstinate. When they were cured in one place, they appeared in another, and the treatment had to be recommenced. Afterwards, pustules arose on the skin, covered with a crust; in some they appeared upon the head, which was the most frequent place; in others they appeared elsewhere. At first they were small; afterwards they increased to the size of an acorn, which they resembled in shape, their appearance otherwise being similar to the crusta lactea of children. In some cases these pustules were small and dry In others they were large and moist; in some livid; in others whitish and rather pale; in others hard and reddish. They always broke in a few days, and constantly discharged an incredible quantity of stinking matter as soon as open; they were so many true phagedaenic ulcers, which destroyed not only the flesh, but even the bones. Those attacked in the upper parts of the body suffered from malignant affections, which eat away sometimes the palate, sometimes the fauces, sometimes the larynx, sometimes the tonsils; some lost the lips, others the nose, others all the genital organs. Many had gummy tumours on the limbs, which disfigured them, and were often the size of an egg, or of a small loaf; when they broke, a kind of white mucilaginous fluid flowed from them.[2]

Few clinicians could improve this description. Initially when lues venerea became epidemic in Europe in the 1490s, early writers distinguished it from gonorrhea and soft chancre because of the constitutional symptoms accompanying syphilis.[3] However, even before the time of Fracastoro's 1546 account, the frontier between the disease types began to blur. It appears that those studying venereal disease after 1550 considered gonorrhea a new "symptom," or, as we might say today, a new type of syphilis that arose spontaneously around 1540.[4]

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[2] Lancereaux, *Treatise on Syphilis,* vol. 1, pp. 24-26.

[3] I. Bloch, "The History of Syphilis," in D. Power and J. K. Murphy, ed., *A System of Syphilis* (London: Oxford University Press, 1908), vol. 1, p. 26; J. Le Petit, *Historique du*

Astruc informs us that James a Bethencourt in 1527, first described gonorrhea as a symptom of syphilis. Among the effects of syphilis:

> I have seen, saith this Author, a young man whose Penis had been perpetually vomiting a sanious virulent Humour, for a year and a half: Which was occasioned by his having layen with an infected Woman. His Member would be painfully stiff, and his complaint greatest at the time of his making water.[5]

However, in medical histories, Paracelsus is generally considered to have started the confusion by referring to syphilis as "French gonorrhoea" or "gonorrhoea gallicum" in 1530[6] or in 1540.[7] I have been unable to confirm whether this is true. Nevertheless it is well accepted that Paracelsus thought of lues venerea as a combination of the old venereal diseases and the sixteenth-century lepra, rather than as a new disease. Furthermore, Astruc shows us that "he takes notice of the Bubo and the Gonorrhoea, as symptoms then common to the Disease" [syphilis].[8] There is no doubt that Paracelsus united gonorrhea and syphilis. It is less certain that it was he who led his contemporaries into the same confusion.

Early French historians more commonly look to Anthonio Musa Brassavola as the originator of the unity school.[9] In his *De morbo gallico* of 1551, he considered gonorrhea as a symptom or local affection of the morbus gallicus. The same idea was echoed by a less known author in the same year, Benedetto Vettori (Benedictus Victorius Faventinus).[10] This view became generally accepted and by 1555,

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chancre mou (Paris Diss, 1913), p. 28; and W. A. Pusey, *History and Epidemiology of Syphilis*, p. 35.

[4] J. Astruc, *A Treatise of the Venereal Disease*, trans. W. Barrowby (London: 1737), vol. 2, p. 287.

[5] *Ibid.*, vol. 2, p. 267. Astruc tells us that James a Bethencourt's treatise on the venereal distemper is a humorous one.

[6] Bloch, "History of Syphilis," p. 26; Pusey, *History and Epidemiology of Syphilis*, p. 35.

[7] H. Goodman, *Notable Contributors to the Knowledge of Syphilis*, p. 38; L. T Morton, *A Medical Bibliography*, 3rd ed. (London: Andre Deutsch, 1970), p. 280

[8] Astruc, *Treatise on the Venereal Disease*, vol. 2, p. 286.

[9] *Ibid*, vol. 1, p. 110; vol. 2, p. 305; J. F. Hernandez, *Essai analytique sur la non-identité des virus gonorrhoique et syphilitique* (Toulon, 1812), p. 19; J. Rollet, *Traité des maladies vénériennes* (Paris: Victor Mason et Fils, 1865), p. 202; Le petit, *Historique du chancre mou*, p. 29.

[10] Astruc, *Treatise on the Venereal Disease*, vol 2, p 307.

Fernel, the most eminent French physician of the Renaissance, supported the same opinion.[11] In his *De luis venereae curatione perfectissima* of 1579, Fernel argued that gonorrhea was a product of a syphilitic infestation in the following way. Syphilis entered by the urethral canal, reaching the blood of the vena cava and aorta. The constitutional affection then involved the spermatic vessels and kidneys, producing a gonorrhea which seemed to vomit the poison outside.[12] The last was a favorable sign, indicating riddance of the poison and recovery from the disease. In his lectures, published posthumously around 1560, Fallopio, the student of Brassavola, considered gonorrhea to be a new symptom of the French disease.[13] Fallopio repeats the semantic blunder attributed to Paracelsus by calling the new symptom Gonorea Gallica.[14]

That gonorrhea and syphilis were two manifestations of the same disease rapidly became dogma. In 1575 Ambroise Paré, who redeveloped the vaginal speculum, considered them to be the production of a latent venom, arising from the same distemper.[15] After 1600, it is current in medical literature that gonorrhea, originating about 1540, was a local symptom of a generalised syphilitic infestation.[16] A century later Thomas Sydenham wrote on gonorrhea and syphilis. Despite his reputation for meticulous observation and careful description,[17] his respected opinion differed little from the theories of pathology of the time. It serves to indicate how firmly entrenched the belief in unity had become:

> But it is an hundred years ago since it [French pox] shew'd it self first in the Species of a virulent *Gonorrhea,* and does so still, endeavouring to go off that way, and it has no other manifest Symptom, only in a very small Ulcer in the Privities, commonly call'd a Shanker, is the first Sign of the Disease; the Venom whereof,

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[11] *Ibid.,* vol. 1, p. 110; Hernandez, *Essai analytique,* p. 19.

[12] J. Fernel, *Le meilleur traitment du mal vénérien,* 1579, trans. L. Le Pileur (Paris, 1879), p. 51.

[13] Astruc, *Treatise on the Venereal Disease,* vol 1, p. 110; Gabriele Fallopio, *De morbo gallico liber absolutissimus,* a Petro Angelo Agatho Materate (Venetiis, apud Haeredes Melchioris Sessae, 1574); Hernandez, *Essai analytique,* p. 21.

[14] Fallopio, *De Morbo gallico,* p. 181.

[15] *Oeuvres complètes d'Ambroise Paré,* collated by J F. Malaigne (Paris: J. B. Baillière, 1840) vol. 2, book 16; Astruc, *Treatise on the Venereal Disease,* vol. 2, p. 341

[16] Astruc, *op. cit.,* vol. 2, p. 287.

[17] R. G. Latham, *The Works of Thomas Sydenham, M.D.,* trans. from the Latin edition of Dr. Greenhill (London. Sydenham Society, 1848), vol. 1, p. 13.

> because it cannot be carry'd off by a Gonorrhea, invades and infects the Blood presently.[18]

Soft chancre, gonorrhea and syphilis were considered as one. This conviction was a central tenet of venereology for three hundred years. How could maladies which had been previously well known become confounded with a striking, supposedly new, epidemic disease? Why merge these symptoms when the same students of the venereal distemper(s) agree that the instances when they occurred as two or three together were exceedingly rare?

As I have indicated, answers to this question have been speculative. The most likely reason proposed by earlier historians is the following:

> One thing only was wanting for the early observers—vis., a synthetic knowledge of the disease [syphilis]; and of the symptoms which belong to it, some were described separately in the chapter devoted to ulcers of the genital organs; others were confounded and classed with the usual diseases of the period; and especially with lepra. It required the great epidemic of the fifteenth century to show the connection between primary lesions and secondary and tertiary affections. . . .[19]

At a time when buboes, extra-genital cutaneous ulcers and loss of hair, teeth and nails were being newly included with the wide-spread syphilitic lesions,[20] it was natural that:

> . . . in a few years, all complaints of the genitals were indiscriminately confounded together, and supposed to arise from one and the same poison. There is, therefore, no room for surprise, that gonorrhea should be included in the number.[21]

Lastly, Temkin considers that the great part played by the Renaissance theory of humors in pathology, encouraged the doctrine that gonorrhea and syphilis were due to the same cause: "The venereal poison, having entered the body, was compelled towards its exterior. Fortunate those in whom it was discharged in the form of a gonorrhea, for they might escape the true lues!" [22]

But what of the delineation of the venereal distemper as distinct disease entities? [23] A long silence over the relation between gonorrhea and syphilis ensues after 1550, most physicians believing that gonorrhea was a local symptom of syphilis. From Leyden in 1701 the voice of Hermann Boerhaave, describing gonorrhea and syphilis as separate affections,[24] was unheard and unheeded. Indeed, he has been credited with antedating Ricord by a century.[25] Perhaps he was responsible for inspiring renewed interest in the problem. However, the distinction is more incidental than emphatic in Boerhaave's works. It has been a conviction that persuaded none of the subsequent contributors to the separation of the two diseases. Even his contemporary, Astruc, ignores this feature, choosing to differ with Boerhaave over matters of treatment.[26] For these reasons, I consider the work of Boerhaave unimportant to the history as I have outlined it. Similarly, Astruc, an innovative obstetrician and a great historian of venereal disease,[27] made no advance over the knowledge of his time.

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[18] Thomas Sydenham, *The Whole Works of that Excellent Practical Physician, Dr. Thomas Sydenham,* trans. J. Pechey (London, 1717), p. 248.

[19] Lancereaux, *Treatise on Syphilis,* vol. 1, p. 60.

[20] Hernandez, *Essai analytique,* p. 20.

[21] R. Carmichael, *An Essay on the Venereal Diseases Which Have Been Confounded with Syphilis* (Philadelphia· T. Dobson, 1817), p. 96.

[22] O. Temkin, "Therapeutic trends and treatment of syphilis before 1900," *Bull. Hist. Med.,* 1955, *29*: 309-316.

[23] Here I do not wish to deal with the problem of diseases as separate entities. The fact

Nevertheless, from the frenzied discussion and investigation on the separation issue which ensued during the latter half of the eighteenth century, it is apparent that the unity theory was suffering from the first inklings of doubt. Little of the popular debate is extant but, as will be seen, the important published works address themselves to seemingly widely-held and roundly-contested public notions, particularly within the profession.

In the midst of the growing scepticism, Morgagni delivered the results of his brilliant dissections in 1761. Venereology, like all of medicine, entered a new era. Formerly, it was believed that syphilis always began as a local ulcer obtained through coitus with a diseased person. In cases when the ulcer was not found, it was presumed to be inside the urethra. Furthermore, this ulcer gave rise to local symptoms, a gonorrhea. Meticulous dissection convinced Morgagni that patients with gonorrhea rarely had a urethral ulcer.[28] John Hunter

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is that present-day physicians regard gonorrhea and syphilis as different diseases, most particularly because the causitive agents are different. However, physicians believed that they were different before the discovery of bacteria and my purpose here is to understand how this happened.

[24] H. Boerhaave, *Tractatio medico-practica de Lue Aphrodisiaca* (Venetiis, 1753), pp. 92-94.

[25] Goodman, *Notable Contributors to the Knowledge of Syphilis*, p. 63; Pusey, *History and Epidemiology of Syphilis*, p. 42.

[26] Astruc, *Treatise on the Venereal Disease*, vol. 2, p 461

[27] *Ibid.*, vol. 2 p. 287.

[28] J. B. Morgagni, *The Seats and Causes of Diseases Investigated by Anatomy*, trans. B. Alexander (London, 1769), vol. 2, Letter 44, pp. 592-615; Hernandez, *Essai analytique*, p. 42.

later emphasized in 1786 that it was in fact his elder brother, William, who first demonstrated the absence of ulcers in gonorrhea in 1749.[29] However his dissections do not equal the precision and extent of those of Morgagni. Later workers have been convinced largely by the latter.[30]

Francis Balfour's Edinburgh Doctor of Medicine dissertation in 1767 was acknowledged to be an explicit, printed declaration of the duality theory of gonorrhea and syphilis. His entire argument is extracted from earlier work and common observation. Much of it is based on the findings of Morgagni. He concludes that gonorrhea has a seat completely other than the prostate, seminal vesicles or Cowper's glands [31] and that the urethral ulcer theory is incorrect.[32] Not only are the forms of gonorrhea different, but also the cause or matter which gives rise to them is different. As telling proof, he adduces that no one who suffered from gonorrhea could propagate syphilis, that gonorrhea is not cured by mercury, that gonorrhea very rarely develops into lues nor the reverse, and that gonorrhea was not observed in Europe until thirty years after syphilis.[33]

To such bold claims, François Swediaur, writing from England, published a reply eight years later. Firmly believing that gonorrhea is but a local form of syphilis,[34] he is dismayed that: "It has of late been asserted, even by some physicians of eminence, that the poison which produces a gonorrhea is different from that which produces lues; and several specious arguments have been brought in favour of this paradoxical opinion." [35] To the claim that a gonorrhea rarely leads to syphilis, he replies that this is due to the restriction of the poison to the urethra, forming a barrier to the circulation. Furthermore, he has on occasion seen gonorrhea with little mucous flow, and hence excoriation of the urethra, leading finally to syphilis. In answer to the claim that gonorrhea does not produce chancre, nor chancre, gonorrhea, he answers that he has seen instances of the former and that no one of the medical profession would deny that the latter happens. To the observation that mercury is not needed to cure gonorrhea, while it is essential for treating syphilis, he argues that the poison in gonorrhea is restricted to the urethra and the constitutional use of mercury cannot be expected to reach the poison.[36] Such rebuttal probably represented, in some measure, the popular opinion of the day. Nevertheless, as the published and widely-read work of a physician of considerable repute, Swediaur's monograph represents a landmark in venereal disease history.

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[29] J. Hunter, *A Treatise on the Venereal Disease* (London, 1786), pp. 29-30. Probably the Hunter brothers' reputation for meticulous study and great success in nearly anything they attempted has led to the popular disappointment over John Hunter's studies on venereal disease. However I believe a careful examination of this treatise and comparison with contemporary works convinces one of the outstanding quality of John Hunter's work on venereal disease.

[30] Hernandez, *Essai analytique*, p. 42; P Ricord, *Traité pratique des maladies vénériennes* (Paris: D. J. Roubier et E. Le Bouvier, 1838), p. 121.

[31] F. Balfour, *Dissertatio Medica Inauguralis de Gonorrhoea Virulenta* (Edinburgh: Balfour, Auld and Smellie, 1767), p. 7.

[32] *Ibid.*, p. 14.

[33] *Ibid.*, pp. 17-18.

[34] F. X. Swediaur, *Practical Observations on the More Obstinate and Inveterate Venereal Complaints* (London: J. Johnson, 1784), p 2.

[35] *Ibid.*, p. 20. As mentioned earlier, Swediaur appears to be addressing himself to a rather popular dispute, little of which has been recorded

However, he also contributed greatly to the eventual separation of the two diseases.[37] During an attempt to distinguish between syphilitic gonorrhea and gonorrhea due to mechanical stimulus, he performed a terrible self-experiment whereby he injected a caustic solution into his navicular fossa. In so doing, he alerted his readers to the value of experiment in studying venereal disease.[38]

His experiment led him to devise a nosology of venereal diseases which incorporated the striking finding of Morgagni's dissections:

> The simple *syphilitic Blennorrhagia* ought further to be distinguished from the *complicated,* or that accompanied with an ulcer in the urethra; the latter admits no radical cure without the internal use of mercury, whereas the former seldom or never requires it.[39]

We perceive in our observations that which we preconceive. Swediaur already had adequate explanation for the same poison having two effects and the differing response to treatment was easily included therein.

In 1786 John Hunter presented his monumental treatise on the venereal disease. He has traditionally been given large blame for mixing, confusing and unifying gonorrhea and syphilis. That process was well completed before him.[40] From the earliest musings of Paracelsus, Brassavola and Fernel to the culminating work of François Swediaur the venereal distempers were considered as one.

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[36] *Ibid.,* pp. 20-25.

[37] For a consideration of the controversy which later developed between Swediaur and Hunter, and for a discussion of Swediaur's part in the description of gonococcal arthritis see M. Kelly, "Swediaur: the vicious anti-Hunter rheumato-venereologist," *Med. Hist.,* 1967, *11*: 170-174.

[38] Again, he was not the first to do so, but he was responsible for focusing attention on the technique and inspiring later contributors to this history to do the same In fact a Dr. Harrison had earlier introduced the pus of a chancre into the urethra with a sound, producing what he believed was a gonorrhea. See Hernandez, *Essai analytique,* p. 66, and P. Ricord, in F. J. Bumstead, ed., *A Treatise on the Venereal Disease by John Hunter, F.R.S., with Copious Additions, by Dr. Philip Ricord* (Philadelphia· Blanchard and Lea, 1853), p. 38

[39] Swediaur, *Practical Observations,* pp. 32-36.

Hunter's treatise begins with the admission that the arguments against unity are impressive:

> It has been supposed by many that the gonorrhoea and the chancre arise from two distinct poisons; and their opinion seems to have some foundation, when we consider only the different appearances of the two diseases, and the different methods of cure; which in judging of the nature of many diseases is too often all we have to go by. Yet if we take up this question upon other grounds, and also have recourse to experiments, the result of which we can absolutely depend upon, we shall find this notion to be erroneous.[41]

However, in trying to "prove" that the two diseases were the same and had the same cause, Hunter worked from premises that could hardly lead to any other conclusion. Whether or not these premises themselves were experimentally based is not clear, neither from the form nor content of his treatise.

Most important is his idea "of diseased actions being incompatible with each other." [42] No two actions can take place in the same constitution, nor in the same part at one time. That is, two fevers can not together afflict one individual, nor can the lues and smallpox be manifest in the same part at once. It should be noted that Hunter does not consider syphilis a constitutional illness, in the sense that a fever might be:

> The term constitutional is perhaps not strictly a proper term; for by constitutional disease strictly, I would understand that in which every part of the body is acting in one way, as in fevers of all kinds, either sympathetic or original; but the venereal poison appears to be only diffused through the circulating fluids, and as it were, to force certain parts of the body to assume the venereal action, which action is perfectly local, and takes place in different parts in regular succession of susceptibilities; there are but few parts therefore acting at the same time; and a person may be constitutionally affected in this way, and yet almost every function going on well.[43]

Thus, if he believed them to be distinct diseases, it would be consistent for gonorrhea and syphilis to occur in an individual at the same time, since neither are constitutional sicknesses for Hunter. However, the fact that lues venerea, in chancre form, and a gonorrhea can at one time afflict the generative organs conflicts directly with a duality theory.

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[40] See comment in n. 29 above.

[41] Hunter, *Treatise on the Venereal Disease,* p. 13.

[42] *Ibid.,* p. 2.

[43] *Ibid.,* p. 287.

Yet if gonorrhea and syphilis are the same disease, they must be caused by the same poison. Of poisons, Hunter believes they must be the same in all cases, have the same qualitative effects and produce the same disease.[44] Admitting that gonorrhea and chancre are the effects of the same poison then to what circumstances are two such different forms owing? [45] Why do they have a different time of onset? The answer rests in some parts being more susceptible to particular disease than others [46] and in the different mode of action of the parts affected when irritated:

> The gonorrhoea always proceeds from a secreting surface; and the chancre is formed on a non-secreting surface; and in this last the part to which the poison is applied must become a secreting surface before matter can be produced. All secreting surfaces in the body being probably similar, one mode of application only is necessary to produce the disease in them all, which is by the poisonous matter simply coming in contact with them. But to produce the chancre, the venereal matter may be applied in three different ways; the first and most certain is by a wound, into which it may be introduced; the second is by applying the matter to a surface with a cuticle, and the thinner that it is allows the matter to come more readily to the cutis; and the third is by applying the matter to a common sore already formed.[47]

Finally, why do they most commonly occur separately?:

> The poison then being the same in both cases, why do they not always happen together in the same person? for one would naturally suppose, that the gonorrhoea when it had taken place could not fail to become the cause of a chancre, and that this when it happened first would produce a gonorrhoea. Although it does not often happen so, yet it sometimes does, at least there is a great reason to believe so. I have seen cases where a gonorrhoea came on, and in a few days after in some, in others as many weeks, a chancre has appeared: and I have also seen cases where a chancre has come first, and in the course of it's cure a running and a pain in making water have succeeded. It may be supposed that the two diseases arose from the original infection, and only appeared at different times; and their not occurring oftener together would almost induce us to believe it was so, since the matter is the same in both, and therefore capable of producing either the one or the other.[48]

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[44] *Ibid.*, p. 12

[45] *Ibid.*, p. 16.

[46] *Ibid.*, pp. 4-6.

[47] *Ibid.*, p. 16.

[48] *Ibid*, p. 16. The logic underlying this passage would appear to be cyclic. This becomes clear when it is pushed somewhat, as in the following paraphrases: It is odd that the afflictions most often appear separately because one poison produces both afflictions. The poison produces one or the other and one can arise from the other at a later time. We do not see them together oftener because the poison is the same, so it can produce either one.

Girded with this defence, Hunter recorded the result of his unfortunate inoculation experiment. Using the urethral discharge of an executed criminal who was alleged to have gonorrhea, he introduced the venereal poison into the glans of the penis with a lancet. The generalised syphilis which ensued over the following three years left Hunter in no doubt as to the unity of gonorrhea and syphilis.[49] Supported by Hunter's excellent reputation as surgeon and investigator, the result of this experiment convinced his contemporaries that gonorrhea and syphilis were one disease.

Despite this error, John Hunter contributed to the separation of the venereal diseases, as much as he confused their cause. Ricord styled him ". . . the author who has had the most rational ideas of the diseases to which we are liable, and in whose works, truth is often seen to shine out of a number of errors, that were common to his age"[50] and that "at last, John Hunter came and laid the true foundation of the science of venereal affections."[51] From Ricord, this is high praise.

Rather than regarding gonorrhea as a local effect or symptom of syphilis, Hunter thought of them as two forms of the same disease. His treatise clearly separates the complications and long-term effects of gonorrhea from those of syphilis. So comprehensive was his description that it formed a standard text for decades. Ricord found it necessary to republish it with extensive commentary in 1853.[52]

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[49] Most historians consider that the executed man had both gonorrhea and syphilis and that Hunter performed a double inoculation on himself. See Hernandez, *Essai analytique,* p. 58, for a suggestion that Hunter did not in fact contract syphilis, or alternatively, he may have contracted it by other means! Also for Ricord's explanation that Hunter's inoculum was the pus from a urethral chancre in a syphilitic, see P. Ricord, *Lectures on Venereal and Other Diseases Arising from Sexual Intercourse,* trans. V. de Meric (Philadelphia: E. Barrington and G. D. Haswell, 1849), pp. 75-81. The possibility that Hunter's symptoms were not due, all or in part, to syphilis has aroused considerable discussion. A review of this possibility is available in I. J. Hunter, "Syphilis in the illness of John Hunter," *J. Hist. Med. & Allied Sci.,* 1953, *8*: 249-262.

However, it should be noted that because of the neutral form Hunter uses in referring to bodily parts, both in reference to his patients and to his experiments, it is not always possible to be certain of whose parts he is speaking Thus in Hunter's *Treatise on the Venereal Disease,* p. 324: "To ascertain several facts relative to the venereal disease, the following experiments *were made.*" Or again on p. 325: "Two punctures *were made on the* penis with a lancet dipped in venereal matter from a gonorrhoea; one puncture was on *the* glans, the other on *the* prepuce." And again on p. 326: *"The above case* is only uncommon in the mode of contracting the disease. . . ." Hence one would construe that Hunter's experiment was a self-inoculation, but it cannot be assumed with complete certitude (*italics* are mine).

[50] P. Ricord, *A Practical Treatise on Venereal Disease,* trans. H. P. Drummond (London: Longman, Brown, Green and Longmans, 1842), p. 13.

[51] Ricord, *Lectures on Venereal and Other Diseases,* p 15.

Hunter's accurate description of and insistence on the hardened, indurated nature of a truly luetic ulcer enabled later workers to distinguish chancroid or soft chancre from syphilitic chancre.[53] It also provided a fundamental starting point for later, more successful inoculation experiments. Finally the great emphasis Hunter placed on inoculation as an absolutely dependable experimental technique, provided the tool that eventually unravelled the venereal tangle.

From Edinburgh again, the voice of dissent arose in 1793. Benjamin Bell presented strong arguments for the duality theory,[54] in response to the position taken by Swediaur and Hunter.[55] For Bell it was a differentiating feature that lues venerea requires mercury for its cure while gonorrhea does not. He observed that the symptoms and consequences of the two diseases are perfectly different; that both have occurred in the same countries, remaining separate for a long time; that gonorrhea is a local disease, syphilis a disease of the constitution; that inoculation of pure gonorrhea or syphilis between persons rarely produces the other disease; [56] that they rarely occur together; and that even in severe cases of gonorrhea no lues ensues and no stage of pox has ever produced gonorrhea.[57]

Almost incidentally Bell relates the results of some inoculation experiments [58] witnessed by himself and a Dr. Duncan.[59] However it was to these results that later experimenters, Hernandez [60] and Ricord,[61] turned for confirmation of their own findings. Two medical students placed lint, soiled with the matter of a gonorrhea, between the prepuce and the glans. Slight sores were produced, but neither developed a chancre. The matter of chancres and buboes was introduced into the urethra, producing some pain and irritation, but no gonorrhea.[62] The

[52] Bumstead, ed., *Treatise on the Venereal Diseases by John Hunter.*

[53] Le Petit, *Historique du chancre mou,* pp. 40-42

[54] B. Bell, *A Treatise on Gonorrhoea Virulenta and Lues Venerea* (Edinburgh: J. Watson and G. Mudie, 1793).

[55] *Ibid.,* vol. 1, p. viii

[56] *Ibid.,* pp. 1-7.

[57] *Ibid.,* p. 32.

[58] *Ibid.,* p. 34.

[59] Carmichael, *Essay on the Venereal Diseases,* p. 95.

[60] Hernandez, *Essai analytique,* pp. 59-60.

[61] Ricord, *Traité pratique des maladies vénériennes,* p. 124.

[62] The account given by Ricord is somewhat different from that of Bell and Hernandez.

results appear to be at least as conclusive as those of Hunter. But for Bell, his case lay in argument and he gave little importance to experiment:

> . . . for, in order to avoid fallacy, and to give support to the opinion, these experiments would not only require to be conducted with accuracy, but to be numerous, and to be repeated on a variety of patients under every possible variety of circumstances; . . .[63]

The next event in this history of the venereal nosology comes from America. In his graduation thesis of 1806, James Tongue published additional inoculation experiments.[64] Approximately twelve inoculations of matter from a gonorrhea were performed: in the arm, into the glans and under the prepuce. No inflammation resulted. Inoculations of syphilitic matter into the arm were done; each resulted in a complete chancre within three to four days. He adduces these findings to be absolute proof that gonorrhea and syphilis are different diseases. In some measure his claim is justified, but it was not to his work that the nineteenth-century professional community would look for the same proof. Nevertheless his thesis did not escape the notice of Hernandez[65] or Ricord.[66]

In England the controversy raged over the next twenty years. John Abernethy, Hunter's student and successor, tried to incorporate the new discoveries into the Hunterian system. The result is a confused and conflicting treatment of the afflictions of the generative organs.[67] Jesse Foot, surgeon and Hunterian biographer, accepted the unity theory as propounded by his subject.[68] From Dublin, Carmichael was convinced by the work of Bell and argued in favor of separation.[69]

He is probably referring to the French translation, *Traité de la maladie vénérienne,* trans. Fr. Bosquillon (Paris, 1802) which is unavailable to me. Ricord states the first experiment, with the matter of a gonorrhea. was repeated three times by one student and that the experiment with the suppuration of a chancre placed in the urethra led to the development of chancre, bubo, and ulcer of the throat. requiring considerable mercury to cure.

[63] Bell, *Treatise on Gonorrhoea Virulenta and Lues Venerea,* vol. 1, p. 33

[64] J. Tongue, "Experimental proofs that the Lues Venerea, and Gonorrhoea, are two distinct forms of disease," in C. Caldwell, ed., *Medical Theses* (Philadelphia: T. and W. Bradford, 1806), pp. 390-393.

[65] Hernandez, *Essai analytique,* p. 64.

[66] Ricord, *Traité pratique des maladies vénériennes,* p. 113.

[67] J. Abernethy, "On Diseases Resembling Syphilis," in *Surgical Observations* (London: T. N. Longman and O. Rees, 1804)

[68] J. Foot, *A Complete Treatise on the Nature, Symptoms and Cure of Lues Venerea* (London: Highley and Sons, 1820), pp. 45-57.

[69] Carmichael, *Essay on the Venereal Disease,* pp 94-98

Meanwhile the work that brought the final solution to the nosology problem began in France. In 1810, because of all the dissention, the Société de Médecine de Besançon offered as a prize question: to determine by experiment and conclusive observation, whether or not the virus of gonorrhea and syphilis were identical. It received a powerful answer in the form of an analytical essay by Hernandez. In this work all the evidence for non-identity, historical, experiential and experimental, is recapitulated.[70] It is the culmination of successive summaries: Balfour 1767, Hales 1770, Ellis 1771, Tode 1774, Duncan 1777 and Bell 1793.

More important to this history and to Ricord [71] are some inoculations made upon convicts.[72] Hernandez believed that much of the confusion about syphilis was due to the presence of other diseases such as scurvy, scrofula and gout. Accordingly he repeatedly inoculated with the matter of a gonorrhea three healthy men, two scorbutics, four with scrofula, one with gout, one with haemorrhoids and six with sickly, irritable constitutions. None of the seventeen developed syphilis. Although other diseases gave rise to some appearance of syphilis and the scrofulous subjects benefited from the internal use of mercury, Hernandez guarantees that these men in custodial care for some time could not possibly have syphilis.

By 1812 the inoculation experiments had been repeated a number of times. If that of Hunter, the two related by Bell, the twelve done by Tongue, and the seventeen by Hernandez be considered the reliable work available to later investigators, then thirty-two inoculations of the matter of a gonorrhea produced only one case of syphilis (Hunter). In addition two instances of inoculation with the matter of a syphilitic chancre had been reported: one by the student told of by Bell [73] and the other, six inoculations, free of alteration, by Tongue, all resulting in chancre.[74]

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[70] Hernandez, *Essai analytique,* p. 19. Again the existence of such summaries should not confuse the purpose of this paper. Rather than write a compendium of all that has gone before, I have sought to eliminate the irrelevant or obscure and to dwell on the events which gave momentum to later developments. My intention is to analyse rather than compile.

Throughout this endeavor I have imagined myself to be listening to the reply to the question, posed to Ricord and his school, "What led you to surmise, and how were you convinced, that gonorrhea and syphilis are different diseases?"

For some estimate of the importance of Hernandez' summary to Ricord, see the latter's *Traité pratique des maladies vénériennes,* p. 109.

[71] *Ibid.*

[72] Hernandez, *Essai analytique,* pp. 61-64.

[73] See n. 58, above.

Midst this general mood of dogmatism and confusion, Philippe Ricord began his work on the venereal diseases about 1830. The success of his work emerged "by taking as its basis the experience of past times, by having recourse to experiments, and by bringing together well-authenticated facts." [75]

The experience of past times has been the subject of this history and a concerted effort has been made to present it in the manner in which it appeared to Ricord.

The bringing together of well-authenticated facts, in some measure, reflects Ricord's perception of that history.[76] However his vast clinical experience at l'Hôpital du Midi assured him of their validity and convinced him of some new ones. By re-introducing the *speculum uteri*, he was able to elaborate some principles of epidemiology.[77] Women, apparently suffering from a gonorrhea, could occasionally be shown to harbour chancres in the depth of the vagina or on the cervix. Hence they would be capable of transmitting either or both. Postmortem dissection in men frequently revealed the presence of urethral chancre. He never found that a chancre had been communicated by a discharge, without ulceration in the sexual organs of the person producing it.[78]

On the basis of his observations Ricord formulated postulates that he tested by inoculation: that a venereal affection already present did not prevent the contraction of others or that the number of successive infections possible was limitless; that primary symptoms occur in only one region so that primary symptoms in a second area must be due to contamination by pus from the first or by reinfection from another person; that secondary symptoms do not prevent the contraction of other primary ones; and that the frequency with which constitutional syphilis developed did not depend on the number of primary symptoms present.[79]

Finding the inoculation of healthy subjects repulsive and that of animals impossible, he determined to inoculate those afflicted, with their own disease. Twenty-five hundred inoculations were performed between 1831 and 1837. Inoculation of secretions of a gonorrhea in many localities including urethra, vagina and conjunctiva, never produced chancre or constitutional syphilis.[80] Inoculation of the pus of a primary chancre, regardless of locale, always gave chancre.[81] Uncertain results from earlier experimenters were attributed to their failure to recognise that only the pus of the primary chancre or ulcer gave constant results.[82]

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[74] In Caldwell, *Medical Theses,* pp. 390-393.

[75] Ricord, *Lectures on Venereal and Other Diseases,* p 20.

[76] Also, anyone who reads Ricord cannot fail to notice the alacrity with which he dismisses not-so-well-authenticated facts!

[77] Ricord, *Lectures on Venereal and Other Diseases,* p. 20; *idem, Traité pratique,* p. 118.

[78] Ricord, *Lectures on Venereal and Other Diseases,* pp 75-81

[79] Ricord, *Traité pratique,* p 79.

Armed with his experimental findings and fortified by his clinical experience, Ricord proclaimed to the world in 1838 that gonorrhea and syphilis were different diseases. To his credit he persisted in doing so for twenty years. The stories of his demonstrations on the wards to students, visiting physicians and patients are legion. Ricord must be credited with two achievements: finally differentiating gonorrhea and syphilis, and convincing the members of his profession of this difference.[83]

What was the process by which gonorrhea and syphilis came to be regarded as separate, distinct diseases? To what extent did conceptual, social and technical developments help or hinder the process? Finally, in addition to the limits of any historical endeavor, are there special factors impeding an exact answer?

Conceptual developments assisting the unfolding of this history must include the notion of contagion. It is contagion, as elaborated by Fracastoro and little advanced by the time of Ricord,[84] that contributes to this history. Contagion as "a certain precisely similar corruption which developes in the substance of a combination, passes from one thing to another, and is originally caused by infection of the imperceptible particles"[85] became the process of transmitting infection as putrescence or corruption between persons. By the middle of the sixteenth century gonorrhea and syphilis became prototypes of this kind of infection.[86]

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[80] Bloch, "History of Syphilis," p 34; Ricord, *Lettres sur la syphilis* (Paris: aux bureau de l'Union Médicale, 1851), p. 15.

[81] Ricord, *Traité pratique,* p. 91.

[82] *Ibid.,* p. 85. Later workers concur with Ricord and in part for the same reason, that only the primary chancre constantly contains infectious material. However, the finding is also explained by the development of a latent period due to primary immunity to syphilis. See H. J. Magnuson et al., "Inoculation syphilis in human volunteers," *Medicine,* 1936, *35*: 33-82.

[83] Ricord's accomplishments should not be confused with what remained to be understood. For example, he refused to attribute any specific, contagious nature to gonorrhea and considered damp, asparagus, and beer as predisposing causes! See Ricord, *Lectures on Venereal and Other Diseases,* pp. 18, 25, 29. He persisted in considering chancre a local disease prior to constitutional syphilis and before which local eradication might be possible (see Ricord, *Traité pratique,* p. 92). Effectively, this prevented him from recognizing soft or simple chancre.

[84] O. Temkin, "An Historical Analysis of the Concept of Infection," in G. Boaz et al., *Studies in Intellectual History* (New York: Greenwood Press, 1968), p. 134.

The concepts of humoral pathology provided a ready explanation for gonorrhea as a kind of running off of syphilis and so retarded their separation. Even Ricord, in refusing to admit a specific, infectious nature to the discharge of a gonorrhea, seems somewhat bound by these theories. It remained for Virchow, Pasteur and Neisser in the second half of the nineteenth century to dispel such outdated ideas.

Several social factors are important to this study. The social attitude toward venereal diseases as filthy and unclean, an attitude which remains prevalent today, retarded their study. The ingenuity of Ricord's experimental investigations needed to be matched by his persistence and his undying ability to be suspicious. However the growth of the large outpatient clinics at the Paris hospital quickened the process. They gave Ricord the population he needed to perform his inoculations.[87]

Technical factors also assisted the separation. Three centuries of careful clinical observation formed the foundation of later advance. The meticulous dissections of Morgagni provided a starting point for more accurate observation. The use of the *speculum uteri* was their clinical counterpart in women. The general resort to experiment by Swediaur led to more refined inoculation methods by Hunter, Bell, Tongue, Hernandez and Ricord.

Several special factors prevent a more exact answer to the question posed for this historical analysis. The persisting puzzle over the origin of syphilis leaves uncertainty, particularly concerning its appearance during the fifteenth and sixteenth centuries. It is difficult to assess the role in the separation process of a more modern finding, the asymptomatic carrier state.[88] Whether or not asymptomatic gonorrhea would be termed an "infection" today, it is an inconceivable state, in theories of infection prior to 1850. And finally, the possibility that either gonorrhea or syphilis has changed in form, virulence or mode of transmission over the centuries, changes even the form of the question.[89]

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[85] H. Fracastoro, *De contagione et contagiosis morbis et eorum curatione, libri III,* trans. W. C. Wright (New York: G. P. Putnam's Sons, 1930), p. 5.

[86] Temkin, "Historical Analysis of the Concept of Infection," pp. 129, 131.

[87] E. H. Ackerknecht, *Medicine at the Paris Hospital 1794-1848* (Baltimore: Johns Hopkins Press, 1967), p. 176.

[88] H. Pariser et al., "Diagnosis of gonorrhea in the asymptomatic female," *Southern Med. J.,* 1968, *61*: 505-506, and "Asymptomatic gonorrhea in the male, *Southern Med. J.,* 1964, *57*: 688-690.

Acknowledgment

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[89] For a recent review of evidence on the origin question and on the changing relation between the treponematoses, see C. J. Hackett, "On the origin of the human treponematoses," *Bull. World Health Org.*, 1963, *29*: 7-41.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

THE INSTITUTE OF THE HISTORY OF MEDICINE KATHOLIEKE UNIVERSITEIT, NIJMEGEN, THE NETHERLANDS

The Institute of the History of Medicine of the Katholieke Universiteit at Nijmegen started modestly three and a half years ago in a building which actually was nothing more than a renovated garage. On October 1, 1974, it moved into new quarters consisting of a suite of four rooms, a library room and a lecture room in a house which once was the seat of the estate where the University has been in the process of building since World War II. The main room of the Institute was formerly a hunting room; the walls of the lecture room are covered with 18th-century, printed wooden panels. The staff comprises a director in the rank of lecturer, a scientific collaborator, and a secretary; three students—two medical students and one student of history—are performing part-time duties. The budget allowed by the Medical School is quite generous and the library, though still a small one, is growing steadily.

The teaching program consists of a weekly lecture, alternating from students in their pre-clinical years to students in their clinical years. For the former group, a chronological survey of the development of medicine is given. For clinical students selected chapters on subjects of more particular interest to them are presented. Last year, for example, the introduction of clinical methods was discussed, as well as the relationship between hospital and laboratory. This year attention will be given to the history of a number of common diseases. History of medicine is not a required course at the University; student attendance, however, is satisfactory on the whole.

Invitations to give historical introductions to symposia and other meetings are rapidly increasing in number and publications appear regularly, mostly in Dutch journals. The Medical History Club of this University, modeled after the Johns Hopkins Medical History Club, meets regularly once a month. The meetings are attended not only by medical staff members and students but also by other members of the University community. The very good relations with other departments must be gratefully mentioned; the Institute sincerely tries to serve an intermediary function between the medical and non-medical departments.

DANIEL DE MOULIN

CARLOS MARTINEZ DURAN (1906-1974)

Carlos Martínez Durán, an honorary member of the American Association for the History of Medicine, died on May 8, 1974. Dr. Martínez Durán was born in Guatemala City and studied there at San Carlos University, graduating in 1931. After postgraduate research in Berlin he became professor of pathology at his alma mater in 1938. In 1941 he published a history of medicine in Guatemala, reprinted in 1945 and 1964, which is his best known work. He also published a biography of Chopin and several essays and monographs on colonial medicine and education. In 1955 he became professor of the history of medicine and retained this chair, as well as the one in pathology, until his retirement in 1966. Martínez Durán was one of the outstanding educators of Spanish America. He was twice elected Rector of Guatemala's University (1945-50 and 1958-62), was the founder of its University City, and finally became Minister of Education (1966-70), concentrating his efforts on extending primary education and eradicating illiteracy. A member of the International Academy and a vice-president of the International Society for the History of Medicine, he was honored by many governments and learned societies all over the world. During the last five years of his life, desperately ill with leukemia, he found the courage to attend medical meetings as usual, even abroad, including the International Congress of the History of Medicine in London in 1972.

FRANCISCO GUERRA

ANNOUNCEMENTS

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE

Annual Meeting, 1975

The Association's 50th anniversary meeting will be held in Philadelphia, April 30-May 3, with headquarters at the Warwick Hotel.

Research in Progress

James H. Cassedy announces that a new list of major active research topics in the history of medicine and related fields is in preparation and will be mailed to members around the end of January 1975.

NATIONAL NEWS

Boston

"Leaders in Boston Medicine," a series of films and discussions sponsored by Boston University School of Medicine, Benjamin Waterhouse Medical History Society, and the Boston Medical Library, will be held at the Countway Library. The program for 1974-75, under the chairmanship of G. Edmund Gifford, Jr., includes the following films and discussants: November 25, "Shields Warren" (John Z. Bowers and Shields Warren); February 24, "H. Baird Hastings" (John T. Edsall); April 14, "John F. Enders" (John F. Enders, Sidney Kibrick and Lawrence J. Kunz); April 21, "Paul D. White" (J. Worth Estes and Edward F. Bland).

University of Connecticut School of Medicine

The Second Transdisciplinary Symposium on Philosophy and Medicine, "Philosophical Dimensions of the Neuro-Medical Sciences," will convene May 15-17, 1975. Composed of six sessions, the Symposium will be held with the support of the Society for Health and Human Values. Program announcements and additional information may be obtained from Stuart F. Spicker, Department of Community Medicine, University of Connecticut School of Medicine, Farmington, Conn. 06032

University of Illinois

The 32nd annual D. J. Davis Lecture in Medical History will be given at the University of Illinois College of Medicine on April 23, 1975, by Herbert Ratner. The title of his lecture will be "Hippocrates and the Learned Profession of Medicine."

Request for Information

Professor James Monahan (Center for Educational Development, University of Illinois College of Medicine, 835 S. Wolcott Ave., Chicago, Ill. 60612) would appreciate receiving information regarding the conduct of Abraham Flexner's survey of medical schools in preparation of his 1910 report. In particular he would like to acquire copies of correspondence between Flexner and N. P. Colwell, Arthur Bevan, and the schools he visited, as well as other correspondence or papers expressing issues of the findings, conduct, or writing of the report.

PROGRAMS OF MEETINGS

College of Physicians of Philadelphia, Section on Medical History

November 25, 1974: "Marriage Counseling and the Related Professions," Emily H. Mudd.

George W. Corner History of Medicine Society, Rochester, N.Y.

October 22, 1974: "A Short Look at the Long History of Pediatrics," William L. Bradford.

November 22, 1974: "Hugh Owen Thomas, Progenitor of Orthopaedics," Leonard Peltier.

Handerson Medical History Society, Cleveland

November 11, 1974: "A Medical Student in the Weimar Republic," Bruno Gebhard.

Johns Hopkins Medical History Club, Baltimore

December 3, 1974: "Adolf Meyer's Wanderjahr: 1890," Eunice E. Winters.

Josiah C. Trent Society for the History of Medicine, Durham, N.C.

November 14, 1974: "Hospital and Patient in the Nineteenth Century," Charles Rosenberg.

Society of Medical History of Chicago

March 13, 1974: Morris Fishbein Lecture: "Tales and Triumphs of Early American Surgeons," Ronnie Beth Bush.

Newly elected officers are: Allen G. Debus, President; Hans Kienast, Vice-President; Walter Necker, Secretary-Treasurer.

University of Virginia Medical History Society, Charlottesville

October 29, 1974: "The Early Years of the University of Virginia Hospital"—"Historical Background," Wilhelm Moll; "An Analysis of Patients' Records," Todd L. Savitt.

November 19, 1974: "Robert E. Lee: The War and His Health," Harry J. Warthen.

Washington Society for the History of Medicine, Washington, D.C.

September 19, 1974: "'Help Yourself to Health': The Story of National Negro Health Week, 1915-1950," Marion Torchia; "Early 19th Century Rural Obstetrics," Dorothy I. Lansing.

November 14, 1974: "William H. Welch—A Comparative View," Peter H. Niebyl; "Hanged and Electrocuted: Joseph Henry's Experiments on the Murderer LeBlanc," Nathan Reingold.

William Snow Miller Medical History Seminar, University of Wisconsin, Madison

October 23, 1974: "Wisconsin General Hospital, 1924: An Oral and Pictorial

History," a 50th anniversary program moderated by Ronald Numbers, with Martin Albrecht, Ralph Hawley, Anna Knell, and Herman Shapiro as discussants.
November 21, 1974: "Women and Freud." Hannah S. Decker; panel discussion by Marjorie H. Klein, Joseph G. Kepecs, and Julia A. Sherman.

NEWS FROM ABROAD

London, England

On July 10, the Science Museum in London opened its first major Special Exhibition on the History of Medicine. It was timed to mark the bicentenary of Joseph Priestley's discovery of oxygen in 1774, and it shows how the results of this discovery and the discovery of other gases play a leading role in modern medicine. The exhibition will be on display for about six months.

Montreal

The Johns Hopkins University Alumni Association held on "Alumnight" on November 8, 1974, with Wilder G. Penfield as guest of honor. The speaker of the evening, Lloyd G. Stevenson, talked on "The Third Career."

Toronto Job Opportunity

The University of Toronto is inviting applications for the new Jason A. Hannah Chair for the History of Medical and Related Sciences. Applicants should have Ph.D. and/or M.D., established program in historical research, and be interested in teaching students of several faculties in an atmosphere of educational innovations. Reply by 7 April 1975 to Dr. Edward R. Llewellyn-Thomas, Associate Dean of Medicine, University of Toronto, Faculty of Medicine, King's Circle, Toronto, Ontario, Canada M5S 1A8.

BOOK REVIEWS

OWSEI TEMKIN. *Galenism: Rise and Decline of a Medical Philosophy.* Ithaca and London: Cornell University Press, 1973. xvii+240 pp. Ill. $15.00

Dr. Temkin has undertaken to expand and produce in book form the four Messenger Lectures which he gave at Cornell University in the fall of 1970. "Galenism" he interprets (pp. xi, 93) as "a medical philosophy, a set of more or less cogently connected principles, doctrines, and concepts, ascribed to Galen, used in thinking about man's body in health and disease, and shaping the physician's attitude to his profession and to human life." Temkin's concern is to delineate the changing silhouette of this medical philosophy in various periods. To present an effective history of the impact of Galen is a most exacting project. Temkin is himself fully aware of this as well as of the difficulties arising from lack of a critical Galen biography. Thus he does not claim to give "a full-fledged history of the subject."

In the introduction Temkin offers the essential biographical data on Galen. Thereby he brings to our attention the fundamental difficulty of any Galen interpretation, even if it deals only with the works that have been edited up till now (10 volumes, approximately 1000 pages each): the profusion of contradictions propounded in the various writings of the Corpus Galenicum. This is not the place to consider the interesting attempt made by L. G. Ballester in his book on Galen to overcome the difficulties by applying the concept of development. In view of the difficulties one can appreciate Temkin's decision to proceed pragmatically Thus, in his first chapter, "The Portrait of an Ideal," he presents some of the Galenic theories, methods and mentality, as a normative matrix, so to speak, in which the reaction of later centuries can be developed and explained. (Further support is provided by the statement of the distinguished Galen scholar, Karl Deichgräber, who emphasized Galen's striving, that he "constantly idealized himself, his life, his research and his medical life.")

The Galen image which Temkin develops in 40 pages is derived directly from sources and is the outcome of a life-long pursuit of them. The results of the older Galen research, and the intensive studies conducted in recent decades, including the broad fields of philosophy, classical philology, Byzantine and Arabic studies, and the history of science, are included in a masterful manner and are extensively discussed in the numerous footnotes. Future Galen research will certainly profit from the wealth of suggestions and stimuli offered by these footnotes.

From what has been said, it cannot be expected that Temkin's Galen portrait follows a conventional scheme. The strong points he sets forth never fall within the sphere of material description. This does not mean that this chapter is not full, even abundant, in subject matter. But the material presented—facts concerning Galen's anatomy, experimental physiology, pharmacology, his theory of elements, qualities and temperaments, as well as his humoral pathology—serves only on the one hand to enlighten the observational, on the other hand the rationalistic-speculative, portion of his epistemology. In all this Galen is presented as an eclectic as well as an enthusiastic seeker after truth whose discriminations in argument (p 20 ff.) remind us of Platonic dialogs. On the other hand they obviously provide an introduction for scholastic disputes.

Temkin regrets for good reason that so far we do not possess a thorough description of Galen's methodology. The more valuable is his demonstration of the similarity principle and the method of *diairesis* which is for Galen, according to the famous Platonic passage (*Phaedr.* 270 D), *the* Hippocratic method. Galen's religious attitude and his theological concept of nature are briefly characterized. Also well described are his idea of progress and his attitude towards tradition, whereby nuances of his self appreciation as successor to Hippocrates are perceived. Last, but not least, in this mission he leads a persistent struggle against the philosophical and medical sects, drawing around them a line of demarcation. Temkin aptly presents the characteristics of the Greek intellectual of the second century when he describes Galen's own self-disciplined conduct of life, his concept of virtue and his perception of responsible citizenship. These have contributed so much to the establishment of the ideal of the perfect physician which has had such a far-reaching force. Part of this concept is Galen's philanthropy. Temkin convincingly demonstrates (p. 50) that this goes beyond purely medical motives and belongs to a higher philosophical realm; it is based on the joy of the Greek intellectual in mental activity and in serving mankind.

Important for the impact of Galen, furthermore, are two basic attitudes which Temkin tries to elucidate: first, Galen's self image as physician-philosopher which goes so far that he distinguishes a second, educationally less pretentious type of physician, namely the practising physician. Far more difficult to explain is the problem of Galen's psychology with its implication of agnosticism and materialism or naturalism. dealt with by Lain-Entralgo and Ballester. While dealing with this kind of ambiguity the limits of Temkin's attempt to idealize become evident. He himself expressly points out (p. 44, n. 115) that the agnostic attitude is not constant in the various publications.

In the second chapter, "The Rise of Galenism as a Medical Philosophy," Temkin embarks on a description of Galen's influence upon the history of medicine. This and the following chapters abound in new aspects and new material. Temkin could easily have developed them into separate books.

First, the early evidences for Galen's fame in the second and third centuries are presented and identified. The period from Oribasius (d. 403), when one can first speak with certainty of Galenism in medicine, until the conquest of Alexandria by the Arabs (642) is of essential significance for the establishment of Galenism. In describing the influence of the Alexandrian iatrosophists and commentators, Temkin is once more in a field of research which he opened up more than forty years ago and to which he has given his attention again and again. Only for an expert with such intimate knowledge of the special literature is it possible to demonstrate impressively this permeation of medicine with philosophical hypotheses which was to be typical of the scholastic form of Galenism in the Middle Ages. Temkin compares the distinct description of the facies hippocratica in the *Prognosticon* with its explanation by the Alexandrian commentator Stephanos which abounds in physiological speculations.

To understand the attitude of Islam towards Galen it is important to know that Galenic medicine reached the East at the same time as Aristotelian philosophy. In the Islamic world the differentiation between physician/philosopher and medical practitioner led to a peculiar situation among physician/philosophers: as physicians they acknowledged the authority of Galen, as philosophers they clung to Aristotle. Many compromises and polemics enliven the colorful account which Temkin gives of the

Galenic prescriptions of Yūḥannā ibn Māsawaih, al-Fārābī, of Avicenna, Rhazes, Moses Maimonides and Averroes.

In the East the translations of Ḥunain ibn Isḥāq were a strong stimulus to the scientific world, while in the Latin West Galen's works reached only the Church Fathers. Using Nemesius of Emesa (4th c.) as an example, Temkin shows the ambiguity which on one hand helped to strengthen Galen's authority, but on the other, made him suspect to the Christian world. Galen's agnosticism was incompatible with the Christian belief in creation, in immortality of the soul or in a personal god of creation. It was equally incompatible with the corresponding doctrines of the Mosaic and Islamic religions. In this difficulty Galen's text, *Quod animi mores corporis temperamenta sequuntur*, with its somatic-materialistic attitude, plays a major role. Nevertheless, Galen was respected within the boundaries of the monotheistic religions as a great example of medical ethics and medical research in the world of the early Middle Ages.

Western Galenism can only be properly spoken of after the translating activities of Constantinus Africanus and his successors in Salerno and Montpellier became known in the 11th and 12th centuries. The unfolding of this medieval Galenism is traced in the third chapter entitled "Authority and Challenge." This kind of Galenism no longer produces immediate discussion of the original Galen, but receives its international significance through its break with the Byzantine and Arabic medium. For the expansion of Galenism in the European universities it was of importance that their medical faculties include in their curricula an anthology of medical texts, the *Articella,* which includes Galen's *Ars medica* (=*Ars parva*) and the *Isagoge* of Iohannicius. Using as an example the *Ars medica,* with its syncretism of the Hippocratic humoral theory, Aristotelian theory of qualities, and Galen's theory of temperaments, as well as the manner in which they are applied in diagnosis, Temkin shows how artificially fabricated this medieval system of medicine was. Only in a vague sense can it be considered Galenic. This is equally true of the *Isagoge* of Iohannicius. Since the Middle Ages the name Iohannicius has been accepted as a Latinization for Ḥunain ibn Isḥāq. It is another example of Temkin's thoroughness that he is not satisfied with this identification and gives good reasons for questioning it (p. 105, n. 28). Another example is offered (p. 109, n. 39) where Temkin, in describing scholastic methodology (*m. resolutiva et compositiva*) in a translation from Arabic of the *Ars medica,* goes directly back to the Arabic codices. Interesting for the area of experimental and quantitative concepts are Temkin's explanations of Galen's pharmacology and its further Arabic development (Jābir, al-Kindī) which, however, failed in their application in practice. Temkin exposes the theoretical self-sufficiency of scholastic medicine in the field of anatomy which, although post-mortem dissections were repeatedly performed, remained for more than two hundred years closely dependent on the authority of Galen and the Arabs. Temkin does not want to make medieval Galenism alone responsible for this concept of reality which deviates from our own notion. He draws attention to a possible critical attitude toward Galen which, in the weight of his judgment, was in principle applied in the spirit of Galen's own search for truth and was also exploited by Alexander of Tralles, by Rhazes, by an anonymous Byzantine physician-author of the 11th century, and by Maimonides. To the series of continuing individual criticisms of Galen belongs the discovery of the pulmonary circulation by Ibn an-Nāfīs in the 13th century, an example of much bearing for the future.

The end of the chapter has been elaborated in such a way that the complex dynamics, which on the one hand lead to the climax of Galenism and on the other hand initiate its decline, stand out clearly in their individual components: the philologist-physicians of humanism with their striving for a pure Galen and the editio princeps of 1525 in contraposition to the battle against blind Arabist adherents of Galen and also to the advent of Paracelsus. The result of this development, which is highly significant for the history of science, is elaborated very clearly by Temkin: the unity of medieval science encompassing Arabs, Christians and Jews, disintegrates. A strong caesura is expressed with the sentence (p. 133): "From here on, our concern will be with Western Galenism only."

Again and again it is the balancing of a masterful synopsis which impresses in this book. This is true for the evaluation of the humanistic and revolutionary elements in the work of Vesalius as well as for the weighing of the scientific achievements of entire epochs, as they are hinted at in the beginning of chapter 4, "Fall and Decline."

The decline of Galenism is represented as a long-lasting process in which, in the course of time, various factors had a share. Temkin characterizes these individually, using such examples as the gradual accumulation of pathologic-anatomical knowledge beginning with Benivieni (1507) up to Morgagni (1761), or the publication of the *Fabrica* of Vesalius, which exerted a rather sudden impact. In the 16th century polemics against Galen became stronger, which Temkin demonstrates in detail when dealing with Argenterius. The breaches in Galen's pneumatology and psychology created by Taurellus, Telesius and Campanella were important factors for the depreciation. One is inclined to agree with Temkin that around the middle of the 17th century, after Harvey's discovery (which is contrasted in a new, enlightened way with its Galenic context) Galenism as a credible scientific system was finished. The Galenic system was shaken at its roots by the revaluation of the subjective qualities into objective qualities—what Dijksterhuis called mechanization of the qualities—which set in with the exemplary temperature measurement of Sanctorius. Nevertheless it held its ground at the universities and in medical practice for a long time. Temkin drafts a very detailed picture of the battle of the Galenists against the representatives of the Nova Scientia in the 17th century. He calls to our attention that, in contrast to the crude mechanism of this century, much of the vitalistic, i.e. teleologic, view of the organism and the interpretation of nature of Aristotle and Galen entered into the vitalistic conceptions of the 18th century and the early 19th century. Hereupon he examines the picture of Galen given by the medical historians Le Clerc, Sprengel and Daremberg. By 1870, when Daremberg's *Histoire* was published, medicine had already created for itself a new theory of life under the influence of Darwinism and positivism. Temkin depicts the last phase, Galenism as merely a topic of historical contemplation, with an accuracy matching that he gives to the late classic, the medieval and the earlier modern stages. It shows the range of the master.

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JOHN NEUBAUER. *Bifocal Vision.* Novalis' Philosophy of Nature and Disease. Chapel Hill: University of North Carolina Press, 1971. 194 pp. $7.75.

At a time when certain groups in our society are calling for the humanization of science and medicine, it is interesting to look at a similar effort which took place in Germany around 1800. Labeled "romantic medicine," this development sought to organize and give meaning to the healing art with the help of philosophy, ethics, and aesthetics. In view of the rather limited scientific knowledge available then, such an interaction between science and the humanities proved deleterious for the former by blunting further empirical research and thus scientific progress.

The so-called "romantic" medicine has primarily received two distinct types of historical evaluation. For those belonging to the positivist historiography dealing with great physicians and great discoveries directly traceable to our present scientific achievement, such a period is merely an aberration, a fertile era of speculation barren of any genuine advances (Garrison). Other historians, on the contrary, have immersed themselves deeply into the philosophical and literary world of romanticism, and emerged from it without a proper perspective and the necessary lucidity to convey the "romantic" phenomenon in medicine to their readers (Leibbrand). It was Professor Ackerknecht's suggestion some years ago that the topic should be researched by scholars who were hopefully neutral on the ideological issue, neither true "romantics" nor "anti-romantics" grinding their respective axes. I believe that the book under discussion, although of limited scope, is written by such a non-partisan scholar

Professor Neubauer of the Department of German at Case Western Reserve has furnished us with an analysis of the poet and philosopher, Friedrich von Hardenberg (1772-1801), known by his literary pseudonym, "Novalis." An early adherent of Fichte's philosophy, Hardenberg turned to Schelling's "Naturphilosophie," which he partially espoused. In spite of his premature death, Hardenberg was one of the most distinguished figures of German romanticism.

The publication of a new critical edition of Novalis' writings has included a rearrangement of some notebooks—often referred to as "fragments"—some of them containing commentaries on the scientific theories and practices of his day. Novalis was not a systematic writer, and his sketches, excerpts, and cryptic notes are difficult to organize and interpret. Neubauer's stated aim has been to piece these commentaries together and describe the poet's philosophy of nature as it evolved from his extensive scientific and medical readings (only scattered and poorly edited portions of his notes became available in 1815). The author is careful to point out that Hardenberg's ideas had no palpable effect on the subsequent development of "romantic" medicine. Rather, these concepts should only be viewed as the expression of one German intellectual grappling with the contemporary issues in science and medicine. Throughout the book, Neubauer stresses Novalis' "bifocality," or tensions occurring between the various components of his thought: epistemology, aesthetics, natural philosophy, and poetry.

Neubauer begins his book with a short review of selected aspects of eighteenth-century science and medicine, including a short analysis of John Brown's system of medicine with its impact in Germany. He rightly depicts the transformation of Brownianism into Kantian a priori principles and later into postulates of Schelling's philosophy of nature.

This chapter is followed by an exposition of Hardenberg's ideas on medical theory, which reflected a strong anti-mechanical bias. Denying mechanical causality, the poet felt that the organism instead obeyed a central entelechy. The other theme pervading his notes was the search for universal, a priori principles, mirroring the effects of other contemporary philosophers and physicians. Neubauer then proceeds to describe Novalis' "magic idealism," his efforts to write an encyclopedia and the importance of one literary work, "Die Lehrlinge zu Sais," to an understanding of his epistemology.

Neubauer helps to debunk the idea that the so-called "romantic" period in German medicine was a totally negative phenomenon, solely evoking contempt and ridicule. As he mentions, the lively interaction between scientists, poets, and philosophers produced a complex cross-fertilization of religious, moral, artistic, and scientific questions still awaiting a more balanced historical interpretation.

Such an evaluation, however, presupposes a vast range of knowledge in a variety of fields, and may have to be subjected to a team approach. There are problems with the interpretation of terms and concepts used by the romantic authors, requiring a firm knowledge of German literature and the aid of linguists (one of Novalis' novels has recently been subjected to a computer analysis to prepare word indices). A thorough acquaintance with Kantian and the idealist philosophies has to be coupled with an understanding of biology and medicine. Further requisites would be a familiarity with the history of both science and medicine, and the capacity to summarize and translate highly abstract concepts into simpler English terms for better comprehension by more concretely thinking Anglo-Saxon readers.

Neubauer has valiantly met the challenge and given us a fairly good picture of Hardenberg's philosophy of nature. His lack of medical and historical knowledge somewhat diminishes his accomplishment. Moreover, Neubauer's writing remains far too Teutonic, with long sentences and paragraphs and the frequent inclusion of German terms and quotations which could have been shifted to footnotes. In spite of difficult reading, the book is a welcome addition to our meager secondary literature on "romantic" medicine.

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LEONARD G. WILSON. *Charles Lyell The Years to 1841: The Revolution in Geology.* New Haven and London: Yale University Press, 1972. xiii+553 pp. Ill. $17.50.

This weighty volume is far more than the first portion of another biography of a famous scientist about whom a number of books have been written. It describes the revolution in geologic thinking and an evolution which embraced not only geology, paleontology, or biology but scientific thinking in general and to some extent religion as well.

On the basis of very thorough research, the examination of a large number of letters, notes, diaries, and data of many kinds the author shows how a little boy grows up to be a student of the classics, then, forced by the prospect of earning a living, goes into law but without enthusiasm, and finally becomes one of the most outstanding scientists of his time and one of the most important geologists ever.

Lyell was never taught geology in classes and his training derived from field observation, study of fossils and samples, and contact with many of the important geologists of that time. The Geological Society of London and the exchange of ideas with its members had a great influence on Lyell's scientific career and he held several offices in the society and also became its president.

It is fascinating to see how an amateur naturalist became a geologist whose ideas changed the science profoundly and lastingly. He was a very keen observer and had the important ability to recognize the meaning and significance of what he observed. Geological observation is based on good judgment and understanding of what is observed. This ability is very difficult to develop and teach but Lyell acquired and mastered it in spite of an environment which gave no help but was rather hostile.

Many phenomena which are taken as long established facts and which are as natural to us as the existence of gravity were observed and interpreted by Lyell and have become standard knowledge. For instance the magmatic eruptions in volcanoes or the intrusion of granite into the earth's crust were then not recognized but basalt was thought to be a chemical precipitate in seawater by Abraham G. Werner at Freiberg in Saxony (not Frieburg!). The age of the earth was much disputed and Lyell recognized the immensity of time. Correlation of sedimentary layers by means of fossils is now routine but was introduced by Lyell. Not only might analogies be drawn between present and past conditions on the earth's surface, but present conditions can only be understood historically.

It is interesting to see that how the scientist grew up, matured, and found himself becoming well established in the scientific community over which he finally towered while his ideas grew at the same time without becoming rigid dogma.

Lyell summarized his thoughts in the *Principles* which appeared in several volumes and six editions. The growth and development of this major work is in itself stimulating. In reading the account by L. G. Wilson, the excitement of the scientific community is readily felt and makes reading an adventure.

Wilson's presentation is excellent and should be read not only by geologists but by scientists and all educated people in all fields. It is very healthy to stop and reflect where we came from and how science has come so far during the last 150 years. Far too few people have any perspective in depth and know where all the so-called knowledge originated, let alone who were their predecessors. This book provides an amazing amount of background in very readable form. It is well written. It also provides source material for students and teachers and makes it rather easy to appraise the present in terms of the past.

Geological principles are outlined in Lyell's original works and many other technical geology books but the growth of ideas, their origin and development, the growth of knowledge in terms of the "Zeitgeist" and the intellectual environment are better seen in this book which is written by a historian who understands the geological principles and facts he deals with.

As we are told this is only the first portion It may be hoped that we do not have to wait too long for the continuation.

Reviewed by Ernst Cloos, Ph.D., late Professor Emeritus of Earth and Planetary Sciences, The Johns Hopkins University, Baltimore, Md.

THE BULLETIN RECENTLY RECEIVED *

Leslie Caine Campbell. *Two Hundred Years of Pharmacy in Mississippi.* Jackson: University of Mississippi Press, 1974. xv + 207 pp. Ill. $10.00.

Edwin S. Gaustad (Editor). *The Rise of Adventism: Religion and Society in Mid-Nineteenth-Century America.* New York: Harper & Row, 1974. xx + 319 pp..

Georg Harig. *Bestimmung der Intensität im medizinischen System Galens.* Ein Beitrag zur theoretischen Pharmakologie, Nosologie und Therapie in der Galenischen Medizin. Berlin: Akademie Verlag, 1974 (Schriften zur Geschichte und Kultur der Antike, 11). 250 pp. DM 32.

Samuel Kahn. *Essays on Longevity.* New York: Philosophical Library, 1974. 198 pp.

Edward J. Kempf. *Selected Papers.* Edited by Dorothy Clarke Kempf and John C. Burnham. Bloomington-London: Indiana University Press, 1974. vi + 492 pp. Ill. $15.00.

Jutta Kollesch. *Untersuchungen zu den pseudogalenischen Definitiones medicae.* Berlin: Akademie Verlag, 1973 (Schriften zur Geschichte und Kultur, 7). 171 pp. DM 22.

Robert Olby. *The Path to the Double Helix.* Seattle: University of Washington Press, 1974. xxi + 510 pp. Ill. $23.50.

Marielene Putscher. *Pneuma, Spiritus, Geist.* Vorstellungen vom Lebensantrieb in ihren geschichtlichen Wandlungen. Wiesbaden: Franz Steiner Verlag GmbH, 1973. xi + 278 pp.

Georges Schüler. *Der Basler Irrenarzt Friedrich Brenner, 1809-1874.* Ein Beitrag zur Geschichte der Schweizer Psychiatrie sowie zur Sozial-, Religions- und Kulturgeschichte der Stadt Basel im 19. Jahrhundert. Aarau: Verlag Sauerländer, 1974 (Veröffentlichungen der Schweizerischen Gesellschaft für Geschichte der Medizin und der Naturwissenschaften, 27). 236 pp. Ill.

Nancy G. Siraisi. *Arts and Sciences at Padua.* The *Studium* of Padua before 1350 Toronto, 1973 (Pontifical Institute of Mediaeval Studies. Studies and Texts, 25). 199 pp. $10.50.

J. Snowman. *A Short History of Talmudic Medicine.* New York: Hermon Press, 1974. 94 pp. $5.75.

Gerald L. Swanson (Editor). *Medical Book Guide.* Boston: G. K. Hall, 1974. (Computext Book Guide Series). Issued monthly. $145 per year.

Carl August Wunderlich. *Wien und Paris.* Ein Beitrag zur Geschichte und Beurteilung der gegenwärtigen Heilkunde in Deutschland und Frankreich (1841). Hrsg. von H. M. Koelbing. Bern-Stuttgart-Wien: Hans Huber, 1974 (Hubers Klassiker der Medizin und der Naturwissenschaften, Bd 13). 159 pp. Ill Fr. 28/DM 25 (hardbound), Fr. 21/DM 19 (paperbound).

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

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